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BREAKING THE FRAMES

Anthropological Conundrums

Pamela J. Stewart
and
Andrew J. Strathern

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Pamela J. Stewart
Department of Anthropology
University of Pittsburgh
Pittsburgh, Pennsylvania, USA

Andrew J. Strathern
Department of Anthropology
University of Pittsburgh
Pittsburgh, Pennsylvania, USA

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Preface

After spending many decades in the field of social/cultural anthropology, working in many countries and with colleagues and students who have come from various intellectual arenas of thought, we have decided to use our experiential knowledge to look at the phenomenon of theorizing and the 'branding' of iconic trends in anthropology.

The history of anthropology shows a constant process of change in which previously held assumptions or frameworks of analysis have been broken by new developments of theory and practice. At the risk of some oversimplification, we can see these processes in terms of moments when serious turn-arounds of perspective moved anthropology in new directions. An obvious example would be the double shift early in the twentieth century from old-style synthetic anthropology based on records made by missionaries, explorers, and colonial officials to the emphasis on first-hand fieldwork and the study of synchronic functions of customs within structural contexts. Another would be the rejection of synchronic functionalism and the move to studies of process and meaning as these emerge historically; or shifts to structuralism and then post-structuralism; or the interpretive turn and the subsequent turn to cognition.

All of these changes have been taken up by their advocates as forms of rejection or replacement of earlier styles of analysis. The rhetoric of change, however, has itself concealed aspects of continuity and overlap in perspectives. In effect, changes have not always been as absolute as protagonists have claimed. New approaches have combined breaking some frameworks while perhaps unwittingly continuing others. Functional analyses continue, for example, in many guises long after the

supposed demise of structural-functionalism; and some kind of concern with the results of actions and their place in wider fields of actions is indispensable. Institutions and patterns of thought alike do take structural forms even if structuralism is repudiated. Processes occur even if we are now supposed to be in a post-processualist era. Finally, with all kinds of 'posts' such as post-socialist, post-colonial, or simply post-modern, it is clear that nothing is clearly 'post' anything else because past, present, and future are always co-implicated and co-present in consciousness, memory, and material culture. For instance, the house where we live in the USA was built in 1938, and every day while we are there we experience aspects of it that locate us partly in the life-world of that time.

In spite of the phenomenological reality of such a perspective, essentializing practices always seek to dichotomize life and so to reinstate frames which may then need later to be broken. The process of breaking and remaking frames is continuous, just as was pointed out by Thomas Kuhn (1962) for scientific paradigms in general. Breakthroughs of creativity occur when a particular frame is breached and a more rewarding perspective is revealed. Our major argument is that very often breaking a frame may simply involve mediating or modifying a false dichotomy on which the frame itself is based. There is also an institutional academic context in which all this happens and in consonance with our realist mode of argument we will include an exploration of this point, and proceed with others in the same critical but reconstructive vein of thought. The themes to be explored include the following, to be fitted into different segments of the work, sometimes briefly explored and at other times with a longer discussion provided:

1. Institutions: The history of anthropology exhibits conflicts between individuals and factions that result in schools or trends of theorizing. This is inevitable in a struggle for survival where resources are scarce and the competitions for them tend to be zero-sum, that is, winner takes all. However, there is an unfortunate set of results that emerge from this process: what begins as a bundle of innovative ideas ends up as dogma that stifles further innovation. We have witnessed this struggle in our own professional experience many times. The harm done to personal creativity is considerable. Sometimes students who do not conform are forced out or are not given support or are even aggressively denigrated. The same can happen with Faculty. Ageist assumptions are sometimes built in, so that Faculty with a different

viewpoint based on long and wide experience are driven out or marginalized. This kind of institutionalized boundary-making clearly shows the dangers of essentializing one trend or school of thought at the expense of others.

2. Individuals: The category of the individual is itself highly contested. One viewpoint is that the concept of the individual is historically specific and scarcely exists in what are known as socio-centric versus ego-centric societies or cultures. This dichotomy, as has often been pointed out but without enduring effect, is misleading and partial. It constitutes a frame that needs thoroughly to be broken in order to build up a more dynamic cross-cultural view of individuals in their contexts. Our concept of the relational-individual will be central to the argument here.
3. Nature and culture: A cluster of old dichotomies still informs much theoretical thinking around the topic of nature and culture (also biology versus society). The idea that nature and culture are polar opposites may have its roots back in ancient Greek philosophy, but it does not serve us well in trying to reach integrated understandings of human life processes. In practice nature and culture are closely interwoven. We will reflect here on the classic category of kinship in the light of this theoretical position, espousing neither sociobiological nor absolute cultural reductionism.
4. Retreat of the social: A recent set of studies on this theme bemoans the supposed decline of interest in the category of 'the social' or 'sociality' in favor of other approaches. We will examine some of the essays in this volume (ed. by Bruce Kapferer), recognizing their value while also resituating them in terms of our general arguments.
5. Religion: In the sphere of the analysis of religion and society new cognitivist approaches have reimported into the study of religion assumptions about rationality and reality, thus essentially opposing religion and science in a way that turns us back to nineteenth-century debates. Much cognitivist research, nevertheless, points to a greater understanding of how rituals and religious ideas work in practice. Pascal Boyer's leading idea of the 'naturalness' of religion points to one way of breaking again the misleading dichotomy of nature and culture, although he himself reimports certain universalizing propositions into his exposition.
6. Language: What is the relationship of language and culture and of both to universalist ideas of their cognitive basis? We will deal with

this theme in the context of a dichotomy between universalists and particularists, centering partly around ideas of Noam Chomsky and his followers and critics.

7. -Isms: We will reprise here in more detail the dichotomies in historical theorizing in anthropology mentioned in our beginning. We will break these down basically arguing against the reification of theoretical positions into dogmatic schools of thought.
8. Conclusion: Why dichotomies? The human mind seems to like these, but humans in action need to overcome them to retain creativity in their lives. We introduce here the idea of a mindful anthropology as a way of expressing our general stance in relation to both theory and practice in the discipline.

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http://www.stewartstrathern.pitt.edu/
http://www.pitt.edu/~strather/
http://www.pitt.edu/~strather/sandspublicat.htm

Our *Journal of Ritual Studies* (Stewart and Strathern, Editors in Chief), Facebook Page is:

https://www.facebook.com/ritualstudies

Large parts of this book were composed while staying in New Zealand as Visiting Fellows in the Department of Anthropology, University of Otago, Dunedin, New Zealand, in a part of 2015–2016. We thank the supportive staff members and our colleagues within the Department, especially Prof. Glenn Summerhayes and Prof. Richard Walter, for their assistance and conversations. We also thank everyone who helped us during our Study Abroad program, Pitt in the Pacific (University of

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The final pieces of this book were completed and the manuscript was made ready to send to the press while we were researching the June 2016 Referendum in the United Kingdom (UK) to exit from or stay in the European Union (EU). We have worked for decades in the Republic of Ireland, Northern Ireland, and Scotland, so it was especially moving to be staying in the three locations in June, July, and August of 2016, during and after the vote for the UK to exit the EU. We have been following the economic, political, and social impacts of the shifting shape and face of Europe today and what this means for the future. The surprise vote in favor of leaving the EU shocked many people and set a huge task for the 'Brexiteers', who had played a leading role in advocating this new pathway, and those political leaders who also had to step in and handle the unanticipated consequences of the vote. It was evident that the turn of events demanded a great deal of creativity of an imaginative and mindful sort for the future. A mindful anthropology would also be needed to study this situation in all its ramifying complexities.

Contents

About the Authors

Pamela J. Stewart (Strathern) and Andrew J. Strathern are a wife-and-husband research team who are based in the Department of Anthropology, University of Pittsburgh, and co-direct the Cromie Burn Research Unit. They are frequently invited international lecturers and have worked with numbers of museums to assist these organizations in documenting their collections from the Pacific. They have worked and lived in many parts of the world. Stewart and Strathern have published over 50 books and hundreds of articles, book chapters, and essays on their research in the Pacific (mainly Papua New Guinea and the South-West Pacific region, e.g., Samoa, Cook Islands, and Fiji); Asia (mainly Taiwan, and also including Mainland China and Japan); and Europe (primarily Scotland, Ireland, Germany, and the European Union countries in general); and also Norway, New Zealand, and Australia. Stewart and Strathern's current research includes the new subfield of Disaster Anthropology that they have been developing for many years. They are the Series Editors for the new *Palgrave Studies in Disaster Anthropology*. Also, the topics of Cosmological Landscapes; Ritual Studies; Political Peace-making; Comparative Anthropological Studies of Disasters and Climatic Change; Language, Culture and Cognitive Science; and Scottish and Irish Studies are ones they continue to investigate. They are on the Editorial boards of the journals *Shaman* and *Religion and Society* and they co-edit the *Journal of Ritual Studies*. They are the co-leaders of the University of Pittsburgh, Study Abroad program Pitt in the Pacific which they developed from their contacts in the Pacific, especially at the University of Otago in Dunedin, New Zealand.

Their webpages, listing publications and other scholarly activities, are http://www.pitt.edu/~strather/ and http://www.StewartStrathern.pitt.edu/.

CHAPTER 1

Framing History

Abstract Retrospective histories of anthropology often involve accounts of how one viewpoint supersedes another, as for example, in the transition between 'armchair anthropology' and the emphasis on 'fieldwork' in British social anthropology. Such accounts conceal the continuity among scholars such as Frazer, Malinowski, and Radcliffe-Brown. Nevertheless, real changes do occur, as when Malinowski injected a theory of performance into the study of magic instead of Frazer's rationalist scheme regarding magic as pseudoscience. I.C. Jarvie in turn criticized the work of the functionalists, using the case of cargo cults in the Pacific, and arguing that a new theory was needed to account for change, highlighting situational logic.

Keywords Armchair anthropology · Cargo cults · Fieldwork · I. C. Jarvie · Magic · Performance

In the history of anthropology, one of the standard transitions that has been invoked has been the supposed shift from 'armchair anthropology' to intensive fieldwork. 'Armchair' here carries the implication of distance from the materials and concomitantly a lack of engagement with the lives of the people studied. 'Fieldwork' implies the opposite: first-hand observation and participation, leading to more reliable and in-depth information.

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This was the prevalent founding myth of the development of a scientific basis for anthropology during the major part of the twentieth century, at least in the milieu of British social anthropology. A dominant figure in the discipline, Sir James George Frazer, was supposedly toppled from his place as an icon of scholarship in part as a result of this shift in perspective. It is interesting to reflect that Frazer was a long-term Fellow of Trinity College Cambridge, and A. R. Radcliffe-Brown, the individual who became linked to the opposing idea of 'fieldwork', also was a shorter term research fellow of the same college. Frazer was aware of Radcliffe-Brown's work, just as he was of the fieldwork of Bronislaw Malinowski of the London School of Economics. Rather than seeing his work as eclipsed by that of either of these two scholars, Frazer perhaps simply viewed their work as operating on a different scientific frontier, but one not incompatible with his own aims. True, it has for long been commonplace, and not without justification, to see the work of Frazer as superseded by that of the 'functionalist' school, as it came to be known. But such a frame of perspective, itself heralding the breaking of a frame and its replacement by another, leaves out a considerable area of overlap and indeterminacy.

First, as has often been pointed out (and recently by ourselves, Stewart and Strathern 2014), the synthesizers and generalizers, such as Frazer, depended absolutely on the field reports of investigators who had actually visited and spent time in out of the way corners of the world working as missionaries, government officers, and travelers. They would naturally, then, have embraced the quality of information brought in by investigators who identified themselves simply as anthropologists, once this label became available and legitimate. Besides, the traditions of fieldwork had already been set in Cambridge by the Torres Straits expeditions led by Haddon and Rivers.

Second, neither Malinowski nor Radcliffe-Brown was in fact content with simply writing the ethnography of a single group or area, even though their reputation was linked with their fieldwork, Malinowski with the Trobrianders of Papua, and Radcliffe-Brown with the Andaman Islanders (e.g., Malinowski 1922; Radcliffe-Brown 1922). They both, rather, sought to make generalizations, Malinowski basing his ideas on a hierarchy of needs that each society or culture had to meet, and Radcliffe-Brown on the rigorous comparison of cases, intended to lead to valid typologies of social structures and customary ideas, such as 'lineage' and 'taboo'. They did not abandon the aim of generalization as such. They simply developed different generalizations from those of Frazer, Tylor,

and others whose work stemmed from nineteenth-century evolutionary traditions of enquiry. Frazer sought generalizations about huge data sets on myths and rituals around the world that led him to identify themes such as 'the dying god', seasonal rituals, and the structuring of time, the significance of fire in general, and bonfires, scapegoats, and the like, themes often rooted in European folklore but resonating also with new ethnographic materials from Australia and the Pacific and with ancient texts from Greece and Rome. Radcliffe-Brown would also occasionally draw on materials of this kind, for example, to illustrate a point about the operation of taboos in society.

In examining Australian Aboriginal societies he was interested in establishing a model of the importance of patrilineal descent in the formation of local groups, and he made some personal field expeditions in pursuit of this theme while he was Professor of Anthropology at the University of Sydney in Australia. Malinowski displays his greater interest in 'culture' in his early work examining the issue of whether Australian Aborigines 'recognized' the role of physiological male procreation: an interest which would draw him somewhat closer to Frazer's work on customary notions than Radcliffe-Brown's concentration on 'structure'. A moment's further consideration, however, reveals to us that these two respective foci should have been seen as interrelated, if 'patriliny' and 'paternity' are to be in any way connected. Incidentally, scholarship has since moved on. Local groups in Australian Aboriginal society were not necessarily patrilineal (see, e.g., Hiatt 1996); and ideas of physiological procreation were also present, co-existing with other components of defining paternity in terms of locality and the landscape and the agency of spirit entities.

Finally, on this theme, Robert Ackerman's detailed study of *Frazer's Life and Work* (Ackerman 1987) makes it clear that Frazer and Malinowski stayed closely in touch over a long period of time and that Malinowski attributed to Frazer's writings some influence over his own thinking, for example, on the topic of magic and religion and their connection with economic life. This example indicates the complexity of relationships between practitioners in fields of enquiry. Malinowski's detailed fieldwork made possible a deeper understanding of magic as what we would now call performance, with a concentration on context and practice rather than simply on mental constructs of contiguity and resemblance (items once in contact remain in contact and like influences like).

When Frazer appealed to mental associations of this kind as the foundations of magical thinking, he was perhaps following a Cartesian-based

philosophy separating mind and body and privileging mind as the origins of action. Performance theory, by contrast, starts from the practice of magic, examining its emotional and embodied sources that result in performances characterized by actions of a mimetic kind reflecting both the power of words and the desires of the performer to create results by efficacious actions. It is quite obvious from Frazer's own innumerable examples from around the world that this emotional and imagistic element is found in accounts of actual magical performances, and that especially in hostile acts of sorcery also these are motivated by desires for revenge or for defeating opponents. In synthesizing such accounts, however, Frazer, an inveterate rationalist, perhaps unconsciously sought to display 'primitive logics' while at the same time safely pointing his finger at them as spurious science.

In seeking to equate magic with spurious science Frazer was directing his readers' attention to the putative evolution of true scientific knowledge out of pseudoscience (alchemy might be a prototype of this). He was both giving a rationalist twist to magic and at the same time preserving a view of it as 'irrational'.

What, then did Malinowski and Radcliffe-Brown add to or take away from this Frazerian scheme? Malinowski supplied the emotional and performance-oriented frame to the study of magic by actually observing how it was deployed by specialists in rituals of canoe-building and garden-making (Malinowski 1922, 1935). Radcliffe-Brown, for his part, took a different tack in his study of taboo as a category (Radcliffe-Brown 1952), pointing out that distinctions between magic and religion cannot clearly be made in cross-cultural analysis and creating a wholly practice-oriented and performance-centered approach by homing in on the ritual acts and processes involved in the category of 'taboo' and the correspondence of ritual actions with changes in the status of the actors.

In both of these cases, however, the essential difference between Frazer's expositions and those of both Malinowski and Radcliffe-Brown lies in the development of the idea of action as performance by these two authors. From this perspective it can be seen, with more than a touch of irony, that their work anticipated embodiment theory, which is generally viewed as a response to, and replacement of, functionalism; which in turn Malinowski and Radcliffe-Brown are generally and somewhat simplistically represented as having founded in the context of British social anthropology.

Their work certainly was functionalist, although in quite different ways; but such an orientation was not a break from Frazer, who himself came to

such a perspective in one of his own works (*Psyche's Task*, published in 1927). What each of them took, however, from their own fieldwork, was an intrinsic understanding of the importance of observing what people do, rather than simply speculating a priori about the logic or illogic of ideas and beliefs; and a performance viewpoint emerges easily out of such a focus on observed behavior.

There were large differences of overall perspective in other ways, of course, between twentieth-century and nineteenth-century paradigms of scientific knowledge. A taken for granted evolutionist perspective tended to dominate in the nineteenth century (somewhat blunted by diffusionism in the case of comparative ethnology). Functionalist theory in the twentieth century brought more into focus the study of the actual working of social systems regardless of their putative evolutionary standing. Moreover, the critique of evolutionist schemes, built mostly on speculation about the so-called group marriage as an explanation of classificatory kin terminologies, was based on better explanations of particular cases founded on a functionalist perspective (see, e.g., Hiatt 1996:52–54 on how Radcliffe-Brown refuted some of Morgan's work by reference to his fieldwork with indigenous groups in Western Australia).

Malinowski also began his work with Australian Aboriginal materials, using detailed evidence based on practices in their context to cast doubt on the reality of any full-scale group marriage structures and firmly reinstating the idea of the domestic family as a unit of social structure. Both men, therefore, used the weapon of fieldwork and close attention to customary practice as their means to undermine speculative evolutionist explanations of features of custom. We can see here, too, that functionalism as a theory grows out of the observation of practice, and thus of performance. Fieldwork thus became the means to establish new standards of both description and analysis.

All such breakings of frames tend to be followed by further repercussions and recursive loops of fashion. An interesting intervention on theory in anthropology was later, but not much later, to emerge in a challenging work by I. C. Jarvie, who was inspired by a philosophical training at the London School of Economics, particularly through acquaintance with the work of Karl Popper. Jarvie took as his starting point a kind of omnibus effect of the works of Malinowski and Radcliffe-Brown dubbed as 'structural-functionalism'. Such a categorization of their work itself produces a part of the problem that the critiques of it claim to solve by demolishing the approach.

Jarvie took this crustacean-like, shell-hardened interpretation of the work of these two 'founders' (who, tellingly, disagreed with each other on many matters) as his starting gate, and immediately announced (Jarvie 1964, p. xvii) that structural-functionalism as a theory fails because it deduces causes (functions) by means of observed events; and also, perhaps more securely, because it cannot handle social change. He went on to examine a well-known and subsequently well-studied phenomenon of change found in Pacific Island societies and categorized in the literature as 'cargo cults' (p. xvii). Instead of functions Jarvie offered 'the logic of the situation' and 'unintended consequences of actions' as the means of explaining events and processes.

Jarvie, however, was not content with this deconstructive/reconstructive exercise. He also had a narrative- drama, based, tongue in cheek, on the Freudian theme of father-killing. Malinowski and Radcliffe-Brown were typecast as sons wishing to kill their father, Frazer, by means of trumpeting the method of fieldwork as opposed to armchair observations, and adding an ethical sub-text about the importance of treating those studied as human beings deserving of respect. Another sub-text might be like the putative 'psychic unity of mankind' which was in fact an idea with much earlier roots (cf. the quote from the Latin poet Terence: *homo sum, humani nihil a me alienum puto*, "I am human, I do not think of anything that is human as alien to me").

Another starting point of Jarvie's enquiry anticipates post-modern concerns: he argues that "all description involves theory because all factual statements have theoretical content" (p. 55); that is, they are constructed from a particular problem angle. All this can be accepted, and also kept in mind as a background to Jarvie's examination of the cargo cult literature as it stood in 1964.

He goes on to give an action and actor oriented version of what cargo cults are all about. They arise out of colonial inequality, particularly in terms of wealth and access to goods, and they are magico-religious efforts to correct or reverse this inequality or disparity by the use of religious doctrines and by following leaders who say they can effect the required changes. As a side-note here we should comment that the means employed for these corrections are all ritual in character (see Stewart and Strathern 2014). From this perspective, a question we can ask is why these attempts at redressing inequality should have taken a ritualized form, and the answers we give will depend on what we mean by ritual here.

This, however, is not Jarvie's focus. He wants answers in terms of situational logic which itself implies a vision of the rationality of action,

including the rationality of cargo cult actions. His approach is also based on methodological individualism, that is, the idea that human behavior must be explained in terms of human decisions, and he focuses on the importance of leaders ("prophets," p. 118) whose actions fall "within the magico-religious framework of the culture" (p. 114). The apocalyptic character of many of these cults he attributes to the case of a "closed society" (p. 115) undergoing change (an idea he derives from Popper). It turns out that this Popperian idea of the closed society is important for Jarvie, since he suggests that people in such a closed society saw the individualistic character of the 'white man' and wished to emulate it (p. 118).

This approach is very shaky, and is not necessary to the starting point of structural logic that Jarvie invokes. We do not know that the Pacific Island societies in which cargo cults flourished were closed. There is evidence that via trade, migration, intermarriage, and the diffusion of custom some of them, at least, were quite open in many ways. If we abandon the idea of the closed society but stick to situational logic, we have still to admit that such logic is inflected by magico-ritual ideas, and by the incursion of new ideas inflected along these lines (and accompanied by rituals, again, we must note). Jarvie follows a somewhat intellectualist and rationalist bent in pursuing the details of explanations along his own lines.

What is missing from his account is an appreciation of embodied collective action and how it works. Taking as an example a movement that flourished briefly in the Mount Hagen area of Papua New Guinea for a while after Jarvie's book was written, the *Red Box Money Cult* (Strathern 1979–1980; see also Stewart and Strathern 2000a), the aims were to transform pieces of metal stored in the red wooden boxes, used by labor migrants to bring home wealth goods to their kinsfolk and so gain power and prestige, into money.

The rituals employed to stimulate this effect were directed to the ancestors, renamed as wind-people because they could now travel on winds down to centers of power such as the national capital Port Moresby far away on the southern coast of Papua. Invocations, sacrifices, ritual songs and dances were enacted as the performative means of ensuring this transformation. Rooted in older notions and in the general significance of ritual action to obtain benefits from spirit forces, this transformative ritual was nevertheless new, without clear precedent, a product of imagination and ritual experimentation. When it failed, because the boxes when opened did not contain money, the leaders were discredited and the movement atrophied. In the local area studied,

the leader was not an established 'big-man', and his group was one that had sunk in prestige in the local network of exchanges of wealth in pigs and shells.

The cult was an attempt by the leader and his wife (who was a sister of a powerful local politician and Member of the National House of Assembly) to recoup the prestige of the group and establish themselves as shamanistic styles of leaders in the contemporary world of change. The important problem here is to explain why they chose ritual means to try to realize their aims. One answer is simply that it was rooted in traditional ways. But this is insufficient because there was also a definite ritual innovation at work. This in turn implies that tradition itself encapsulated the results of earlier ritual experiments. That is an illuminating point because it belies the concept of the closed unchanging, or 'primitive' society that one might have expected Jarvie to challenge since he was opposing structural-functional analyses that depend on the notion of the unchanging society. In our conclusion, therefore, the solution to the problem of explaining cargo cults thus lies both in understanding the prevalence of ritual as a means of dealing with difficult situations and in reckoning with the human propensity to creatively 'innovate' and, by implication, to learn from experience.

Situational logic can indeed explain history as Jarvie claims (p. 223); but it needs to be linked with a broad contextual understanding of what that situation is as seen by the actors themselves and that brings us back to culture and to interplays between structures, functions, and innovative actions. In conclusion here, Jarvie's intent was to break the frames of structural-functionalism by offering an explanation of cargo cults that depended solely on situational logic. However, it turns out that, first, this putative logic must comprehend culture, and second that culture must comprehend innovation, which ex definitione implies change.

Finally, the cults are all run on ritual, and therefore ritual theory is crucial to understanding them. The frame is here broken by being replaced by another problem-frame, the problem of ritual actions: to which, as we have pointed out, Malinowski and Radcliffe-Brown made notable contributions, structural-functionalist or not. Towards the end of his broader discussion on 'the aims and methods of social anthropology', Jarvie has a footnote in which he characterizes Malinowski's functionalist paradigm as the core of an academic cargo cult that promised to deliver a science of society. Jarvie admits that this particular cult actually delivered "an abundance of goods" but, he adds, not a full-scale Science of Society (p. 184).

He also suggests that both Malinowski and Radcliffe-Brown were inclined through their fieldwork in small-scale situations to think of societies as wholes.

We may rather suggest that this idea of the whole was a theoretical post hoc move, because the social contexts these two founding figures studied were either quite fluid and expansive or had been modified by colonial contact. Perhaps also Jarvie himself is in a quandary here, because his own idea, borrowed from Popper, of the 'closed society' suggests a bounded whole as a unit of study. These differing frames of analysis produce multiple contradictions and conundrums. In seeking to break functionalist frames and to lead us back into the kinds of generalizations Frazer attempted as an exemplar of comparative and generalizing study, Jarvie himself depends on the very idea of a totality that he employs to criticize the functionalist theories he is attacking. He seeks to bring back to life Frazer and to kill his own immediate father figures to give life to the grandfather. However, a more subtle approach recognizes that this is too crude an approach, and to get closer to history we have to break the frames that Jarvie has used to break other frames. So the process continues.

CHAPTER 2

Change

Abstract Change is a complex concept, but change is normal in the history of societies. Synchronic accounts do not preclude paying attention to change. Cargo cults result from historical factors of relative deprivation. Explaining these requires that we understand why they take a ritual form. The Red Box cult movement in Papua New Guinea is adduced, arguing that its rituals were invented as a way of shortcutting the pathway to obtaining wealth, when there was swift political change and rapid exposure to the outside world. Neither structural-functionalism nor Jarvie's methodological individualism is adequate to grasp the phenomenon. Comparison between cases in Papua New Guinea helps to show similarities and differences. A mindful approach helps to steer a pathway through the construction and deconstruction of theories.

Keywords History · Methodological individualism · Papua New Guinea · Red box cult · Ritual · Wealth

The primary charge that Jarvie brought against the structural-functional approach was that the theory which informed it is incapable of dealing with the explanation of change. He himself modified this charge at a later point in the discussion of cargo cults, suggesting ways in which structural-functional models could in fact be used to explain change.

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We want to take up this issue from our breaking the frames perspective. First, what does the term 'change' mean? The answer is that it means too many things to be used as a simple counter in any debate. The critical case that Jarvie took up for extensive examination was a case where changes of various kinds were obviously, if not glaringly, implicated in the activities of people in cultural contexts that had been theorized as formerly in a stable and unchanging state. How, then, did the changes arise that led to the activities labeled as 'cargo cults'?

It hardly takes much time to find numbers of responses to this conundrum. First, the question is based on wrong or inadequate assumptions. It was the arch-functionalist, Radcliffe-Brown, who pointed out that in cases where we have no means of reconstructing histories, all we can do is to produce synchronic analyses of the present. By the same token, however, when we do establish some lines of enquiry into history, we do not necessarily find stable or long-lasting situations of complete continuity. Rather, we find from oral history or from literate sources where these exist, that change of one kind or another is a normal state of affairs.

Second, it is not entailed by a synchronic functional account that change is precluded. Functionalist accounts are models of how the analysts think that structures of social relations replicate themselves over time. They may show that elements of structure work together towards this replication. But they do not in any way show that if circumstances change this 'system' will automatically adjust by resisting introduced changes rather than by accommodating to them and thereby unleashing a series of changes (and thus exemplifying Jarvie's own point about unintended consequences of action). Moreover, a demonstration of the imputed functions of an existing state of affairs in no way precludes that individuals may introduce changes in accordance with their own wishes, whether there is outside influence or not. Individual creativity is always potentially present, especially if it is not stifled by inequities of power; or, those holding power may also desire change and be able to institute it.

Third, and finally here for this part of the argument, change covers many things, from big to small; but even small changes can lead to bigger ones. There is the famous study by Lauriston Sharp of how the introduction of steel axes into one Australian Aboriginal community is said to have caused an extensive set of resultant changes in the structures of power and influence (Sharp 1934). Whether this story covers the whole set of events or not, it exemplifies how a technological change may trigger further changes without its instigators intending to unleash these. Technology,

by itself, however, is not likely to be the sole cause of change, since it is always inserted into processes that involve existing structures of relationships and also persons with agency who develop the potentialities of a technological change within the limits of social values to which they adhere, or per contra they may change and redefine those limits.

Cargo cults (to return to our discussion above), as they were dubbed, exhibit rapid and often unexpected sequences of change in practices, not ones that result simply from technological change but complex changes of reactions to colonial situations. Inequalities of wealth and power that make themselves sharply felt in those circumstances stimulate wishes to redress such inequalities, and these in turn issue in ritual activities designed to achieve such a redress. Since the circumstances are new, the rituals also are new, but they are built out of components that draw on features derived from past complexes of action.

The problem of explanation, therefore, comes from the difficulty of understanding how change can happen if we use a model of the situation that implies stability and lack of change up to the moment that the cult begins. The problem instead, (as we have noted), is why the response to change takes a ritual form, as a way of obtaining wealth, rather than any other form of economic or political action. The first thing to observe here is that the cult ritual does not preclude that people will also try other means to help themselves. However, the ritual is designed to shorten the process of reaching one's aims. To illustrate this point, we will review again the case we have already alluded to above, dating from a time shortly after Jarvie wrote his survey of materials, and in a region where people had also taken up cash cropping by growing coffee and selling coffee beans to commercial buyers. In other words, they had responded to the injunctions of the colonial authorities to take up cash cropping in this way, as a part of the project of modernization intended to lead up to political independence. The locale was Mount Hagen in the Papua New Guinea Highlands, and the dates were 1968–1971 (see Strathern 1979–1980).

The central ritual of the cult sequence was never directly observed because its procedures were closely guarded by its participants and supporters who hoped to benefit from it. The historical background is that there were earlier movements in the Highlands with similar aims after 1945 when Australian colonial control was stepped up and ideas of development and change first began to be implemented. The 1968–1971 movement was distinguished from these earlier disturbances by a number of ritual innovations that showed the lability and creativity of human

imaginations fed on an increasing amount of information reaching the Hagen area from outside, through labor migration and the first phases of political change coming with the introduction of local government councils and the prospects of an elected national House of Assembly, as a forerunner of an independent Parliament. Cash cropping from the cultivation of coffee, along with the new taxes payable to the local council, added stresses from the level of the increasing significance of state money and the subsequent demise of the value placed on pearl shells and other shell valuables that had been used to stabilize intergroup relations and to enhance the growth of the influence of local leaders through their deployment of these wealth items in peace settlements between groups. In other words, the new movement arose in a time of speeded up political and economic change, with an entanglement of the people's perspectives on the world to which they were increasingly exposed.

The instigators of the movement came from a group with a turbulent local history, the Yelipi clan of the populous Minembi tribe. Their story, as well as their actual clan name, revealed that they were war refugees, driven out from an original territory far to the south in a different dialect area, and obliged to seek help from affinal relations in a clan of the Tipuka tribe, after they had fallen out with their paired allies in the Minembi tribe. They had been granted an infertile, rocky territory on which to settle, and they had also recently failed in an attempt to establish themselves via a set of pearl shell and pig transactions with neighboring partners.

The movement, therefore, had the double aim of reversing relations of inequality with their neighbors and further raising the level of their wealth in new monetary terms in comparison with the colonial government and its settlers, who owned and operated coffee plantations in the area.

How, then, did the new rituals aim to deal with this situation? The central symbol was a set of red wooden boxes of the kind sold in Chinese-owned stores in coastal locations and brought home by returning labor migrants with trade goods such as clothing which the migrants could share with their relatives. These boxes were re-purposed by the cultists and filled with old pieces of metal, and then placed securely in secluded houses built for the storage of wealth. The participants made sacrifices of pigs and invocations to their ancestors, asking them to turn the contents of the boxes into the introduced state money notes.

While such prayers to ancestors, accompanied by pork sacrifices, were entirely in line with customary ritual forms, the red box element was new and unprecedented in local ritual logic. This element also induced

skepticism and scoffing by the leader of a neighboring clan of the Kawelka tribe. Again, such a circumstance of skepticism and doubt was new in local ritual history. New cult practices to strengthen group fertility and secure environmental cosmic favor were known and indeed were continuing to circulate and become adopted by successive groups in chains of exchanges along with the hiring of ritual specialists. But these cults offered long-term and unquantifiable benefits, with an expectation that the cosmic powers would be renewed over the generations by a repeat of the basic rituals, after which the cult stones were secretly buried in clan group territory, to be mobilized later.

The new cult, by contrast, claimed to produce quick, identifiable results and its leaders set a time when they would hold a great celebration dance, bring out the boxes, and open them up expecting to find treasures of state money (at that time Australian dollars, the colonial currency and marker of power to purchase new commodities).

In previous practices, also, ancestors would not be approached to send immediate benefits of wealth, although their general beneficence would be sought, in particular in situations of sickness or in-group conflicts. The red box cultists had renamed the ancestors, previously known as the spirit people (*kor wamb*), as the wind people (*köpkö wamb*). The wind people were supposed to have the power now to fly from the local area down to the coastal city of Port Moresby, the political capital where the new House of Assembly was located and where the colonial Administration headquarters were, and flying back and forth they could supposedly tap into the power to transform base metal into money. Some of the cult leaders went down to Moresby by plane and were said to contact the wind people there and gain intelligence about the progress in the making of the money.

All of this imagery arose out of two new sources. One was the actual factor of increased contact with the outside world, symbolized by Port Moresby, which was also a target of labor migrants. The second was the image of flying, brought into being by experiences of planes that brought cargo from the coast and carried important people both ways. 'Wind' was the symbol of the new powers of mobility. The whole enterprise of the cult was called 'wind work' (*köpkö kongon*).

Certain elements of the secret rituals were also said to have been imported from Madang, a coastal city located in the middle of a region that had experienced long and repeated efforts by local leaders or 'big-men' to create wealth by ritual means. In his discussion of such cults Jarvie tended to stress the importance of 'prophets' or new leaders who could

gain influence and resources by persuading large numbers of supporters to put energy into making the cargo arrive by intense ritual activities. In Hagen, while each local manifestation of the wind work practices had someone at the heart of it who was a prime actor, no consistently influential set of 'prophets' emerged to spread the rituals from group to group. Local leaders in Hagen were, and are, deeply involved in public exchanges of wealth through which they make peace, create alliances, and gain in prestige. Nevertheless, some of these leaders stood indirectly to gain from the wind work. The wife of the leading figure among the Yelipi was the sister of a prominent local politician in a neighboring tribe (as noted earlier). She and her husband solicited money contributions from supporters and much of their money seems to have been 'banked' in the settlement area of the politician, and when the cult failed, these monies were rumored not to have been returned to those who had contributed them.

Undoubtedly money itself was the major focus of ritual interest in the cult. Songs were created, with images of the wealth that was to come from the 'work'. One of these songs contained an image that invoked the World Bank, envisioned as about to break open with money over the head of the singer, like money from 'heaven'.

If we review these elements of change we see that they all belong to the sphere of vital cultural imagery, and all are set into the context of the experience of specific changes in local politics and economy. Social structure as a whole was not a factor, because its existing patterns were simply harnessed to mobilize support for the new cult.

At the culminating dance the boxes were found, after being opened, to contain only the useless pieces of metal that had been placed in them. The cult leaders melted away, and the rituals were never (to date) repeated. The original impetus for writing about this ritual experiment was to demonstrate that such cargo rituals could emerge in Highlands societies that were seen by outside observers as based on rational and secular economic practices which would make them impervious to such 'irrational' activities. The example showed, per contra, that Highlanders were as susceptible to imaginative images of possibilities as any others. It also showed, however, that whereas the cults had often tended to replicate themselves again and again in coastal areas, in Hagen, at any rate, this did not happen. Hageners, instead, immersed themselves in ceremonial exchange, cash cropping, national politics, and conversion to Christianity – enough to keep them busy with efforts to achieve wealth and influence of the kind the wind work cultists dreamed of.

What price, then, in all this, both of structural-functional analysis, Jarvie's charges against it, and of his alternative of methodological individualism? Structural-functional models assumed that systems maintained themselves in states of relative stability. However, by the same token, if changes were to occur in one part of the system, changes would automatically be expected in other parts. This is exactly what stimulated the wind work practices, which responded to multiple changes that had happened and sought to deal with these by rituals that aimed to speed up a rectification of inequalities of wealth that the cultists had observed and experienced. So these cults do not demonstrate that structural-functional models are powerless to explain change. However, Jarvie's own concept of 'the logic of the situation' glosses over, in its supposition of rationality, the complex effects of cultural imagination which we have seen crucially at play in wind work. Wind work was a kind of cultural bricolage, and as much based in collective fervor and what Pierre Bourdieu would have called 'misrecognition' as on any individual rationality. The collective situation of the Yelipi group was the effective seed bed for the emergence of wind work. So, to understand it, we need a complex, multi-layered method of setting out a historical analysis, founded neither on a strict structural-functional model nor on a sole reliance of the analysis on methodological individualism. Recognition of this point entails that we also realize the dangers of total system models and the need to pay attention to agency and imagination of individuals and groups in complicated historical processes.

Such a realization does not entail that we abandon all attempts at the applications of general ideas or theory to ethnographic materials. It does mean that theory should be kept in its place and not allowed to smother all alternative viewpoints on the data. For example, methodological individualism can be useful in so far as it draws our attention to the importance of individuals in social processes, but not in so far as it would incline the analyst to overprivilege individuals as autonomous actors vis-à-vis wider social processes of interaction. A useful guard against excesses of this kind can be set up by maintaining a comparative perspective, preferably among cases that show sufficient resemblances to be easily compared and contrasted in terms of their patterns.

The 'cargo cult' literature reviewed above illustrates these points. In his discussion, Jarvie focused on the significance of prophets as leading figures. This could well apply to coastal cases that he examined. We have seen that it applies less well to the Hagen red box cult movement. Also, prophets are

not merely individuals. They gather into themselves all manner of collective concerns, mythological themes, responses to class relations and historical exploitation. As individuals they make sense only as bricoleurs of the collective, otherwise they would not have followers. There is, further, a comparative contrast between Highlands and northern coastal cases, which can be explained in terms of different colonial histories and different economic circumstances. A feature of the discussion on coastal cases (as we have seen) was that cult movements tended to reappear after fading away, in other words they acquired a chronic or repetitive character. This did not happen in Hagen. Why? First, coastal areas were exposed to outside influence much earlier. While forms of Christianity came earlier, cash cropping did not come until later and was preceded by long-term labor migration to expatriate owned plantations where inequality was obvious and pervasive. The Highlands were exposed to colonial change much later and not long after this were introduced to self-governing local councils and cash cropping with coffee, including the demise of expatriate ownership of plantations.

The transitional period of the late 1960s and the 1970s when the wind work phenomenon came up was a time of stress when the people realized the difficulties of gaining significant wealth through growing coffee and became aware of the power of transactional forces as well as of the significance of banks and the processes of manufacturing money. These circumstances provided a unique context for the money cult to arise. As an experiment, it had a kind of pragmatic risk factor built in, because the boxes were to be opened and inspected; and, as with any investment scheme that carries risks, there was a danger of it failing, which it promptly did. Such a clear cutoff made it much more unlikely that anyone would try that experiment again. The cultists could have claimed that next time they would get the rituals right, but the investors would have needed heavy persuasion to put money and pigs in again, especially because there was in fact no 'logic of the situation' based in traditional practice that would have encouraged them. The ethos of the movement was rather expressed by one participant as an ideology of hope: *kol rop etep mep pamona pamona kopa etimba* ("we can pretend and pretend about it and then it will come true").

So much, then, for the immediate discussion of the cargo cult problem. In his attack on structural-functionalist anthropology Jarvie added another element. The revolution, he said, had produced a paradigm for writing specific ethnographies of local systems, but the works in these forms had

lost sight of the kinds of comparisons and generalization that the earlier armchair anthropologists had sought. If this was so, it would partly have been because the whole world view of such systems had changed away from an evolutionary perspective. The societies anthropologists studied were still quietly pigeonholed as simple or undeveloped but without much emphasis on this – understandably perhaps because the ethnography revealed how complex they were.

Drawing on earlier work that was actually rooted in evolutionary theory, anthropologists following in Radcliffe-Brown's footsteps looked to models of different systems of kinship, descent, and marriage as a locus of comparison and generalization.

While these exercises in turn drew criticisms, they certainly were efforts to build comparisons out of particular ethnographies. To operate, they had, however, to identify separate entities to be compared. As a heuristic device this was justifiable. However, when one realizes that the entities compared are not fixed but fluid, it becomes apparent that a different approach is needed. Processual theory provided one means of doing this. The frame of separate 'societies' became questionable, and the tide in study of flows and change came strongly in.

Such a process of change reminds us of how theory in anthropology often emerges from events in the world at large, as anthropologists adjust to the actual conditions and exigencies of fieldwork in new circumstances. This experience brings with it in turn a different perspective on past studies, based on some other paradigmatic assumptions. It is change that stimulates new creativity and questions about earlier analyses. As we have already argued, all this is very complex and does not necessarily amount to a story of simple incremental progress, but rather to an understanding of the dialectical relationship between anthropological theories and the world that they relate to. Such a consideration in turn leads to a realization that theories aiming to explain events are themselves a part of such events. The outcome is to cast some doubt on totalizing systems of explanation, while in no way abandoning the project of theory construction. Highly closed and deterministic applications of theory, however, are less likely to survive over time than open-ended and provisional ones. We introduce here the idea of 'Mindful Anthropology' as a way of proceeding thoughtfully through theory construction and deconstruction in such circumstances.

'Be mindful' is an injunction that counsels us to be open to the world of experience, and to find our way through it by noting and remembering the fine lace of connections among the phenomena we study. A mindful

approach to anthropology would keep us open in this way. It can be counterpoised creatively to approaches that steer practitioners into frames that are in vogue and are supposedly the hallmarks of a modern or contemporary version of theory. It is important to keep up with these frames of course, while bracketing them as provisional. It is even more important to realize that every time we adopt a particular frame we are excluding some other approaches or frames that might provide an insight of their own.

A mindful anthropology, therefore, enables us to adopt a particular viewpoint while remaining aware that other viewpoints may suit aspects of the analysis more fruitfully. A related point is that when we are discussing a particular ethnographic area of the world we can helpfully keep in mind comparison with other areas, unrestricted by region or timescale. A further point is that we need to keep the conceptual boundaries of our discipline open, while retaining its central insights into the materials we study. Philosophy can always be a useful aid to mindfulness, especially if we extend our consciousness to the stances inherent in, for example, doctrines that explicitly address mindfulness itself as 'being aware'. In our own studies of 'landscape', memory, history, ecology, and ritual/religion, inherently enter the analysis, leading to a holistic, but open-ended, view that remains rooted in practice and experience.

Anthropology has also to be open to a changing world. We cannot always talk about the same things because things change, and it is our job to track and understand these, as we also are a part of them. If one practices anthropology for half a century, say, changes both in the world and in fashions of anthropological analysis become very evident. Some topics fall out of fashion, putatively because they are 'out of date'. This was the fate, at one stage, of 'kinship studies' as a central category of study, partly as a result of the applications of rigid modes of analysis of 'kinship terms', and partly as a result of deconstructionist efforts to shatter the concept of kinship itself as cross-culturally applicable. A moment's mindfulness can serve to dissipate this mirage of something like kinship being out of date. People around the world continue to practice kinship ties on a daily basis, whether anthropologists say so or not. A new and expanded generation of kinship studies has sprung up within the overall context of 'globalization'. Globalization itself is a loose descriptive term for a conglomerate of diverse processes. As a linguistic usage it may give fixity to a highly fluid set of phenomena. It may reify into a frame of analysis that substitutes itself for more in depth or local analysis. A mindful approach, like that of a far-seer pilot in an interspace

machine, will seek to look through it (globalization) to the diverse circumstances that compose it, rather than simply labeling the phenomena as a result of 'it' – when 'it' itself is not an explanation but just a label. The same must roundly be said of all buzz words that arise from time to time in the discipline and are stretched, squeezed, and spread so as to make further thought 'unnecessary', for example, the term 'neoliberalism', which runs as a partner along with general historical critiques of global capitalism. Fine, but what is left out and why? The same with 'post-modernism'.

Finally, here, there is another feature to which we must attend. As practitioners we tend to place boundaries around our discipline. These boundaries may suit us individually, and that is legitimate, but they are also rhetorical artifices of a political kind. How often do we find statements like 'Oh, that person is not really an anthropologist', meaning that their viewpoints and approaches differ significantly from one's own. The process is ineradicable because of the great and ever-growing diversity of topics that those who self-identify as anthropologists involve themselves in. A mindful approach brackets all such judgments, while seeking to make explicit personal philosophies and preferences that underlie them. 'Truth' and 'untruth' may live side by side here. Certain values such as engaged participation and observation, alertness to embodied behavior and language use, and overall humanity of description and analysis, can serve as guidelines, but there will be other, more activist, philosophies as well. A mindful anthropology will try, literally, to keep all this 'in mind' and appreciate its complexity as well as a countervailing need to sift that complexity and find the essential matters that may lie at its heart.

CHAPTER 3

Processes

Abstract Fredrik Barth's work is exemplary for the study of social processes. He used his ethnographic studies as ways of breaking previously applied frames of analysis. His work with Swat Pathans of Pakistan and the Baktaman of Papua New Guinea is adduced to show how his ethnographic findings led him to break or modify other frames of analysis. He inserted the study of competitive power politics into segmentary lineage theory. He struggled with the fluidity of Baktaman ideas, and the workings of secret knowledge, leading him to consider memory, loss of information, and ritual variation among different groups. He eschewed whole system typologies and a temptation to impose too structured an interpretation of data. His work verges on what we now call embodiment theory.

Keywords Baktaman · Embodiment theory · Fredrik Barth · Power · Segmentary lineage · Swat Pathans

We take here the work of Fredrik Barth as an example of a dedicated thinker who constantly related his ethnographic work to the production of theory, by examining how, in mindful ways, theory and ethnography can feed into each other. Ethnographic experience leads to the identification of problems that do not yield to some previously formulated frame of interpretation, and therefore provokes a re-think of these frames. In other

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words, ethnographic experience is the primary source of 'breaking the frames', but only if it is already linked in a thoughtful way with some existing scheme of analysis.

Barth himself provided a succinct account of this process in multiple ethnographic contexts of his fieldwork around the world. We will look at his discussion in relation to two such contexts: his Swat Pathan study and his work with the Baktaman of Papua New Guinea. These contexts are remarkably different in scale, history, and the questions that informed Barth's fieldwork. So, our purpose is not to seek any direct comparison, but rather to see how Barth posed his problems and solved them by choosing a frame of analysis that enabled him to shift explanatory schemes from one modality to another.

An important point that Barth makes is that his own marginality and plurality of background was an intellectual strength for him as well as a career disadvantage at early stages of his trajectory. Plurality of viewpoint kept him from being wedded to any one way of theorizing at the expense of others. Marginality meant that to begin with he could seek to explore contexts of learning from outside of Norway, for example, Cambridge in the UK and Chicago in the USA, and subsequently build on these to establish a strong institutional context in his own country.

A fundamental viewpoint that he traces in his own rendering is one he derived from the work of Edmund Leach's *Political Systems of Highland Burma*, first published in 1954. Leach had demonstrated two things: first that cousin marriage practices among Kachins varied in step with political processes; and second that ethnic identities, also, could shift, with Kachin at the upper end of a socioeconomic scale becoming Shan and consolidating their wealth within the new identity.

Much in line with Leach's general ideas, Barth reports that he decided to avoid an analysis in terms of institutions, rules, and norms among the Swat Pathans, and instead to concentrate on choices of political affiliations made by male actors. Such a choice would seem to align him with the kind of methodological individualism espoused by Jarvie (see previous chapter). However, it is important to look at the reasons he gives for adopting this approach. The Swat Pathans had ramifying, segmented descent groups, with ranked subgroups among these. This is undoubtedly an institutional framework informed also by certain moral ideas of correct behavior. But in terms of politics, descent did not simply determine identity. Men made their own strategic choices in search of what appeared to them in their best advantage. With some influence of rank and chiefship, leaders needed

to maintain men's houses in which they fed their supporters. The chief point here is that supporters could shift their allegiance to another men's house so that leaders were in competition with one another. The ability to mobilize supporters for force in social action was crucial. So choice was not just random individual action. It was strategic, linked to political economy, land ownership, surpluses, and access to resources.

Attracted by the idea of a systematic game theory analysis, Barth went on to propose that the process of competition led generatively to the emergence of two large political blocs. Apparently these blocs existed and were recognized by the actors, but could not be explained by any narrative rules or traditions. Other questions arise: were these blocs stable or unstable? What was the outcome of their emergence: a balance of power or a perpetual disequilibrium? Formal game theory by itself may not be able to explain these matters, since they are likely to be contingent on history. These same questions, however, are very relevant to what has happened in tribal politics among Pashtun speakers subsequently. Afghani and Pakistani politics have been characterized by the emergence of large blocs in regular conflict with state governments. These blocs clearly have some underlying structural features that correspond to large-scale tribal coalitions, but they appear as ideological units in conflict with other such units. Fredrik Barth's modes of analysis clearly, therefore, relate to the emergence of entities such as the Taliban, that have subsequently preoccupied international concerns, but their relationship to ethnographic work is what would be needed to turn Barth's work to further advantage today. (He himself explored these topics in a work published in Norwegian.) The tragic events that have overtaken the Swat Valley where Barth did his seminal work underscore the need to understand these processes and how they do or do not intersect with introduced Islamist military ideologies.

Several other points arise here. First, what is left out? A feature of Swat social organization that appeared in Barth's (1959) ethnography was the significance of 'saints' (*pir*) as mediators in social relations, including relations of conflict. Religion and ritual therefore enter into the analysis at this point, but how exactly does this modify the way we may view the whole analysis? Second, Talal Asad made a critique of Barth's work from a basic class-based Marxist framework, pointing out the great inequalities between persons and groups in the Pathan society. Barth notes that he did not find himself much attracted to such Marxist recensions of his materials, and it is not hard to interpret why, because his own analysis concentrated on the leaders as political players and did not require a class-based analysis

for it to work. Barth objects, as we do, to the importation of frames that are the product of extraneous concerns other than the immediate case materials to hand, although we must add that Asad's viewpoint enhanced the understanding of inequality in the Pathan social structure. Third, and perhaps most fundamentally, Barth has throughout his life stressed the importance of identifying *processes* and *patterns.* Processes are generative; they underlie patterns. Process is an analytical, not a descriptive, category. How do we read back from patterns to processes, then? That is where a mindful approach is required. In looking at Pathan politics, it is clear that patterns of gender-based honor and masculinity are at play. The processes that these patterns lead into invite comparisons with other areas that can reveal analytical continuities. Barth's own 'generative approach', which he developed in his "*Models of Social Organization*" implies that by starting with a few fundamental features of a social situation we can generate ways of understanding how complex outcomes of choices emerge. The problem, again, has been to know how many intermediate factors have to be adduced to arrive at such outcomes.

There is a fourth consideration that brings religion into focus, just as the first point about 'Saints' did. Akbar Ahmed criticized Barth's work by arguing that it misses out the factors he calls 'millennium' and 'charisma'. Charismatic leaders emerge who can generate new support for themselves by envisioning ideal patterns of change. If this is correct, it goes far to explaining the emergence of groups such as the Taliban in the region. Ahmed implies that Barth's analysis is secularist and rationalistic in its orientation, and hence is unable to comprehend dimensions of action that fall outside the scope of his models. This kind of point obviously applies more strongly today when there are endless debates about the working of links between religious ideologies and violent actions, most notably in relation to the rise of severe conflicts in places such as Syria, Iraq, and Pakistan/Afghanistan, but also parts of Africa and the Philippines.

It is therefore particularly interesting that Barth's own roving mentality brought him much later to tackle the problem of understanding religion and ritual in a remote part of the world among the 183 Baktaman people of the Mountain Ok region in Papua New Guinea. While the bulk of his analyses in other areas remained concerned with ecology, economy, and balances between these factors in achieving social equilibrium, in the Baktaman study he became absorbed with questions of meaning and the articulation of knowledge.

As with all of his work, Barth was seeking to find a way to understand the meanings in Baktaman rituals that would avoid the pitfalls of two widely followed but diametrically opposite theoretical standpoints. One was the structuralism of Lévi-Strauss, which Barth viewed as abstract and too far from lived experience. The other was Clifford Geertz's method of thick description or the layering of multiple details in the search for patterns. Instead of these approaches Barth wanted to employ his long-standing method of study with everyday experiences of people and how they themselves gave meanings to their rituals, and then moving on to the organizational ways in which these experiences were codified and transmitted. The first thing he found was that in circumstances of this kind, where the field worker starts almost from scratch, there has to be someone who particularly helps with the work. Barth found this person in Nulapeng, a young man who was himself both an insider and an outsider among the Baktaman. Particularly, he spoke Tok Pisin, or New Melanesian Pidgin, a bridge between local vernaculars and English. Barth also focused his topic down by looking at the stages of male initiations – much as he had looked at the actions of male leaders in Swat. These initiations were staged over many years of an individual's life, but Barth was able to 'construct' a picture of the various stages during the one year he had in the field. He was, in effect, making a mental reconstruction of Baktaman experience. Although he emphasized direct observations and non-invasive methods of enquiry, he was doing what he had also done in his Pathan study. There, he was actually building an account of a system that had flourished prior to the emergence of a central state ruled by the Wali of Swat from 1914 onwards. In other words, he had to dig back from a structure in which the two large factions had been absorbed into one dominant coalition.

It was the sixth degree initiator Kimebnok who secretly told him about the seventh degree of initiation. The question of who knew, or did not know, what turns out to have been central, and Barth himself shifts the question of meaning around by examining it from the viewpoint of how knowledge was communicated – or not. The initiations were repeated only about every ten years, and the experts who presided over them had to be creative in introducing performative variations rather than simply memorizing and sticking to exact details. This in turn would mean that much variation could enter into rituals over time, and systematic codifications of meanings would not occur. Barth was then able to link this feature to the almost entire lack of exegesis about meanings of ritual actions, a lack which went in turn with a stress on secrecy and a fear of telling anything that was

not allowed. We arrive at the paradoxical point that knowledge was protected by not being passed on as much as by communicating it. The whole elaborate hierarchy of initiation grades also seems to have been devoid of politically coercive power. Those in the highest grade did not command services from others except in relation to the ritual taboos themselves. Why, then, the seven degrees of initiation at all in an economy that was small-scale and based on subsistence? Barth's careful and patient work brought him eventually to two conclusions.

One was that the experts he worked with tentatively brought forward what he calls metaphorical meanings in the objects used in rituals. By this he is saying that the initiations of novices are all about growth of human bodies and plants such as taro. That is, the main values sought out in the rituals are clear and all are directed towards cosmic and social reproduction. The meanings of items were inflected according to these values. Water, for example, was seen as a cleansing agent, but in the form of dew on leaves it could be used to stimulate growth. Perhaps we are not dealing, from the Baktaman ritual viewpoint with the same things here. Perhaps stream water could be cleansing, while dew was seen as imparting fresh powers of growth to plants. In any case it is not clear that we need to invoke the idea of metaphor here, other than by saying that any object can be seen to have powers associated with its emplaced life force.

Another feature captured Barth's attention. Actions explained as secret knowledge to lower grades were later said to be tricks or hoaxes. Items said to be polluting would also turn out to be sacred when their meanings were revealed at higher levels. The effect of this would be to bracket all knowledge itself. Barth sensed that at another level this bracketing led to a sense of mystery rather than established codification, such as a structuralist analysis might seek to impose on the details.

Apart from this suggestion, Barth became aware of much local variation in ritual practices among neighboring areas. In 1982 he was invited by the Ministry for Energy and Minerals in the Papua New Guinea Government to revisit the area of his fieldwork and surrounding areas to assess changes and adaptations to change stemming from the activities of the Ok Tedi gold mine. Barth arrived in Papua New Guinea with his wife and colleague Unni Wikan, and one of us (AJS) received them at the office of the Institute of PNG Studies, where he was at the time Director, on leave from his position as Professor and Chair of Anthropology at University College London. Barth was to write a report for the Ministry and the Institute was helping with the arrangements. In characteristic fashion Barth had

packed only a single large suitcase to take into the field, which he handled himself, and the pair departed before long for the Ok Tedi area. The report he wrote contained much sensible observation on conditions in the hinterlands of the mine. In his biographical account, he also notes that the occasion of this revisit enabled him to establish points of comparison and contrast between the Baktaman and neighboring Faiwolmin speakers, and he uncovered a series of variations in ritual practices. Transcending the limits of the local Baktaman study he was able to gain a wider view of the meanings of ritual practices, which he turned to good use in his book '*Cosmologies in the Making*' (Barth 1987). Secrecy would preclude the widespread sharing or diffusion of cultural forms of ritual. Barth found that in particular communities there could be incremental efforts by experts to enrich and harmonize the ritual codes. But it is evident that the overall effect would not be to build up systematic and complex elaborations of meaning. The process of integration would instead be partial and dependent on individual experts as thinkers and innovators.

In suggesting that there could be moves towards integration of themes Barth was actually following a line of thought that at least touched on a part of Lévi-Strauss's work on 'La Pensée Sauvage' (1962), in which Lévi-Strauss uses the concept of 'bricolage' and points to how concrete items can become the vehicles of abstract thought. In breaking with a formal structuralist approach dependent on binary schemes, and pursuing what we now would call embodied and immediate meanings, Barth finally turns back to the idea that thinking through objects leads to a particular pathway of understanding the ontologies and philosophies of people.

We can see how in this work Barth eschews both whole system typologies of exposition and any temptation to impose too much structured interpretation of his data. This is an approach that fits well the Baktaman case, as was his purpose, and indeed, the 1987 book fits well with a historic debate about order and integration in New Guinea ritual ensembles of practice (Brunton 1980). In effect, PNG societies show much variation in this regard. Practices directed towards a category of Female Spirit in the Mount Hagen area lend themselves well enough to analysis in terms of binary opposites and their resolution (see, e.g., Strathern and Stewart 1999). In terms of the social organization of knowledge a completely different approach stresses how different ritual experts competed with one another to define the most effective or 'correct' ways of handling assemblages of materials, thus following a 'practice' approach rather than a view of 'structure'. One approach is not superior to the other because

they have different purposes, not the same ones. However, we can usefully ask why in Hagen there could be a greater stress on binary opposites than among the Baktaman, and an answer is readily available, since binary categories and their recombinations are at the heart of Hagen social practices, as they are not in Baktaman. The Baktaman world is constructed around a ritual hierarchy that is insulated from the practices of a political economy of exchange, whereas these practices are central in Hagen. And here one of Barth's suggestions comes into focus. He suggested that it is in transactions that commensurability between values can be progressively achieved. The Hagen case fits this idea very well, whereas the Baktaman case does not, in spite of an apparent hierarchy of values displayed in the degrees of initiation.

Returning here to our first case of Barth's innovative work, on the Swat Pathans, an intriguing possibility presents itself of comparing Pathan and Papua New Guinea modalities of political competition. Here too, however, we will find that exchange practices are different. In Hagen *moka* exchanges linked and resolved issues of conflict in the aftermaths of violence, but these conflicts were rarely zero-sum in the way Barth found for the Pathans. And it is this difference that perhaps points a way to understanding how the later trajectories of these societies differ. In Hagen, killings were always constrained by the need to make reparations for killings. No such rule held for the Pathans, and the possibilities for conflicts even between close agnates were also greater. The fragmentations of descent groups conduced to the creation of bigger and more violent coalitions. Here, we are applying our 'Breaking the Frames' focus through deploying comparisons that can lead to fresh insights by leaping across ethnographic divides. We feel this is quite in harmony with the perennially exploratory and mindful attitudes of Fredrik Barth himself.

A twist of perspective can be found here that is a part of the Barthian magic of method. We have not explored here his seminal 1969 work on Ethnic Groups and Boundaries. The twist he injected into the debates on ethnicity was to move away from cultural contexts to the organization of boundaries between groups. This is quite like his approach to knowledge in general, where he is interested in how knowledge is created and transmitted (or, for that matter, lost). Of course, in the end, cultural content has to be involved, as a part of the competitive process of creating meanings and defining boundaries. The example shows by now familiar patterns. One frame is broken to create another. In another

moment, the new frame has to reincorporate back into itself an aspect of the old to arrive at an adequate synthesis.

Overall, then, it is processes that we are always returning to. This is why we ourselves cannot agree with approaches that say, for example, that processualism has to be replaced by post-processualism or any other 'post' approach. This is because, like Fredrik Barth, we do not worship '-isms', pre or post or whatever. Processes, in the Barthian sense, can be investigated in relation to particular problems, without involving any 'ism' at all, but by sticking to the point. As he himself discovered, this further implies that meanings cannot be left out; they are a part of processes themselves. Any opposition between meaning and process, therefore, is unproductive. To say this is just a beginning, as there are many different approaches to the question of meaning. One issue is whether the concept of metaphor is a semantic universal and so can be used across the board to identify processes of amplification of meanings in ritual contexts such as Barth encountered among the Baktaman. Metaphor is a term derived from literary usages of language. Metaphor and simile are recognized as related tropes which enable authors to highlight an aspect of a phenomenon by bringing it into alignment with another. It is obvious that something like this happens in oral poetry and song but the embodied character of these genres makes it important for us to see differences: when Baktaman elders employed in their rituals animal fur or water and leaves they were not creating metaphors as flights of pure imagination. Rather, they saw in these items powers they wished to impart to the novices, through contiguity or direct application. Their ritual actions were in this sense no more metaphorical than the actions of someone taking medicines for digestive or skin therapy. If there is any unexpressed ontology behind such actions it is simply a Frazerian idea that certain entities can influence others through physical or linguistic contact. In the direct world of experience of people like the Baktaman, these are just realities of being and embodiment.

Another tack on the question of meaning can be taken by asking what the people's own ideas of meaning are. In Hagen, the Melpa language provides just such a tool in the concept of *to*, a measure or comparison, entailing some uncertainty as to the validity of the comparison being made. A *to* is also like a conjecture, an exploratory hypothesis. The complexities of *to* reveal how the Melpa have a sophisticated idea about truth and reality. In all probability, this is quite like the Baktaman view, even if the Baktaman do not have a specific expression to signal this.

Fredrik Barth's work has been centrally important because of his capacity to break frames and make new ones. With the Pathan study he introduced choice and game theory. With the Baktaman study he took a grounded approach to ethnographic knowledge, eschewing structuralist schemes, and espoused a method that led him close to, although not quite into, embodiment theory.

CHAPTER 4

Individuals

Abstract Theorizing about the individual has a long history in anthropology, but has often been vitiated by the imposition of frames that oppose the individual to the collective or social realms and/or to the dichotomy between primitive/civilized societies. A historical and processual view sees the ongoing dialectical relationship between categories. Customs are not static but change, while still maintaining aspect of continuity. Thoughtful discussions are found in Anthony Cohen's work stressing self-consciousness. He criticizes Louis Dumont's argument that the concern with the individual is an ethnocentric western notion. Another arena of debate has been in Papua New Guinea, where the question of dividuals versus individuals has dominated debates. Our term, relational–individual, mediates this dichotomy, introducing a dialectical ontology.

Keywords Anthony Cohen · Customs · Dichotomies · Dividuals · Papua New Guinea · Relational–individual

Individuals exist everywhere. Classically, the concept of the individual has been both linked to and opposed to the idea of the collectivity, with several normative implications. Individuals are a part of the collective. They may sometimes oppose it or come into conflict with it. The collective is also composed of individuals with varying roles and identities. People

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who commit crimes are seen as enemies of society, loners. Collectivities differ in terms of how they handle individuals. Some societies are more individualistic than others. And so on. All of these analytical devices depend on the assumption that there are individuals who belong to societies and that people as individuals are socialized into conforming to the norms of society, expressed by way of cultural values. While there is obvious truth in this scheme, it is also obvious that it is greatly over-simplified. Conformity with norms, deviation from norms, and social control form one part of human social life, but creativity, innovation, conflict between norms, and patterns of variation in personal adaptation form the other side of the coin.

When we employ an expression like the above, 'the other side of the coin', we need to explain what we intend by it. The category of life called 'society' tends to be seen as emblematic of social norms, as opposed to deviance from such norms; but the reproduction of a viable way of life equally involves changes in these norms via strategies of survival or success in particular ecological circumstances, as Fredrik Barth pointed out in his studies of ethnicity and identity (Barth 1969, with contributors). Change and continuity not only do, but must, coexist in the wider context of ecological survival and dispersion of peoples. That is why we say they are two sides of the same coin, that coin being survival and the evolution of new forms over time, modified even further by chance, history, and contingent confluences of events.

Here, we are taking again a processual view. Processes tend to alter the terms of binary forms of identity or political struggles over these identities. An easy example to hand is the alteration in the People's Republic of China's politics towards families. A rigid one child policy was established to limit what was seen as the threat of population growth. Processes, however, negated this policy, which in any case ran deeply counter to more old established norms regarding the family. The Chinese industrial economy expanded, demographic aging set in, and from January 2016 the government began to officially allow couples to have two children, to produce enough workers to sustain the economy, pay taxes, and support retired persons with pensions or family leave.

At the level of norms and established ways of doing things, arguments about what is customary or traditional typically involve conflicts between social actors who have a history of factional disagreement. Custom then becomes embedded in politics, and since politics is about competition for power over resources, custom itself becomes an object of contention, as

we have noted. Hence, the very term that invites us to consider it as equivalent to continuity acquires a very different aspect of discontinuity when we place it in historical contexts. But this is also not the end of the matter, because discontinuity itself may conceal, or even deliberately reveal, aspects of continuity: a theme that we have explored in relation to the adoption of Christianity as an introduced religion among indigenous people around the world (e.g., Stewart and Strathern 2009).

At every point, then, when we inspect the categories of binary opposites such as custom versus change we find that the closer our look is fixed on processes as they actually occur the supposed dichotomy disappears. Ultimately, we will find that this observation applies to a series of such dichotomies, specifically custom/change, stability/innovation, individual/social, and nature/culture. In so far as these dichotomies have formed implicit or explicit frames for analysis, transcending them becomes important as a part of our 'breaking the frames' theme. Equally every breaking of a frame must imply the creation of a new frame, otherwise our exercise would simply be deconstructive rather than reconstructive.

At all stages of enquiry, also, it is necessary to remember that there is a long history of the application of concepts to social processes, and that to some extent we entrap ourselves in language in trying to rethink such concepts. The concepts are ideological in their reference and as such become objects of debate and rhetorical tools in conflicts. This applies to a related complex of terms that we are involved with here, such as 'self' and 'person'. Self is often opposed to person as individual is to society. Senses of self may be said to get to the heart of individual experience, yet they too may be said to be culturally inflected – as indeed they must be. Personhood is often, in turn, defined as a social and relational concept, yet if we think of 'personality' as an idea and what a 'person' is really like we inevitably find that persons vary and morph back into the individuals that as human complexes of identity and history they actually are. Normative statements about ideal personhood remain important but they are not the same as persons, or what Fredrik Barth, replying to a critique that pigeon-holed him under methodological individualism (Barth 2007) called, 'whole persons', the actual professed object of his analytical interest (see also his essays on person and society among the Swat Pathans, Barth 1981).

We will proceed now by considering some very thoughtful contributions to these classic debates, reminding ourselves at once about the innovative use of terms by Margaret Lock and Nancy Scheper-Hughes in

their article on 'the mindful body' (1987). The first thinker we will work with here is Anthony Cohen and his book '*Self Consciousness*' (1994). In the Preface to this book he announces a long-standing concern that gnaws away at the putative foundations of structural functional analysis and its holism, a direct descendant of the work of Emile Durkheim. Cohen points out that individuals may interpret very differently community-based symbols (p. 1). He goes further and notes that individuals are the focus of his research, and as such they often hold misunderstandings about themselves and others. These individuals in his field area in Shetland also felt it was problematic to generalize factors about them that would allow observers to construct them into communities. So, for Cohen, while individuals remained very palpably real, community and society were elusive, perhaps speculative. Cohen reaches a perceptive conclusion, akin to our figure of the two sides of a coin motif. He suggests that it is proper for anthropology to study 'society' (American anthropologists might have written 'culture' here, with the same import), but that we cannot do this in a sensitive fashion unless we take full account of individuals (p. x). Such a process of accounting must further depend, we may note, on what folk theories of self and person influence the ways in which people see themselves as individuals and what meanings they give to the term: for example, whether it implies autonomy of action or not. Awareness of the complexities engaged here can be a useful way to improve the scope of ethnographic accounts, as Cohen remarks (referring to Okely and Calloway 1992). Perhaps the most valuable overall part of Cohen's discussion is that he insists we do not deny to the people we study the self-consciousness that as anthropologists we claim for ourselves (p. 5).

In his discussion of these topics, Cohen is not only questioning practices of anthropological writing but he is also fundamentally questioning and breaking down the frames that had informed such writing. He is further turning problems inside out. Instead of taking it as axiomatic that humans are social he asks how it is that individuals as selves can be motivated to form social groups. He notes, significantly, the importance of mind and language as individuals use these resources to reflect on their actions and choices of action (p. 9). This is a fundamental observation which is worthy of much more unpacking. Clearly reflecting on his own experience, Cohen also writes that the conflicts between individual sensibility and social demands placed on the individual are often causes of anguish and difficulty. This is an entirely common sense remark and this existential condition is the stuff of almost all dramas and narratives. So

how anthropologists have sometimes managed to ignore the point or have claimed that in some remote part of the world (remote, that is, from the anthropologist's experience) such conflicts do not occur because 'the culture' does not include the idea of conflict between individual and group, is an interesting problem. We can only suggest that this and other tendencies derive from 'cultural determinism', the idea that humans are somehow both mentally and socially imprisoned in frameworks which nevertheless are human-made, even if they are claimed to be divinely sanctioned. Cohen points to an extreme example of this trend in the thought of Louis Dumont who argued that concern with the individual is an ethnocentric western notion, to be contrasted to the Indian concept of hierarchy. This, as Cohen notes, is also an extreme case of the dichotomizing tendency in social theory (we would add, primitive vs. civilized).

Cohen's reflections are at all points mindful. He is quick to say that he is not reinventing the 1960s wheel of methodological individualism, and to disavow any idea that he wishes to dispense altogether with the ideas of community and sociality. Rather, he is attempting to restore a balance in analysis in which the issue of individual/society interaction is not glossed over but built thoughtfully into ethnography. Incidentally, his remarks here indicate the swings of fashion in anthropology. A term that at one time may be a rally slogan for a particular viewpoint, such as methodological individualism, later falls out of fashion and an analyst with things to say that in part do converge with it has to categorically deny it. If we go back to I. C. Jarvie's effort to bring this approach in, we will find that his purpose was to focus on the individual effects of prophets as charismatic leaders in 'cargo cults' as a way of explaining the genesis and trajectory of these movements. Detailed work by Andrew Lattas (Lattas 2010) has, at a later point in time, strengthened this idea. But although the leaders Lattas studied are certainly very idiosyncratic individuals, they owe their influence to an ability to refashion motifs from folklore and tradition to suit their millennial messages. Without this field of tradition to work on, they could not exercise power over people's imaginations. Jarvie's approach on prophets was fruitful but insufficient on its own. So, as he employed the term, methodological individualism was a justifiable tack. It was abandoned later because it was interpreted as a grandiose total approach to phenomena, and so it could be used as a term of abuse to reject it. It is a disease of thought to elevate one's tools of analysis into total explanations. Theory is fine, as long as it does not turn into a tyranny and dogma, or as a ready to wear brand of clothing to place on top of any set of data. It often occurs

that this is exactly what happens to an innovative idea or concept that begins as a provocative twist on existing theory and ends up as a form of outworn and over-used dogma in itself, simply because too many followers and epigones have imported it holus-bolus into their accounts for want of seriously rethinking matters from their own field data in the way that Fredrik Barth did in every new study that he undertook.

The ethnography of the Papua New Guinea (PNG) Highlands is replete with examples of trying out versions of theory on areas not previously investigated in depth by professional anthropologists (although often highly informative and scholarly accounts were available, but in German or French rather than English). We will take here for consideration two topics that we have examined in previous publications, to illustrate our present theme of breaking the frames.

First, we will look at the constitution of groups. By any account, groups and networks are salient and important aspects of the lives of people in the PNG Highlands, falling within the general purview of kinship, marriage, and associative partnerships built on or modeled on kinship and affinity. Of this, there is no doubt, and it is probably as true today as it was in the 1950s and 1960s when Anglophone research workers began to undertake fieldwork in every available corner of the region. The major analytical question at this time, however, was: What were the principles on which these groups were established? Was there a rule of descent that primarily determined group membership? Separately, if there was a rule of descent, did this have to be unilineal descent or could it be cognatic descent of a type found elsewhere in the Pacific? Anthropologists had found that unilineal descent was important in some African cases, or was said to be so, notably among the Tallensi studied by Meyer Fortes in Ghana (Fortes 1945) and the Nuer studied by Evans-Pritchard in the Sudan (Evans-Pritchard 1940). 'Descent', whether unilineal or cognatic in character, is a typological concept, not a concept of process. It suggests that rules and structures are in play, and this in turn suggests that these might not always be followed, if circumstances so determine. The concept of a rule is also variable in its import. How rigid or important is the rule, how central to social values? For the Tallensi, in Fortes's account, unilineal descent was of prime importance at all social levels and formed the basis of authority, leadership, and ancestral worship. Tallensi were heavily populated agriculturalists, for whom land near to their compounds was especially important. For the Nuer, a different pattern emerges. The Nuer were pastoralists, depending on their cattle and moving around with them. They belonged

to ramifying segmented groups that maintained claims to resources, and kept extended ties of kinship and descent to enable them to do so. Their tribes were political blocs within which there was some recognition of payment for killings in fighting. Age-set organization held together wider sets of youths and adults across segmentary divisions. With both Tallensi and Nuer, however, the segmentary organization of groups was important.

When anthropologist trained to be acquainted with the Tallensi and Nuer cases (regardless of their highly significant differences) encountered the New Guinea Highlands social groups, their first attempt was to fit them into a unilineal model of descent. Discrepancies soon appeared, on two fronts. First, if there was a unilineal rule, why was it often enough not followed? Second, what were analysts to make of at least one case, the Huli, who appeared to have cognatic or ambilineal descent?

An important clue to this situation might have been found in the Nuer case, where it was obvious that a unilineal ideology permeated the segmentary political structure of groups, but at the level of residence and local affiliation people might belong with their mother's people, or they might be in origin members of another set of persons, the Dinka, drawn into the Nuer groups through conquest and assimilation (an ironic point given the contemporary state-wide struggles in South Sudan between Nuer and Dinka factions). Unilineal at one level and not at another, then? Because descent was sometimes taken as a total system typology, it had to stand or fall as a descriptive device on whether it was consistently followed. This, however, was a mistake. Equally, it was a mistake to suppose that if it did not operate in this way, it was somehow a mirage, as John Barnes suggested (Barnes 1962).

A way out of the descriptive impasse had to be found. Two ways were available. One pertained to the idea of descent itself, or to the dogma of descent, as Barnes (1962) put it. What is a dogma for? It is to maintain a certain valued ideology. In some New Guinea Highlands cases there was indeed such an ideology applying across the board of social groups, but whereas it was applied unequivocally to relations between political segments of groups and ensured their solidarity it was not applied to the actual level of recruitment to these groups. As one of us (AJS) first worked out in 1965, the rule of recruitment in the Hagen groups of the Dei Council area at this time in history was by filiation, not by descent (Strathern 1972). This was a major turn of analysis, made possible only by breaking the frame of descent as a total model. Naturally, it raised

opposition, but it was solidly based on empirical data. Another way out was also available, and in this respect too the Hagen data were crucial. Hagen society depended as much on the elaboration of exchange relationships, based on marriages between exogamous clan groups, as on the rules for the internal constitution of the groups. Affines as exchange partners were important, and affinal ties resulted in extensive matrilateral ties. So, if there was a shortage of people in a group, it was an easy step to incorporate sister's children into one's group. Finally, here, the possibility to recruit in this way helped leading men to maximize their abilities to command resources and the allegiances of kin within their group. The most important thing was not any rule of descent but the ability to mobilize resources. Demography and personal ambitions conspired together to produce a hybrid social structure, compounded out of a descent-based ideology, the importance of exchange, and recourse to filiation as a way of getting people into one's group. In other words, if we turn from rules to processes, we will find here that a combination of descent at the political level with affinity at the level of social exchange transformed itself further through the importance of wealth transfer into the sort of 'big-man' kind of achieved leadership and network-based prestige that ethnographers were variously describing.

Cognatic descent is another story. The Hagen case is not made one of cognatic descent by the mere fact of recruitment into groups via either the father or the mother (and indeed others are also brought in as orphans or adoptees). The rule of recruitment is filiative, not by descent. Children brought in to their mother's group belong to it by maternal filiation. If they are male, their children in turn are called 'man-bearing', just as agnates by descent are, and filiation is swallowed by descent over two generations.

There are, however, cases where this does not hold. As we have examined extensively for Duna-speaking groups in the Aluni area, true cognatic descent operates where extensive matrilateral as well as patrilateral ties continuously feed into local group identities, fixed by ideas of home territory or ground (*rindi*), and group genealogies record this. Duna social structure is on the model of Huli structure described early on by Robert Glasse (1968). Glasse's innovative account was incorporated into the comparative ethnography largely as an outlier, but if we consider social structure from a generative point of view as Fredrik Barth did, it is evident that a cognatic structure can emerge out of an extensive application of bilateral recruitment rules. Cumulative filiation might as well be bilateral

as unilateral in form. However, this is not the whole story, as both the Hagen case and the Duna case show in their different ways. In Hagen descent reasserts itself in the segmentary constitution of political groups. Among the Duna each local group or *rindi* has an internal 'backbone' through the precedence granted to a single senior agnatic line within the *rindi*. This line is connected to the putative founder of the group, often described as a powerful outsider with extraordinary abilities who came to the land and brought his strength and creativity to it. So, for the Duna, a dogma of (agnatic) descent reasserts itself from within the milieu of cognatic relations. And for Hagen a similar dogma of agnatic descent is imprinted on a variable set of bilateral ties of recruitment. Agnation and cognation are pivoted together – as indeed they were among the Nuer studied long ago by Evans-Pritchard. Instead of there being an 'African mirage' that had to be dispelled in order to understand the New Guinea Highlands we find instead that an aspect of social reality stretches across the world, notably between Hagen and the Nuer. A frame has been broken only to be remade when we look at the underlying processes at work.

Now, what has all this got to do with individuals? We return now to the topic of this chapter as a demonstration of how innovations turn into overworked models and mindful anthropology is thereby lost. First, if there is one point that consistently emerges from the New Guinea Highlands ethnography, it is that choices made by individuals enter significantly into social patterns. Variations in group composition (in contrast to relative stability in group ideology) are all products of choice. Exchange networks, while guided by kinship and affinity, are also products of choice. Choices that leaders make, and their strategies of self-development, are equally a product of their choices and creative ability. People choose where to reside. The choices of women about whom to marry enter in. Among a set of brothers one may distinguish himself as a leader beyond the others. In Hagen all such variations are recognized and expounded via the concept of the *noman*, the 'mind' of the speaker. Persons are therefore seen as mindful, conscious agents in the sense put forward by Anthony Cohen in his general discussion. Bringing together persons of different *noman* into a state of agreement is recognized as an art of leadership and a product of discussion. Again, this is like Cohen's viewpoint, that the creation of consensus is difficult; and to Cohen's observations (influenced perhaps by his experiences in academic administration) we can add now the element of leadership added to that of personal choice. Suddenly we are

also back here, in a certain way, with Fredrik Barth and the Swat Pathans. Neither descent alone nor exchange alone ('the gift') explains how New Guinea Highlands social life works. It is human choices, choices made by individuals, that turn out to be crucial, as Paul Sillitoe has consistently (indeed, insistently) argued for the Wola people of the Southern Highlands Province (e.g., Sillitoe 1979).

So we have individuals, and as Sillitoe has also insisted we have individuals of either (or any) gender. Both women and men are seen as having minds and as potentially autonomous persons. The earliest Anglophone anthropologists in the Highlands were all struck by the individuality of the people they got to know. Indeed this characteristic was built into the work of applied social scientists (notably Ben Finney) who saw how readily some persons took to entrepreneurial activities once cash cropping and business opportunities were introduced by the Australian colonial administration. Of course, this stress on entrepreneurship would also have to be balanced against the reentry of entrepreneurs into the production of social capital through their pouring of resources into communal exchanges. But in the event the stress on individuality was overtaken by a phenomenon called the New Melanesian Ethnography in which individuals were deconstructed back into a world of Durkheimian-style 'sociality' and transformed by the magic wand of the post-structural imagination into an entity called 'dividuals', following work by McKim Marriot on (the very different) societies of South India. Dividuals carry society within them. They are already society. There is therefore no true individuality. Dividuals are simply a part of society (or culture, if you wish). They have no ontological basis outside of this context of society.

The suggestion that we look on New Guinea Highlanders as 'dividuals' was an innovative twist in its time.

We reviewed this topic and commented on it in earlier publications, for example in our edited volume *Identity Work* (Stewart and Strathern 2000b). The original formulation on the theme was couched in a sophisticated way, recognizing both individuality and dividuality. This was not particularly surprising, since it is patently clear that in all social contexts there is a tension between individual wishes and a countervailing desire to conform to custom. This was not, however, what the formulation meant, because the intention was to focus on indigenous concepts and ideas, and to foreground the hypothesized 'dividual' aspects of these. But what was the dividual supposed to be, other than a person who carried within himself or herself an awareness of being a part of a wider society?

Anthony Cohen, reflecting on the category of the self, argued that the self is multiple, containing different identities. For Cohen, individuals have selves that are complex and contain different aspects of sociality. It emerges that Cohen's self/individual is actually like a 'dividual'. What, then, was the point of setting up the concept of the dividual as opposed to the idea of the individual instead of in conjunction with it? Probably because the aim was to take a new twist in theorizing and set a new and simplified trend on its basis. And this is what happened. Instead of a balance between individual and relational (or 'dividual') aspects of behavior, the dividual took over. Ethnographers had stressed individuality too much. So now they had to talk about dividuals instead. The process became absurd. From being a new idea, the categorization of people as dividuals rapidly turned into a dogma, or a naturalized supposition of ontological truth, rather than a piece of rhetoric, as it in fact became in its dichotomized form.

Entering this arena with the aim of reintroducing a modicum of balance, we suggested that what we are dealing with in all our ethnographic observations is 'relational–individuals' – individuals who are nevertheless tied in to one another by relationships, which they variously respect, manipulate, refashion, conform to, and the like. Cultural relativism and the theoretical cult of difference may lead analysts to deny or forget this substratum of social life in favor of current orthodoxies and essentializations; but, to adapt the Latin tag, *naturam* (or *personam*) *expelles furca, tamen usque recurret* (drive out nature (or, the person) with a pitchfork but it will always come back).

The academic argument was more about personhood as a concept in indigenous thought than about whether people behaved as individuals or dividuals (how *does* a dividual behave, in any case?). Personhood became established as one of the things ethnographers needed to understand, and personhood represents the actors' ideals of how a life should be conducted. It is a concept of value, or ideology in the broad sense. Understanding such ideals is a completely valid aim of fieldwork; but when juxtaposed with the debate about individuals versus dividuals, the outcome can be confused. If we concentrate on the dividual, it is easy enough to equate this dimension with personhood. However, this is illusory, because the ideals of personhood may equally recognize values of individuality.

A common feature of discourse in numbers of the societies or languages of the Papua New Guinea Highlands is a concept that has as a core part of

its significance 'mind'. A person for Hageners, for example, is someone who has a mind (*noman petem*) and is mindful (*noman-oronga pitim*). But, as we have pointed out in other connections, in practical usage the idea of the *noman* can be deployed to index all states of mind, from those most individually oriented to those most collectively oriented. *Noman*, in other words, is a holistic concept that realistically embodies what Anthony Cohen referred to as the complexity of the self as a general point. Hageners, therefore, signal in their *noman* concept what we have called the relational–individual, or we might as well call 'the social person'. It follows that the dividual versus individual dichotomy is a non-issue, and is in fact merely a product of various persistent trends and biases in sociological and culturalist theory.

How, then, can we account for the phenomenon that occurred in Melanesian studies of the person, that where there had been individuals as social actors, now there were simply dividuals? A semantic transposition occurred. How did this shift of perspective occur? We have to ask now, how does academic knowledge or what passes for it, get produced and reproduced? The suggestion about dividuals was an act of breaking frames, and the suggestion itself was progressively simplified by eliminating all forms of complexity and replacing these with an essentialized dichotomy. In one of his insightful and reflective essays, Fredrik Barth explored an anthropology of knowledge, in which he distinguished between semantics (or substantive assertions), communication, and the production of knowledge via social relations (Barth 2002:3). In academia, as in other human arenas of competition, the production of knowledge is determined by social and institutional power and the effects of mimesis linked to power. If we ask again, then, how could a semantic volte-face have occurred in the Melanesian ethnography about persons as individuals as against dividuals, two answers are at hand. One is that fashions are very important in academia, and just as in clothes fashions for women long skirts may be replaced with short ones, so in terms of concepts if a familiar way of labeling phenomena is eclipsed by a new one, it is often done by replacing it with its polar opposite. And with concepts as arguably with clothes, whole philosophies of being are tied up in them. Hence shifts tend not to be partial or mediated, but holistic and essentialized. Before there were individuals. Now there must be dividuals. But in this process of upending and reversing concepts ethnographic accuracy and plausibility are sacrificed. One possible reason why our own concept of the relational–individual did not greatly catch on was that it was altogether too realistic and middle of the road to attract a set of cult followers who could

build their own careers on it as a new fashion. With individuals, the problem is how can their actions result in social order? With dividuals, the opposite occurs: how can conflict, violence, and social disorder arise? In effect, since order and disorder alternate or are intermixed, we are left with the likelihood that the combination of relationality and individuality can easily have a variety of outcomes, ranging from order to disorder. And this is what we tend to observe in reality. Hence recognition of the dialectical ontology implicit in the concept of the relational-individual is what is needed, whether this can be used for academic advancement or not.

Some anthropologists, per contra, have built their careers on a defiant insistence on the importance of the individual in society, culture, and history. One of the most deft exponents of this viewpoint is Nigel Rapport (see, e.g., Rapport 1997, 2003). In terms of the production of knowledge we can trace a direct line from Anthony Cohen to Nigel Rapport, especially since they have actually collaborated in their work (e.g., in the volume *Questions of Consciousness*). In the work of Cohen that we have noted earlier, we see his reaction against over-collectivist interpretations of human behavior. Cohen, nevertheless, recognized that the main aim of anthropology was not simply to examine what individuals do but to build understandings of how social life is achieved. Rapport takes the stance a step further, and dwells on the importance of the individual in creating, molding, and transforming social life, and he bases his work on the idea that the individual is in all cases ontologically transcendent and society is an abstraction, whereas individuals are real. All knowledge and agency is fundamentally held by individuals, so they must be the focus of our study. These are fighting words. However, society or social groups are not *just* abstractions, and ideas of individuality are themselves in a certain sense abstractions. We return, then, to the relational–individual as a unit of description and analysis. This concept represented a refusal to adopt a dichotomous framing of issues in relation to social personhood. It was intended to break a false opposition between individual and dividual. Underlying this unreal opposition there was a supposition that 'individual' means 'natural' and dividual means 'cultural'. The old nature/culture division therefore seems to be lurking at the base of this debate as well as many others. We will take up this topic next.

CHAPTER 5

Nature Versus Culture: A Mistaken Conundrum

Abstract What constitutes humanity? Is it nature or culture? Culturalists like David Schneider or Marshall Sahlins stress culture, whereas biologically inclined theorists stress nature. The distinction is itself based in older, religious dichotomies between body and spirit. Claude Levi-Strauss used it in his theory of the evolution of marriage practices, and it figures into arguments about biological or cultural features of kinship ties. However, kinship is an intrinsic amalgamation of nature and culture, and it is important to see it as a fusion of these two, thus breaking the frame of dichotomous thinking. Since the body/mind distinction underlies the kinship debates, it is useful to note that the concept of mental versus physical health dichotomizes life processes that must be recognized as embodied and holistic.

Keywords Biology · Body/mind · Claude Lévi-Strauss · Fusion · Humanity · Kinship

This opposition between nature and culture is at the root of discussion of what it means to be human. Does nature make us human or does culture do this? Anthropologists who espouse the culturalist approach in anthropology, such as the two notable figures of David Schneider and Marshall Sahlins, would answer unequivocally that culture is what constitutes humanity. Anthropologists who by contrast are attracted by biologically

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based viewpoints will give the opposite judgments. The opposition itself is a product of folk culture that in Europe is based on an older distinction between body (nature) and spirit (culture). Many times we find in anthropology that analytical concepts rest on older folk concepts and prove difficult to disentangle from these. The problem is compounded by the fact that the nature/culture opposition is almost like a founding myth of human sociality, placing humans beyond nature by virtue of their creation of culture (along with something that often stands as the proxy for culture, language).

We will argue here, perhaps not surprisingly, that this opposition has a stultifying effect on attempts to bring together 'culturalist' and 'naturalist' arguments about human life patterns. There have been, of course, brilliant pieces of work that remain founded in it; most notably Claude Lévi-Strauss's early work on kinship and marriage (Lévi-Strauss 1969) in which he saw the prohibition of incest and the institution of forms of marriage as among the elementary manifestations of human culture. For Lévi-Strauss also cousin marriage was the sign of this elementary development of culture because cousins are the next close category of kin to siblings. If sibling marriage is prohibited, cousin marriage is the next step away from it, according to this argument. This elementary move, he argued, also produced the phenomenon of the importance of the mother's brother in many kinship systems. The mother's brother, according to this viewpoint, represents the creation of marriage ties and the production of marital alliance, projected into the next generation after children are born of the marriage. Lévi-Strauss's finding here was intriguing, and it stands whether we accept his theory that kinship ties began as a result of a cultural prohibition on incest. The mother's brother is indeed an interesting figure and has attracted interpretation from Radcliffe-Brown and much later Maurice Bloch, as well as many fieldworkers in the Papua New Guinea Highlands (see, e.g., Strathern and Stewart 2011, with references). In the case of marriage (from the male viewpoint) with mother's brother's daughter, the mother's brother figure becomes particularly important, as a kinsman who also is a potential affine and bridegiver: again, a representative of the intersection and transition between nature and culture; or, otherwise put, between in-group and out-group relationships.

The foundational myth therefore stands behind Lévi-Strauss's spirited suggestions. We can, however, accept the ethnographic significance of his work without adding the philosophical and evolutionary elements he adds to the ethnography of cousin marriage systems. Cousin marriages

do set up networks of alliance that are narrower in focus than exogamous marriage to non-kin among a range of groups. We do not know, however, whether cousin marriage was an original rule-governed form of marriage in general rather than an outcome of the prevalence of small-scale group life. And the implications of this form of marriage vary greatly between cases where the immediate cousin is married, often for reasons of the consolidation of property, and those where the marriageable pool is a category of persons all classified as a particular kind of cousin in a context of ongoing alliance between groups. As these cases are so different, a single explanation cannot hold for them. This brings us, in turn, to a problem that has been with anthropology ever since Lewis Henry Morgan's work on kinship terminologies. Morgan (1871) distinguished between descriptive and classificatory terms, thereby setting the tone for the basic categorizations of kinship semantics for a long time, in addition to setting up a typology of systems based on his samples of terms from ethnographic accounts that happened to be available to him at the time, for example, the category 'Crow-Omaha' derived from Native American cases.

Rethinking this early distinction between descriptive and classificatory/kin terms, we may notice some confusion in it. All kinship terms are forms of classification, including those that Morgan dubbed 'descriptive'. Equally, classificatory terms are also forms of description.

The question arises here, descriptive or classificatory of what? If the basis, or cross-cultural putative foundation, of kinship ties is considered to be the biological processes of procreation, then descriptive terms should be primarily descriptions that refer to these processes. 'Descriptive' as a term tends to appeal to such a notion. The idea is that all descriptive terms can be broken down, or parsed, into an etic grid of genealogical links. These suppositions are derived from a systematization of folk concepts enshrined in the English language (or languages with the same or very similar folk concepts), so at heart the problem is one of translation between different languages. If, in the English language, the terms brother and sister carry the implication that those so described share one, or two, biological parents through procreation, then we can build up kin types based upon genealogical readings and marriage ties. Thus, for example, the term aunt can be defined by the following types: mother's sister (MZ), father's sister (FZ), but also mother's brother's wife (MBW) and father's brother's wife (FBW). While the translation into symbols renders these points unambiguously and thus brings them under the purview of steps of procreation and

marriage as their basic building blocks, this etic notation passes over the issue of classifications by reducing the description to elementary pieces. If we ask instead why it is that such diverse types are actually all lumped together under a single term, aunt, then we find ourselves thrown into the recurrent conundrum of what do kinship terms really mean? In this instance the term tells us (1) that no lateral distinction is made between kin on mother's versus father's side and (2) the distinction between affinal and consanguineal ties is also overridden. The first point is in line with the bilateral character of kinship terms in general in this particular system. The second point shows, further, that although there is in practice a strong distinction between affines and consanguineal kin – an idea that is itself founded on the implication that affines are not also consanguines, for example, not cousins, in the terminology this distinction is at least partly blurred. This is done by making affinal terms hybrids of consanguineal ones, so that husband's or wife's parents become parents-in-law. This distinction is further blurred in folk usages by the conventional aphorism that the in-marrying affine is now like a consanguine, son-in-law for example becoming a 'son', and daughter-in-law a 'daughter'. These are rhetorical claims that attempt to overcome what is in practice also a powerful difference found in other domains of folk attitudes, values, and actions, for example, from a male viewpoint between mother and mother-in-law (and the same for female speakers). The basic aim of the rhetoric is to assimilate affinity into the realm of consanguinity, at least in behavioral terms. The exercise is partial and incomplete because there are in fact fundamental differences that impede such an assimilation. The terminology can thus be seen as a kind of ideological construction, built up out of the notion that David Schneider pointed to: the opposition between 'law' and 'nature', where law stands in for culture in general. Schneider was identifying a nature versus culture motif or trope that informs the much so-called Western thinking. He was also arguing that what in such thinking is identified as natural – and by implication also universal – is in fact culturally particular and by no means universal.

Several further points are easy enough to make here. The first is that ideas of procreation itself can vary considerably across cultures. In other words, 'nature', or 'biology' in this case, is itself a construct, not an immutable fact. Second, contrary to this first point, there is nevertheless across cultures a widespread recognition that sexual intercourse has a vital role to play in reproduction. Even in cases where the connection is denied at some ideological level, as putatively among the Trobrianders of Papua

New Guinea or Schneider's Yapese informants, at another level it is recognized (see discussions in Strathern and Stewart 2011). If this were not so, we would not expect to see such strong rules governing sexual activity and linking it to marriage and the legitimacy of children born to a marriage. Even in the Trobriand case, where maternal ties predominate, patrifiliation is also recognized as significant and linked to marital ties. Third, then, what we find is an underlying substratum of ideas founded on the human species' reliance on sexual reproduction, coupled with numerous variations in interpretations of this fact and the cultural elaborations built up from it. From a holistic viewpoint, we must embrace both sides of the argument. What we call 'kinship' is a product of the fusion of this substratum with numerous systemic differences. In this process of fusion nature becomes culturalized and culture is naturalized. It is therefore unproductive to pick apart this holistic web of signifiers and dissolve it back either into nature or into culture, or to argue that one or other of these categories is transcendent. Kinship is the intrinsic amalgamation of nature and culture in the processes of embodied living and adaptation. Further, what we call biology should not be narrowly linked to genetics or conception or birthing. Nurturance and care of offspring are obviously as important as any other factors, and these features are also obviously linked to species-wide patterns of survival based on the need for long periods of growth and socialization of humans. 'Children' as a symbolically loaded social category are the markers of this selectionist feature of human sociality.

Finally, here, the insistence on the special imperatives of 'nature', whether focused on genetic ties, birthing, or caregiving, can be seen as another evolutionary feature. If nature is seen as providing a self-explanatory imperative, then it will work as a motivation for individuals with or without recourse to further sources of value of values placed on life. Thus nature in this sense will always be incorporated into culture, if we see culture also primarily as an adaptive force. Nothing, however, proceeds out of nothing, and without a background substratum of processes, ideological elaborations could not work. The upshot of this discussion is that the perennial debate between extensionist and constructionist themes of kinship is to be resolved not by proving one side right and the other wrong but by stepping back and seeing that both are right in their own terms. What we call kin terms do recognize 'extensions', distinguishing immediate/'real' from classificatory/extended ties. But the extensions are just as real as the immediate relationships in building systems of social organization. The plasticity of human imagination enables actors to play these themes either way. So, for example,

in Mount Hagen in the PNG Highlands where ties with the mother's brother are important immediate genealogical ties between cross-cousins can be recognized as primary, or alternatively classificatory cross-cousins may be seen as 'true' kin, because of the practical obligations they take up and fulfill. Recognition is contextual. Language usages point us to the recognition of immediate ties as prototypical or 'true'. Another set of usages point us to the recognition of a performative bias that can supersede the immediate ties. Whoever performs the ideal role becomes the 'real' kinsperson. But the transposition does not work with indefinite flexibility. It has to work via classificatory rules that shape it, and these too are enshrined in language usages. In the 'kinship wars', then, within anthropology, we must affirm again that both sides were right in that they drew attention to real phenomena; yet both sides were wrong in that they subscribed to a dichotomous mode of thinking that stipulates truth as belonging to only one side of the debate. Dichotomous thinking is certainly a part of the human repertoire of thought and is often marked in ritual contexts. However, from an analytical viewpoint, much can be gained by refusing to adopt this tendency, and instead looking beyond it to an integrated view that embraces both sides of a debate but does so on different grounds from those of the opponents. In this case we have sought to break the frames of an adversarial debate by expanding the idea of truth to a broader level of understanding. Theory thus becomes synoptic and avoids the myopia of a fight between frames (exactly as it proved necessary to avoid dichotomizing the 'individual' versus 'dividual' debate and for analogous reasons).

One sphere of enquiry which is beset by problems of definition stemming from the same dichotomous thinking about nature and culture that we have been probing here is in the sphere of what is called 'mental health'. This concept depends on a separation between mind and body.

Mental health is a well-established category in the biomedical system, marking off the mental from the bodily spheres. It is the division, in fact, between mind and body that represents both a tenacious tendency of thought and an error in trying to understand the phenomena themselves. Cross-cultural evidence shows two things: First, an absolute difference or ontological separation between body and mind is rarely posited in folk cultures, by contrast with the ideas of the philosopher Descartes. Second, some sort of distinction is generally made, but the relationship of the body and the mind is seen as intimate and close, so they constantly influence each other within the same order of being. Cross-culturally, then, the categorical separation of mind and body in the Cartesian scheme can be

dissolved against the background of viewpoints from other cultures. Still, the idea that mental health is a separate category of experience and enquiry persists.

Why should this be so? Biomedicine's own materialist basis leads to this situation, ensuring that mental and physical health are conceptually separated so that 'mental health' emerges as a category. In practice, however, biomedicine reduces all health conditions to a physical or materialistic basis, because the treatments that are prescribed are drugs that are aimed at altering physical aspects of the person, symbolically located in the 'head' or 'brain'. Therefore, there is no 'mind' that is separate from the 'body', and the category of mental health should not have any basis. The reason why it is still a part of the scheme of things is that it is still symbolically set apart from other conditions, for two reasons, one pragmatic, the other belonging to culture and history. The pragmatic reason is that mental health conditions are recognized to be complex and hard to treat effectively. The other is that 'the mind' has an overwhelming presence in literary texts and in the imagination of people. "It's all in the mind" is a commonplace saying based on the idea that the mind is another world, akin to the poet Gerard Manley Hopkins's idea that "Oh, the mind, the mind has mountains." Also, the expression "Nothing is good or bad in this world, but thinking makes it so" (paraphrasing Shakespeare's Hamlet, Act 2, Scene 2) assigns to thought, that is, mind, a paramount role in the production of meaning in the world.

It is only a short step from this point to an assertion that thinking produces culture, and culture reciprocally produces thinking, and we are once again in the world of mentalities. Mentalities in turn are rendered mysterious because of the perceived elasticity of the imagination. All these observations are culturally encoded cues that carry much experiential force. But, according to the way this matter is seen in New Guinea contexts, everything still remains embodied.

So for the Mount Hagen people of PNG *noman* or 'mind' is a real force in life. It is imaged as located not in the head but in the chest, in a central position in the body. It is conceptually there, but would not be found if you cut up the body and looked for it. It is a category of experience and feeling, not a category of anatomy. Therefore, it corresponds neither to a biomedical concept nor to a culturalist way of seeing mind as separate from the body. Whatever the *noman* is for the Melpa speakers of Mount Hagen, it is not the part of the person that turns into the *min* or life spirit, after physical death, even though the *min* is represented as operating

rather like a disembodied *noman* of the dead person, that is, it has agency and desires and feelings as shown in dreams which the living have.

So, in the Melpa way of thinking, *noman* corresponds neither to something fully material like a body part nor to something fully spiritual like a soul. Instead, as an embodied notion, it partakes in elements of both a material and a spiritual character and so mediates the body/mind distinction in its own distinctive way. As a concept *noman* is a frame-breaker and a frame re-maker, in a fashion that replicates in micro our argument in macro in this book as a whole.

Returning to the category of mental health, we suggest that a definitional stance closer to the Melpa sense of *noman* would be useful in trying to think of forms of therapy, situating the therapy in social practice and its management as well as in biomedical treatment based on substances and their application.

Putting this in the terms we began with, the mistaken opposition or division between nature and culture, we can suggest that in a biomedical worldview the body is seen as natural and therefore universal, whereas mind is seen as cultural and variable. Biomedicine then attempts to reduce this mysterious mind to the material, to the body, by the use of physical treatments. There is no doubt that such treatments can have efficacy, but they are also potentially risky and inconclusive. Science will continue to improve our knowledge of materiality and treatments that can be applied to it. But efficacy is more likely to be achieved by a combinatory approach, linking treatment not simply to an ethereal notion of culture but to praxis, social practice, as the Melpa do with issues involving *noman*. In proposing a mindful anthropology, then, we could also put it this way: we propose an anthropology that takes seriously and incorporates into itself indigenous philosophies as a means of enriching our own interpretative ways of dealing with the world.

CHAPTER 6

Retreat of the Social? Where to?

Abstract Bruce Kapferer, and contributors to his edited volume *Retreat of the Social*, sees a danger for anthropology in an emphasis on individual agencies rather than social processes. Such an emphasis he sees as reductionist. He includes neoliberalism here, pointing out that it is used as an ideology. Other contributors to this volume include: Christopher Taylor, who discusses the cosmological power of Rwandan kings; Roger Just, who notes that foregrounding the individual raises the problem of what to do with society and culture, suggesting that we should focus on creativity; and Thomas Ernst, who again warns against reducing social life to individual action. However, the individual and the social should not be seen in closed dichotomous terms, but as open and mutually constituting relationships.

Keywords Bruce Kapferer · Creativity · Individual · Neoliberalism · Reductionism · Rwanda

Bruce Kapferer, in his Introduction to the volume *The Retreat of the Social* (2005), puts a problem squarely: he sees an individualist and subjectivist turn in anthropology, which for him, and others, means a turning away from institutional structures and social processes in favor of concentrating on individual agencies and strategies (Kapferer 2005:2). Kapferer sees in these moves a trend of "the vacating of the social" (p. 3), and an opening

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of the door to reductionism. He instances 'economism' as an example, theory that relates everything in the world to economic notions of a certain kind. We would add here 'neoliberalism', a version of economism that relates all actions in late capitalist society to individualistic profit-seeking. Kapferer adds other categories, for example, 'gender', as an abstraction removed from the particularities of process and action. He hits out at the ignorance of history that may lie behind reductionist generalizations (p. 5). Basically, he is opposed to any forms of reduction that ignore complexity. He suggests that the theory of neoliberalism itself opens the way to all forms of reductionist thinking, usefully appropriated for their own purposes by state authorities. We may cite here one example from University practices. 'Going green' or paperless is trumpeted as a great improvement, yet often it is not, because the individual academics may have to print out on paper which they themselves buy documents that are sent digitally by the University authorities. So going green is cheap for some, expensive for others.

Kapferer is hitting at numbers of targets here. His general argument is that an inappropriate stress on individual action lends itself to reductionist explanations amid the emptying out of the category of the social. We see here a number of points. First, the observation about abstractions such as neoliberalism taking the place of more complex social processes as explanations is well taken. It is high time such pretenses at explanation were thrown out and a genuine attempt to restore complexity to analysis was set in hand. But Kapferer goes further and shrewdly points to the fact that states, which are institutions not individuals, adopt the theory of neoliberalism for their own purposes. This means, in effect, that he is saying there is a social process of adopting the ideology of neoliberalism for institutional advantages. Neoliberalism then does not explain anything, because it itself is used as an instrument for social action. Kapferer has thus found his own way out of the impasse he describes, because neoliberalism is now not an analytical explanation but is an ideological phenomenon embedded in structures that require other means of explanation. Scholars who attempt to put it forward as a form of explanation are suffering from misrecognition in Bourdieu's terms.

The other point we wish to make here is that not all ideas of the individual give the individual autonomy as against the realm of the social. Our own concept of the relational–individual (see above in this book) situates the individual in a complex web of autonomy versus relationality. Hence, Kapferer's own argument addresses only one extreme case of emphasis on

the individual. Other definitions incorporate the social and so obviate his general argument.

In his contribution to the volume Christopher Taylor (2005) takes up another aspect of the debate. He notes the rejection of culture as a monolithic explanation of things, going on to say that culture has tended to be replaced by power as the explanation of everything and the desire for power as the prime mover of human motivation: certainly both a reduction in Kapferer's terms and a reification, as Taylor points out, with the accompanying proposition that the desire to maximize power underlies all human action. (At the risk of turning a general argument into a particular one, we suggest on the basis of long-term participant fieldwork in academic departments of anthropology that the proposition does fit well enough the behavior of many Chairs of Departments in Universities, who tend to justify their actions by arguing that they are doing everything 'for the good of the whole'.) Taylor contests the universality of this model of individual power maximization and offers in support of his view a brief reference to the traditional power of the King in Rwanda, which was based on the royal ability to conform successfully to a cosmological scheme in which his body and person ritually facilitated flows of fertility for the community or society which he served. The king's cosmological power was not his individually. It was a collective ideal that he had to live up to. The Rwandan kingship here stands in for the cosmos, and cosmos for culture. Individuals might be found wanting in relation to this collective (social) ideal. Power, therefore, may be individual or collective. It is not one thing. Indeed, we may observe, it is not a thing at all but a process of becoming and waxing/waning. So, for Taylor, cosmology stands in for both the cultural and the social realms. Cosmology provides a signature of holism on social processes.

Taylor's aim here is like that of many other contributors to this book, to bring society back into the picture. One of the most accessible studies in the collection is by Roger Just (2005). Just carefully lays out a scaffolding of arguments. He notes that the concept of 'the individual' has come to the forefront of analysis, and in its universalizing form is linked to the old idea of the psychic unity of mankind (Just, p. 59), as well as to a kind of shift away from the emphasis on the collective. As Just notes (p. 62), the consequence is that "anthropology's problem is now what to do with society and culture".

What, indeed? Just further notes that evolutionary psychology obviates this problem by jumping to the putative universal individual. Another

solution, he notes, is not to obliterate but at least to sideline culture as a kind of background or milieu (p. 62). Another, more dynamic, solution is to recognize, as we ourselves would advocate, that just as organisms exist in and with their environment, so do persons within their cultural contexts. Just reminds us (p. 64) of the shadow of functionalism in anthropology. Functionalism in an extreme form suggested that every activity contributed to the promotion of the social whole. As this is not universally true, what are we left with? Just does not provide a forthright answer, but he offers a glimmer in the distance before us by glancing at the importance of creativity in social life (p. 65). Certainly, if we recognize this point, it provides a means of achieving insight into dynamism in social life, while avoiding the overdetermination of either a totalizing functionalism or a detotalizing and reductionist evolutionary psychology.

The last of the chapters we will consider here is by Thomas Ernst (2005). It is a thoughtful piece. Like all the contributors, Ernst is against something called reductionism, however defined. For the contributors, reductionism means the reduction of the social to the individual, function to agency, custom to negotiated meanings. Ernst refers to Marshall Sahlins's well-known stance of emphasizing culture as distinct from any universal "practical reason" (p. 73), and his refusal to reduce culture to anything but itself. For culture read society or structure in other people's formulations. Ernst also sees individuals as always encapsulated in their cultures, including those individuals who emerge as such in the context of capitalism. Capitalism is also a culture.

But, whichever way we push for the two supposed oppositions, the individual or the collective, we will be forced to ignore one side. The frame that radically separates the category of the individual from the collective, he says, renders mediation impossible. Curiously, however, all the contributors want to stress 'the social' in this book, and bemoan that it is being vacated. But none of them actually tackles what it is. They discuss instead what it is not, i.e., that it is not the individual or a collection of individuals. (Incidentally, Radcliffe-Brown had no problem with seeing groups as constituted by individuals.) What is it, then? The impasse comes from the frame itself. Individual and social or collective or society or group are not to be defined in themselves or in isolation. They are to be defined in relational, often dialectical, terms. We return to our concept of the relational–individual, which attracted almost no notice by contrast with the borrowed concept of the 'dividual' firmly fastened onto the analysis of personhood in the New Guinea Highlands societies. The implications of

our concept were actually profound. There are no individuals outside of social relations, but social relations always take place between individuals. In this way we can build concepts of groups without invoking a transcendence of culture or a bedrock of evolutionary psychology, and can get on with understanding what is always in front of us, people in conflict and in co-operation with one another. And Roger Just provided a spark with his recognition of creativity – the capacity based on imagination, able to produce change.

CHAPTER 7

Religion and Cognition

Abstract Cognitive studies have offered an explanation of religion. Important contributions to this discussion have been made by Pascal Boyer and Harvey Whitehouse. Boyer argues that religious ideas are minimally counterintuitive and therefore easily adopted. The argument depends on what constitutes intuition and on the assumption that there are standard cross-cultural cognitive processes, and that beliefs are the main focus of discussion. Embodiment theory, by contrast, would focus on practices and rituals. Whitehouse has argued for a combination of cognitive and social modes of explanation. Whitehouse developed a robust ideal-type dichotomy, postulating doctrinal versus imagistic modes of religiosity. While his concept here comprises a dichotomous frame, Whitehouse recognizes that the frame is broken in practice by the coexistence of these modes within a given system.

Keywords Beliefs · Embodiment theory · Harvey Whitehouse · Intuition · Modes of religiosity · Pascal Boyer

Anthropological theorizing has always been the site of competitive takeover bids. Evolutionary psychology has been briefly mentioned in the previous chapter. As an analytical framework it is often linked with approaches through cognitive science, which attempts to look beyond any particular set of ethnographic materials to their deeper foundations

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in cognition and evolution. Culture and society do not explain themselves in this scheme. Rather they are themselves produced, determined, and are to be explained in general cognitive terms. A favorite context in which this topic is pursued is religion (and with it ritual). The mode of explanation offered lies in generalizations across cultures, not from within the culture studied. Cognitivism therefore proposes to break the frame of analyzing culture or society as both phenomena and the means to explain phenomena. The explanation is to come from outside, *theoria ex machina*, like the ancient theater god who came down on a swing to pronounce judgment on humans, the *deus ex machina*.

How successful has the cognitivist approach been? This is a dynamic growth arena of research in which a prominent part has been played by Pascal Boyer and Harvey Whitehouse and their many collaborators. It is important, as Boyer has been at pains to point out (Boyer 2001), to understand what exactly his cognitivist approach aims to explain and what it does not. Boyer's work, as he repeatedly says, does not seek to explain all of religion in cognitivist terms. It does seek to explain why there is a recurrent set of similarities between cultures in terms of ideas about spirits, ghosts, etc. that may plausibly be argued to be foundational to religious beliefs. Boyer aims to explain these similarities as the product of evolved capacities that are triggered biographically in the cognitive development of children. Children, he says, universally develop a knowledge of realities in the world. (Note that we are back here to postulates of universalism.) This kind of knowledge Boyer calls "intuitive ontology" (Boyer 2001, p. 58). It is not the same, he says, as the language-based acquisition of classificatory terms. An example of intuitive knowledge is the cognized difference between humans and others: another is the difference between animate and inanimate things in the world (Boyer, p. 61). Boyer says that the tendency to learn these distinctions is an evolved propensity of the species, so it shows up everywhere. Allowing that this is a massive claim, we are next invited to move on to "religious ontology" (p. 63). In this, propositions emerge that run counter to the realities first learned. Spirits, for example, are said to be able to move through physical objects and to be mostly invisible themselves (p. 63). How does this come to be? Boyer appeals to another heuristic idea, that by being counterintuitive these ideas capture attention, become memorable, and so are passed on.

Moreover, Boyer argues, such ideas are counterintuitive only in minimal respects. In other respects spirits are like people and hence the ideas of them gain plausibility. We are presented, therefore, with a theory of

human learning that argues first that we are disposed to learn factually based ideas, but second that we somehow have an ability to negate such ideas and create counter-realities that are then the basis of religion. Religious ontology negates ordinary ontology, yet the two coexist, it seems, according to this argument. This is made easier, Boyer suggests, because once the counterintuitive step is taken, the rest is perfectly intuitive and in line with reality. Boyer is then able easily to show cross-cultural examples of cognitive actions attributing intention, agency, and feeling to objects held counterintuitively to encapsulate spirit power.

Part of the argument, then, is based on imputed childhood cognition. What, then, are we to do with the propensity of children (and adults) to imagine all kinds of things? Boyer's argument suggests that religious ideas are counterintuitive. But what if in a different way we could equally say that they are intuitive or based on varieties of the experience of embodied human subjects?

Another problem here, if we follow Boyer's line of argument, is why do the counterintuitive ideas emerge at all? There seems to be no cognitive theory that can explain this in itself, other than as an improvised way of arriving at an explanation of religion in a kind of Neo-Tylorian modality.

Once we emerge into the world of developed institutional practices, we see how ideas are enveloped in and endowed with legitimacy by institutional structures, and are upheld by communities ("churches" in Durkheimian terms). Boyer would agree with this point, since he notes that religion contains moral rules and ideas of identity and sometimes emotional states, as well as notions about supernatural powers (p. 57). This in itself points up how modest his cognitivist aim is. The aim is not to explain religion, but to explain (speculatively) how its hypothesized core notions came to be entertained. The explanation, however, does not seem to be based on anything other than the supposition that what is counterintuitive is memorable. To supplement this idea, some functionalist suggestions would need to be put in place. Why the need to use these counterintuitive ideas? The cognitive theory by itself does not offer an answer to this question.

When we consider, also, how religious ideas are actually transmitted, the answer lies in the authoritative handing down of knowledge by respected persons, not in the spontaneous development of ideas in children or adults. We can still suppose that in some way people are able readily to accept such notions and thus we can attribute this tendency to an evolved predisposition. Indeed this appears quite likely; but if we argue

this, the question of what is intuitive versus nonintuitive begins to dissolve away. If learning is customary or institutionalized, this in itself explains why religious ideas are transmitted. Boyer recognizes this: "some religious assumptions can become part of a cultural 'routine'" (p. 59). In fact, most ideas do so; otherwise they would not come to be passed down. Boyer would still maintain his general proposition: that the register of basic ideas is quite small, indicating a unity of cognition. This is the cognitive scientist's equivalent of the old psychic unity of mankind hypothesis.

What, then, has been achieved? Some limited, but useful propositions have been advanced. One is that core religious concepts are only minimally counterintuitive. The validity of this is constrained by the point that intuition itself may produce these concepts. A second is that counterintuition applies to the same domains as intuition, and is geared into special uses, for example, when to think that a bird might be an ancestor, and when sometimes it is really just a bird. Ethnographers can easily enough identify such specifications. A bird may be seen as an ancestor if it appears in a sacred place or it intrudes surprisingly into a social space of humans in an unexpected and mysterious way.

At a more general level, it is easy to see that Boyer's enterprise is to introduce a degree of reductionist analysis into the study of religion, focusing on why elements constantly appear in the cross-cultural record. He argues that these correspondences stem from standard cognitive processes and so are not products of any particular culture. Culture, however, does not disappear so easily. It produces variation, and it is the way in which notions are transmitted. Cognitive theorists, then, break the frame of culture only to implicitly reinstate it or at least come to acknowledge it at another stage of their argument.

Our own position here is that we see the merit in Boyer's mode of argumentation, but the question of what is intuitive or not intuitive is bothersome because it does not have a clear basis for a demarcation of one from the other. Boyer argues that the acquisition of intuitive concepts by children is universal and is not triggered by language, but we may be reasonably inclined to think that language must be involved because children acquire language early on in their development and it plays an important part in their socialization.

The last comment to make here is that the whole choice to concentrate on beliefs as a category determines the way analyses will proceed, giving an almost textualist aura to discussions. If one were to begin with emotional orientations and link these with ritual theory, we might find ourselves

bypassing the question of belief as secondary to the worlds of embodied practice and performance, as we have hinted in our earlier publications (e.g., Stewart and Strathern 2014: ch. 7). Connected to this point is that cognitive theorists tend to concentrate on the mind in an almost Cartesian way and have fixed on ideas and beliefs as products of the mind operating under evolutionary constraints but not in terms of embodied engagement with the world. In attempting to break out of the frame of culture-based analysis they have actually taken their stand on a view of what culture is, that is reminiscent of discussion prior to Thomas Csordas's groundbreaking arguments for the importance of the body (the mindful body, that is, of course) (see essays in Csordas 2002). Cognitivists also construct tests of memory recall and cognitive patterns that again remove them from social action or participant observation studies, which remain the sphere of ethnographers. The overall emphasis on mind is shown in the title of one of Harvey Whitehouse's many edited volumes, in which Boyer's chapter appears: *The Debated Mind*. Whitehouse's own use of the issue of memory is productive, because he has linked it to his hypothesis of differential emphases in religious systems (e.g., Whitehouse 2000). He has also consistently argued for a rapprochement between cognitive and social anthropological approaches to explanation, respecting ethnography because of his own early work in Papua New Guinea.

We will take here further contributions to this kind of debate using materials from the intriguing volume edited by Whitehouse and Laidlaw and published in our own Ritual Studies series (Whitehouse and Laidlaw 2007). Laidlaw stood out in this volume as a skeptic. Most contributors were true believers. Whitehouse himself worked hard to accommodate different viewpoints. We provided a preface to this book in which we experimented with materials from Mount Hagen that could fit in with Whitehouse's approach. We did so in a spirit akin to Whitehouse's call for a combination of cognitive and cultural modes of interpretation. Lanman's chapter in this volume (2007) illustrates some of the difficulties in such an endeavor. He shows (ch. 4) that the versions of interpretation of the killing of Captain Cook in Hawai'i presented by both Marshall Sahlins and Gananath Obeyesekere rely upon partial inferences from the available data. Sahlins thought the people considered Cook to be the God of fertility, Lono, but were upset when he came back at an incorrect ritual time. Obeyesekere argued that Cook was thought of as a chief, Lono, and they killed him when he declined to give them military help in a local war. Lanman applies available cognitive ideas to the data and finds that these

facilitate a critique of both Sahlins and Obeyesekere but do not help to advance a superior interpretation. Moreover, the cognitivist ideas Lanman advances are not different from standard anthropological stances. They amount to saying that cultural themes are not the same as particular ideas in people's minds, and that there are no ideal forms of practical reasoning, only specific attempts at such reasoning. In effect, these points coincide with the typical views of Hageners (and others in Papua New Guinea) that you cannot ascertain what is in another person's mind. It follows that you cannot use a guess about a person's thoughts as a reliable explanation of events they are involved in, although in practical social life people do this regularly. The idea also leads to a stress on dealing with the consequences of action rather than endlessly guessing why people acted as they did.

Lanman is avowedly a cognitivist himself, but his chapter indicates that cognitive science is not a magic key to unlock the door to all explanations of social phenomena. Moreover, the cognitive science he deploys is on a par with the common sense of Mount Hageners in Papua New Guinea. In rebuking Marshall Sahlins for supposedly equating cultural dogmas with individual motivations. Lanman cites the cognitivists' "discovery" of "the mind" (Lanman, p. 116). However, not only did the Mount Hageners 'discover' this aspect of mind before cognitivism became popular in academia, but also the same kinds of observations are commonplace in social anthropological writings.

Boyer's theorizing has been found most productive in terms of the general cognitivist explanation of religion. This is at least in part because he is very careful to make his claims modest. His theory aims to explain only a small part of religion, but it is nevertheless a critical part if we consider that beliefs are central to religion. A problem in his theorizing is that, while he aims to establish religious ideas as only minimally counterintuitive, the issue remains of why such counterintuitive ideas emerge at all and what ends do they serve? Of course, if we see religion as a product of the creative imagination, whether intuitive or not, we obviate this conundrum.

Whitehouse himself is perhaps best known for his theory of the two modes of religiosity, the doctrinal and the imagistic, each depending on different modes of memory, semantic, and episodic. Like Weber's ideal types, the two modes are not intended to identify whole systems – a given system may contain both modes (but how does this work in practice?). Gilbert Lewis is a social anthropologist noted for his thoughtful and quiet observations on large themes, from the vantage point of his own ethnographic work in Papua New Guinea (Lewis 2004). Lewis's thoughts in this

chapter turn on looking at complexities in assessing what is to be described as doctrinal or imagistic. The overall effect of his discussion is to make us realize that in actual practices elements may be mixed up together, so that the ideal type disappears in such complexities. It is hard to see what the application of Whitehouse's dichotomy should be other than pointing to differences, but if in practice these differences are blurred in the nuances and details of social life, it becomes less clear what the value of the hypothesis is. Reformulating the hypothesis, it seems fair to say that Whitehouse's doctrinal mode flourishes in circumstances of literacy, and the imagistic mode is found in the indigenous oral religious patterns of New Guinea. But there is always a cross-cutting element. Taboos, for instance, are doctrinal in character, but they may depend on imagistic ideas. The doctrinal/imagistic dichotomous scheme turns out to have many exceptions, and yet it has still helped to illuminate our thinking about ways in which religious practices are transmitted over time. Whitehouse's distinctions have become a part of the ways we think about the transmission of religious ideas and practices. The distinction runs parallel to the old primitive/civilized dichotomy, but offers instead an operational pair of ideal-type definitions that enable us to think about how these types interrelate and how one may shift into the other over time, or the two may coexist.

We can see that this part of the cognitive theorizing about religion stands on its own separately from the general cognitive theory of religion as propounded by Pascal Boyer. We have also already seen that Boyer's own theory functions essentially as a just-so theory about how religion began and how it fundamentally is generated at the individual cognitive level, but it does not operate as a sociological argument about the group aspect of how religions work. Durkheim's model of the collective life as a phenomenon in its own right is therefore left largely intact. His sociological frame has been not so much broken as sidestepped or bypassed, and the question that others have raised about how to relate the collective to the individual aspects of practice is not addressed. Language is again crucially involved here and in the next chapter we turn to discuss arguments about its role in human life and its cognitive basis.

CHAPTER 8

Language and Culture

Abstract What is the relationship between language and culture? One argument has to do with whether there is a universal (biological) program for the acquisition of language or whether there is a set of potentialities which are molded by culture. Noam Chomsky argued for a universal (natural) program, whereas other linguists have argued that language is always a cultural invention. Daniel Everett suggests that only a general learning capacity is at work. Chomsky distinguished between broad and narrow faculties, the latter reducible to recursion, the folding of one statement into others by grammatical means. This, however, is not found in the Piraha language of Amazonia studied by Everett. Everett offered 'intelligence' as the factor enabling language acquisition, but the ability to combine patterns gets us closer to the processes involved.

Keywords Biology · Dan Everett · Language acquisition · Noam Chomsky · Pattern-making · Recursion

The relationship between language and culture is one of those perennial topics that merit a continuous revisiting over time. Different parts of arguments come into play. Does language determine thought? Does culture determine language? Why is a capacity for language a universal human feature? Among these debates, one in particular links to themes we have explored in previous chapters of this book. This is the argument about why languages exist at all and whether the capacity for language unites or divides

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us. Essentially this discussion revolves around the same set of categories of thought that appear with the nature versus culture dichotomy, and also in the arguments over reductionist analyses in anthropology. Indeed the master trope that guides many arguments between natural and social scientists seems often to be the same nature/culture categorization. Equally, all resolutions of these arguments inevitably end up by breaking the frame itself, mediating if not obliterating the dichotomy. Cognitive scientists have approached this point by acknowledging, as Whitehouse does (2001, p. 209), the importance of social learning and the fact that we must study the properties of the mind "*in its environment*" (p. 209, emphasis in original). This in turn implies what we would call a processual approach, seeing learning as a continuing situation over time. Whitehouse follows a neuroscientist, Edelmann (1992), according to whom in Whitehouse's words "human brains do not come pre-equipped with modules for classifying the world, for acquiring grammars, or for any other mental functions" (loc. cit., p. 213). Instead they consist of a complex mass of potentialities ("circuitry") within which patterns are made and strengthened through experience (p. 213). This formulation seems to fall on the culture side in terms of the nature/culture dichotomy, but on the nature side there is no tabula rasa but a complex set of capacities ready to be shaped by what Edelmann calls neuronal group selection – in other words, seen from outside the brain itself, what we call culture, always noting that culture, too, is not abstract but is a matter of experience. The old terms of a nature/culture debate thus dissolve, replaced by a complex model of interactions. Also, if we substitute 'the embodied person' for mind in these formations we come closer again to the way in which the world is experienced by humans.

This argument can now be carried over into the sphere of language and culture. The human capacity for language is generally seen as a prime although not unique or self-sufficient enabler for culture and society to develop. Insofar as this capacity is indeed universal it has been tempting to consider it as specifically hard-wired into the brain, not as a generalized learning aptitude, but as a particular mechanism holding the ability to understand and replicate grammatical patterns. This was the position adopted by the famous international linguist Noam Chomsky based at MIT in the USA. Daniel Everett, in his book *Language, the Cultural Tool* (Everett 2012), discusses the issue at some length. Is language a cultural invention developed to meet emerging problems of communication vital for the survival of human groups and passed on for these same purposes? Or is it something that all humans are equipped with as

a specific physical capacity from birth? Is it, in other words, a product of nature or of culture? (And if nature, what made nature evolve that way, we might ask).

One empirical background out of which this argument plausibly emerges is the fact that children generally learn language with great speed, suggesting that they are at least preadapted to doing so. Also, children learn language in definable stages, regardless of which particular language it is, again suggesting that an innate kind of 'knowledge' guides them (Everett, pp. 67–68). One element of controversy is whether there is a critical period for language learning, which makes later acquisition of languages harder. This is probably true, and for a number of reasons, none of which requires an idea of innate grammatical knowledge, but all of which require a general learning capacity. First-language acquisition is oral and social, and success in it is overdetermined. Second-language acquisition through books or formal classes is quite different and often combined with the need to learn many other technical forms of knowledge. This is why the immersion method, replicating aspects of oral first-language acquisition, is considered to be more effective, whenever this option is available. (When languages no longer spoken are involved, of course, this option cannot be realized, but a kind of intensive text-based immersion can substitute for it.) Furthermore there are no predefined areas of the brain exclusively dedicated to language, although such areas can develop with experience (Everett, p. 73). Instead the brain works in more plastic and polyvalent ways. Specialization develops with learning and experience, in other words, via cultural means.

The bread and butter of practical linguists who study different languages is that languages vary in their grammar. Some universal features may be found as tendencies but not as predominant overall patterns (Everett, p. 85). If there was a uniform species-specific language system in the brain, uniformity would be greater than it is, Everett concludes. If, then, there are few linguistic universals (other than, we might note, language itself) the likelihood that there is an innate universal grammar becomes very faint. Theorists of the brain could more easily agree, however, that the brain is 'a good problem-solver' and so could develop language to solve a problem of communication just like it can enable the development of any tool needed for survival.

In response to difficulties of this kind, Chomsky and his associates distinguished between broad and narrow faculties for language (Everett, p. 89). The broad faculty is said to comprise a number of enabling

features (some shared with other species). The narrow faculty Chomsky says is unique to humans. In this formulation it becomes very narrow indeed, however, because instead of being a term for a whole suite of grammatical capacities it now became applied to just one grammatical feature, commonly found indeed, the feature of recursion. Recursion refers to a self-referential process whereby one statement is encapsulated inside another, enabling a complexity of statements to be made. This proposition is the apotheosis of the narrow version of Chomsky's theory, the broadest version by contrast being the notion that the whole of the human brain (or body, one might then add) is a language tool – but it is a tool for everything else as well. And just as people vary in overall ability, so they vary in language ability.

Overall, evidence is on the side of supposing that language acquisition is learned, not innate. A general capacity for language must be a part of the human brain because all cultures have developed languages as means of communication. If we like, then, that capacity can be called innate. But it emerges only in a particular engagement with the environment, and in human contexts that environment is the kinship system with parents and others as caretakers. At once, therefore, language and culture presuppose each other, depend on each other, because kinship goes with kinship terms, that is, with language. And kinship exists because offspring require time to grow up. A functioning kinship group produces a motivated context in which all kinds of learning can safely take place, including initial language learning and inculcation followed later by other influences, such as peer–group interaction, formal schooling, travel, etc.

It is interesting that Everett, after arguing at length against a language instinct (with all the ambiguities of meaning contained in that term), supports instead another innate element, an "interactional instinct" (p. 185) as the driving force behind the acquisition of language and cultural skills. Perhaps, though, this also is not necessary as a hypothesis. A generalized ability to learn would suffice to explain how children develop language abilities. This argument frees Everett to discuss many clear examples of how cultural ideas and practices shape expressions in language. It seems that language is brought fully back into the realm of culture, in a relationship of co-evolution. Of course, it remains the case that particular languages have different grammars and that these differences produce technical problems for translation. Everett produced a striking example of this sort, because the Piraha language spoken in a part of Amazonia which he studies does not use recursion. It does not embed one sentence in another. Each sentence

stands on its own. Recursion is the feature which Chomsky and his fellow workers declared was universal, but the Piraha case negates this claim, Everett says (p. 200).

Culture and language go together but do not always match each other directly. Everett's main argument is simply that language itself is a cultural tool. If so it is also a very flexible tool available for changes to be made. Language is porous and can easily absorb new terms and meanings. Neither language nor culture is static. So in saying that culture produces language we must not impute rigidity to this process. We must instead break the frame of rigidity and recognize the prevalence of fluidity.

The discussion so far has served to make problematic any innatist argument about language. At the same time, in assigning language to the same conventional realm of culture as all learned behavior, it points clearly to possibilities of changes in language because culture itself is constantly changing. Also, while we find that the vocabulary of a language can change rapidly, its basic grammar shifts more slowly, so that grammatical forms may persist that date from an earlier time and are no longer in step with other aspects of culture. Rules of gender accord are candidates for this interpretation in European languages that owe a part of their origin to Latin, and more remotely Greek. There clearly are psychologically common-sense aspects to some of these rules but it is hard to show how they could possibly apply overall. Everett gives a comparable example from Amazonia. In the Banawa family of languages, which spans Peru and Brazil, feminine forms of gender marking predominate and are exclusively used for first and second pronoun usages, regardless of the gender of the speaker or the person referred to. Banawa culture also happens to stress segregation of the sexes and has elaborate initiation rites for young females, but neither of these facts explains why the language has grammatical rules that make all actors feminine other than as a grammatical device used to provide grammatical co-ordination in sentences by marking subjects of verbs (as happens also in French).

What overall message about language and culture can we convey, then? First, since language itself is a part of culture this is not a relationship between two separate 'things'. So much is axiomatic. Second, however, language is not all of culture, since there are many material and embodied practices as well as aesthetic forms of expression that exist without the intervention of language. Third, nevertheless all of culture tends to get back into language when activities are named and/or described. Taken as a whole, each language therefore encapsulates reference to many sectors

of culture (and society) and functions as a storehouse of signs of knowledge. Within this frame, certain significant markers of identity stand out and provide foci for discussion and exposition. A term we have often highlighted in this regard from Mount Hagen is the Melpa language word *noman*, meaning mind, consciousness, will, reason, seat of emotions, ethical sense, individual desire: personhood for the Melpa, and as such it comes into a plethora of social interactions that define humanity and social life (see, e.g., Stewart and Strathern 2001).

There are also aspects of grammar that are enlightening when seen as cultural phenomena. We pick out two of these here. Melpa has an enclitic suffix, signaling agency or cause of an action. Linguists have identified ergative verbs as ones in which the subject of the verb when it is intransitive appears in the same form as it does when it is the object of the same verb when transitive (e.g., compare 'the window broke' with 'the boy broke the window'). The reasons why such usages have commanded attention have to do with the dual senses of meanings in the verb along with variations in the form of the nouns such that the form of a given noun varies when it appears as the subject of a transitive verb as opposed to an intransitive use of the same verb. In English, however, subject and object are in any case not inflected as they are in Latin, and are identified instead through word order. This is generally true in Melpa also, although the word order is different from English. In English elementary word order is Subject Verb Object (SVO), whereas in Melpa it is SOV. But in Melpa, if the speaker wants to stress the agency of the subject of the sentence, the suffix /-nt/ can be added to the noun that identifies the subject. However, this same suffix can also be added to an instrument wielded by the subject. Both the subject and the instrument may take the suffix /-nt/ in that case, without causing any confusion. In other words, Melpa has a usage that stresses both the agency of an actor in a transitive verb usage and the agency of some instrument the actor employs, for example, in warfare when an axe is used to strike and kill and opponent. Persons and things are thus linked as having agency, a feature that connects with many others in the culture. The affinity with ergative usages is only passing here, because Melpa usage only picks out a special transitive usage rather than a regular marker of the transitive form, although there are verbs that operate in complex other ways, as with the verb 'eat':

Transitive *na röng nond* (I eat food) (literally, I food eat)
Intransitive (?) *na peng nonom* (I have a headache) (literally, Me head eats, that is, it eats my head, my head hurts).

In the latter example, what is the subject and what is the object? We have to put fillers in if we are using English: (It) is eating me (in the) head. The Melpa expression defies simple parsing of Melpa ideas about the body and the person into the grammatical categories of subject and object. Head and self are closely intertwined by synecdoche and metonymy. In the expression here, also, the SOV order of words is not followed but is if anything reversed, yet head is not the simple subject of the sentence. The embodied psychology of the statement is quite clear in experiential terms, while the grammatical analysis remains complex.

The other feature of Melpa that is of note here (among many that could be cited) is precisely the feature that Chomsky regarded as universal and Everett pointed out is absent in Piraha: recursion.

The Melpa language has a facility for the enclavement of subordinate clauses within longer utterances, where the subject of the main clause is separate from the subject(s) of the subordinate clause(s). The grammatical item that expresses this situation is the participle, which takes a different shape according to whether its subject is the same or different from the main verb's subject, for example:

*mokl*pa *tepa noman tep mondopa purum*
(after a while [he] considered matters and left)
(literally, being taking mind taking making to be [he] left)

Contrast this with:

*elim mangkona mokln*ga-*kin wuö mat-nt ruk orong*
(while he was staying in his house some men came in [to the village]
(literally, he home being – with, men some – acting in came)

In the first sentence the – *pa* form marks the concordance of the participle forms with the subject of the main verb, whereas in the second sentence the – *nga* form marks a disjunction with the subject of the main verb. This grammatical feature enables Melpa speakers to produce long and complex utterances in narrative form, drawing in many contextual features of situations. A subtle view of events and agency emerges. The grammatical form both produces cultural form, in a sense, and itself is molded by Melpa social patterns.

These are examples of the kinds of analyses we need in elucidating the articulation of language and culture together. Language and culture are not separate. We must break the frame that might appear to see them as separate, when in fact they are indissolubly connected. We found the same in considering arguments about the nature of language, finding that it is born in the context of cultural praxis, rather than in brains as isolates. A mindful anthropology aims to look into this process and this context and seeks to ascertain and understand it, simultaneously from outside in and inside out.

When we speak of cultural praxis, we are speaking about powers of communication, not just utterances as 'action' but talking as 'interaction'. Everett argued strongly that language is a cultural tool, meaning that it is used for cultural purposes. We can also argue that it is first and foremost a social tool, used to enhance and to develop social relations within a speech community, as sociolinguists from John Gumpertz onward have always argued. This being so, what capacity does spoken language exhibit that enables it to function as a supremely widespread social tool? From here, we can proceed to a further question: Is it 'Intelligence' that is involved, or something more specific?

In his treatment of the issue of the origins of language, Everett proffers the idea of generalized intelligence as an alternative to Chomsky's theory of a specific capacity for language. While Chomsky's suggestion seems too specific, intelligence may seem too unspecific or vague. Theories that have relied on a notion of differences in 'intelligence' between specific categories of people have also generally been set aside in social science arenas since the work of H. J. Eysenck, who used various tests to produce graphs of hypothesized group differences of intelligence (e.g., Eysenck 1979, *The Structure and Measurement of Intelligence*, Springer Verlag). Intelligence, then, while clearly a valid notion in general terms, is not a very precise concept that can be applied to capacities for language. We argue instead, that a very general human propensity and capacity has to do with the recognition and production of *patterns*. Pattern-making, finding patterns, cultures as patterns, these are elements that language depends on for its emergence and propagation. In language learning sounds can be picked up and mimicked. To do this, and to remember utterances beyond the moment of mimesis itself, requires the storage of information in a retrievable form, and this is made easier if pattern recognition is superimposed on imitation. It also implies a generative ability to apply the pattern across cases. Take, for example, the problem for an English-language speaker

of comprehending the patterns of German pronunciation of words, made more deceptive by the fact that English and German are quite closely related via Anglo-Saxon ancestry. As an English speaker, there is a tendency to imagine that letters will be pronounced in German in the same way as in English; but this is exactly not the case, so that pronunciations have to be re-learned, and this is facilitated by generalizing from example to example, especially because often the learning is done in the form of looking up words in a dictionary, so that the visual symbol of the alphabet appears first and the ability to pronounce it second. /Z/ in German is pronounced like /ts/, /w/ as /v/, /eu/ as /oi/, and /v/as /f/. A pattern therefore emerges in which across a whole spectrum of utterances the same phenomenon is found and remembered, thereby translating into a 'rule' (or course, there can be context determined 'exceptions', described as such once the terminology of rules is adopted). The question of pattern exerts itself even more strongly at the level of grammar, because to make a comprehensible statement about events in the world grammar is needed to link the semantic elements together. Grammar by itself does not encapsulate all meaning, because a statement can be perfectly grammatical but still appear as nonsense in the sense that it cannot be successfully connected to experience. Chomsky's famous example can be cited here: the expression "colorless green eyes smoke furiously" violates several experiential codes. With modification, an analogous statement that does confirm to sense can be brought into play: "the deep green-colored eyes seemed to emit smoke when s/he was angry". Grammar itself can, however, be said to carry meanings such as 'this is a transitive/intransitive verb' and 'subject verb object' in the word order in English where a tagmeme (ordering device) operates across the words themselves and the directional drive or aim of the statement hangs on this grammatical point. Also, of course, this whole superstructure of clusters of semantic reference and grammatically coded orderings is itself built on an infrastructure of basic patterning that is the system of phonemes and overall phonological features that enable everything else to work. Phonemes are, in turn, built on differences of sound, in other words they are tied to speech, not written forms of language – which is why spelling words is a challenge, especially in English which has taken in and domesticated a great deal of vocabulary from other languages, preserving or more often altering the pronunciations of the words incorporated into the stream of the language.

Our point here is simple. Language ability depends not only on an ability to perceive patterns but also on an added ability to synthesize

different levels of patterns together so as to create a complex product of communication. This observation reinforces our argument that replacing 'intelligence' with 'pattern-making ability' helps to add precision and specificity to the discussion of what was involved in the evolution of human language. It is also possible that we could argue that all intelligence is dependent on the making of patterns, for example, through the work of hand–eye coordination in the exercise of motor skills, in which the understanding of pattern and its application must constantly work together. There are, further, no doubt many different varieties of meaning of the term 'pattern' itself. Here, it is enough for us to suggest that the ability effortlessly to layer patterns together to communicate by speech provides the theoretical kernel for the emergence of language, and that this idea mediates between the over-specificity of Chomsky's argument and the over-generality of Everett's proposition about intelligence.

CHAPTER 9

Against -Isms

Abstract Stereotypical histories of anthropology tend to revolve around a succession of theoretical stances described as '-isms', for example, evolutionism, diffusionism, structural-functionalism, structuralism, transactionalism, processualism, etc. Labeling history in this way reveals a tendency to essentialize shifts and schools of thought. 'Neoliberalism' is another, recent, term. Viewpoints presented in this vein are promulgated by networks of power and influences in academia. The process of struggle against such networks is found in the early career difficulties of Fredrik Barth, who was later able to draw productively on American and British traditions of analysis, refusing to join bandwagons, and insisting on meeting the challenges each field area presented. He followed what can also be advocated generally, the aim of bringing theory and description appropriately together, as he did in *Ethnic Groups and Boundaries.*

Keywords Academia · Essentialization · Field experience · Fredrik Barth · Neoliberalism · Stereotypes

The history of anthropology is replete with the rise and fall of different schools of thought, usually emerging out of the ideas of one dominant personage or in retrospect attributed to that personage. With a rise in the popularity and prestige of a particular approach, more grant money and more teaching positions, and in step with these two trends more

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publications, get allocated to the followers of that approach. When criticisms emerge and gain attention, the trend reverses and another approach comes in on the back of the critiques. All this takes place against the backdrop of, and is influenced by, wider economic, political, and philosophical movements that anthropology draws from and which sometimes draw also on anthropology. At the peak of a period of rise for an approach, the propositions it puts forward are regarded as 'truth', at least by its most ardent adherents. At the bottom of the trough it may later fall into it may be discredited as 'untruth'. Not everyone subscribes to these views, or tries to surf the waves and ride into their highest point, escaping when the wave sinks. It takes strength, however, to stand outside of a current, and generally such strength is available only to those whose social position is secure. The least secure are graduate students, who do not yet have their Ph.D.s, and will be struggling in the job market after they do receive their higher qualifications. They have dissertation committee chairs and mentors. If they refuse to follow some particular theoretical line, or are caught out on some technicality such as a difficulty in finishing an exam question in a specified time, they may be flunked out of the program, or may try to shift elsewhere. The easiest tack for them is to adopt the prevailing theories or ideas, and deploy these as a part of their eventual dissertations. An example that we know of is where a Professor espouses the currently fashionable term 'neoliberalism' and students are told they should use this as a primary explanatory device. Predecessors of 'neoliberalism' as a term included 'underdevelopment', 'exploitation', and 'unsustainable'. All of these terms are descendants, or collaterals of descendants, of Marxist theorizing, which achieved a vogue in British social anthropology of the 1970s and 1980s. Neoliberalism marks an awareness of the shift of responsibility onto individuals within state societies and their bureaucracies. Website technology actually has played a big part in this process, because it makes it more possible for such a shift to happen. But this process is assuredly not the only process to be taking place, because collective action and collective movements also continue to be very prominent.

What we have said here should not be taken to mean a wholesale condemnation of use of the word neoliberalism. Like all jargon words, it has its uses. What we are identifying here is the uncritical use of this term, or any other term, that occurs when it becomes a fad rather than an analytical tool. Students respond to fashions and they will sprinkle fashionable terms on their texts in the hope that these will fertilize their

scripts with insight. This could in fact happen if they were to free their own minds to think about the terms they use rather than robotically following mandatory conditions. On the side of the academics who impose, or are felt to impose, this situation, the reasons for their actions lie in career-building. If students follow their lines, they will take these with them and help to propagate their viewpoints. If their advisor is influential in a network, the advisor can contrive to seed various departments with their acolytes and so spread the word. We are not making any of this up. Observations in the UK system show that great influence can be wielded by high-ranking academics in prestigious, old established universities who write references and give recommendations for jobs to networks of colleagues in other universities. A kind of oligopoly is thus set up and maintained. Outside of the UK we have also found by experience that one weapon academics can wield on a student's Ph.D. committee is to allow them reluctantly to pass but to refuse to write job references for them. The opposite process occurs too. A professor may ensure that a poor dissertation is accepted because the student then enters into a clientage relationship with their sponsor. Voices of disagreement are overridden by a majority vote, made most feasible when the committee contains, for example, a married pair and one of their senior clients holding a position in the same power network. Nepotism to this extent can smother the legitimacy of a whole program. Often it is not even that a single theoretical viewpoint is at work; it can be pure patronage–clientage ties as leverages of power. It is in this context also that another weapon is at times deployed, ageism. Senior Faculty who take a different and independent view of academic issues are made potential targets for marginalization and exclusion from influence, in terms of courses they teach and in particular access to the training of graduate students through being Advisors and getting in students who wish to work with them.

Micro-level processes of this kind eventuate into larger scale ones that produce -isms of various kinds. We hasten again to say that much of this is how science normally develops. What we wish to stress, however, is that once a mode of analysis becomes institutionalized it tends to obliterate other modes if its practitioners are powerful enough. Of course, it is the practitioners and not the ideas themselves that bring all this about in pursuit of extending their personal influence. Ideas are not agents, humans are. Moving around from country to country provides an opportunity to view this process at work without getting completely pulled into it.

Mobility precludes one from exercising power within a given context, but it can enable one to get a better view of things.

Historians of anthropology like George Stocking and Regna Darnell have given admirable accounts of circumstances in which leading scholars exercised legitimate influence in their discipline, so we do not need to enter into this sphere. However, we have already looked at one case history of the career of a particularly innovative thinker who experienced opposition from entrenched viewpoints. This is the case of Fredrik Barth.

Thomas Hylland Eriksen (2015) has written a brilliant appraisal of Barth's work, containing also many memorable insights and condensed formulations of his own. Eriksen and Barth together form a nexus of positions and orientations that exemplify strongly what we ourselves understand by a mindful anthropology. Eriksen portrays accurately and imaginatively both intellectual issues and interpersonal conflicts that tend to be intertwined with these issues. Barth clearly belonged from the start to an intellectual elite in the arenas of Norwegian social scientists, but he nevertheless fell foul of entrenched authority on at least two occasions. The first was when he submitted as a Ph.D. dissertation in January 1953 his early study of South Kurdish populations in Iraq. He had gone to this field area as a participant in an archaeological expedition and stayed after the expedition was over to spend some time doing a study of his own design, with the help of a Kurdish leader who kindly sponsored him and gave him hospitality and protection. Barth had good fortune in this and subsequent field projects, probably because of his congenial and perceptive ways of handling situations and because of his love for fieldwork in general. The field for him was not just an experimental laboratory, although in his work he sought to make comparisons of an exploratory kind. The field was a lifeworld into which he fully entered, engaging himself in a completely mindful way with observing and understanding what was before him. Where he, like many others, ran into intractable difficulties, was within the hierarchical professional and institutional venues back in his home arena in Norway. When he returned to Oslo and submitted the results of his Kurdish studies to his doctoral committee there, he received a setback. His Ph.D. dissertation was a disquisition on the reasons why Kurdish social organizational forms varied across a broad region, while the general culture was uniform (or relatively so). Its tightly organized argument is premised on a contrast between 'culture' – a broad set of customs and practices – and 'society' as a historically specific set of structures and arrangements of power, which was a part of the viewpoints

expressed in British social anthropology, for example, by Meyer Fortes and by Edmund Leach, and in American anthropology by Roger Keesing, who was influenced by these thinkers. Barth had just spent a year at the London School of Economics and had met Leach there.

Barth used this distinction productively in much of his later work. He concentrated on the social structure side of analysis and played it off against culture. His groundbreaking analysis of Swat Pathan politics is set into descriptions which take Pathan culture into account but look at how individual leaders use these cultural forms to increase their power. In his edited book *Ethnic Groups and Boundaries* (1969) one of the themes is how culture is used to maintain boundaries between identities, but can by the same token become a mark of a new identity for individuals or household units if people cross over such boundaries.

The seeds of Barth's later thought can therefore be seen in the Kurdish study. Its sharp focus was apparently not appreciated by Barth's Ph.D. Committee, however. The Committee of senior people included Gutorm Gjessing, Director of the Ethnographic Museum at the University of Oslo. Barth had not got on well with Gjessing, whose vision of anthropology seemed antiquated to Barth, as it was based on the idea that an anthropologist could study any aspect of culture and should try to do so. (In principle, we may note in passing, there is nothing intrinsically invalid about this kind of holism as a theoretical stance, but as a recipe for the practicalities of fieldwork by a Ph.D. student, it was unproductive.) Barth, by contrast, was influenced by British social anthropology of the time, with its emphasis on contemporary social relations. In any case, his dissertation seems to have appeared unduly precocious to his august committee, or at least Barth himself later speculates to that effect, because he was only 23 years old at the time he submitted it. Eriksen (p. 22) notes that no one on the Committee was in the field of the new social anthropology, and that this may explain why they decided to ask the Oxford-based anthropologist, E. E. Evans-Pritchard, if the dissertation would be given a pass mark in Oxford. Evans-Pritchard responded, it seems, by saying that it would not, because it was based on less than a year's fieldwork, which in Oxford was required, on grounds that it was necessary for proper understanding of the society to observe a complete seasonal cycle of community activities (a stipulation that is arbitrary in several ways, including that social life is not always centered on a stable and recurring seasonal round within a specified period of months or other units of time). Barth later missed out on receiving the Curl Bequest essay prize of the

Royal Anthropological Institute in 1958, and he later surmised that he had not been respectful enough of Evans-Pritchard's work on the segmentary political system of the Nuer in that essay (Evans-Pritchard 1940), and so had been denied the prize. There is more than a tinge here of a feeling of opposition between Cambridge, where Barth went to present his further findings of his fieldwork with the Swat Pathan people in Pakistan, and Oxford, where Evans-Pritchard was Professor. In any case, Barth's eventual Ph.D. dissertation at Cambridge was based on the Pathan study and he was awarded the degree there in 1957. No doubt it helped him that Edmund Leach, as well as Meyer Fortes, was there, and they were leading exponents of the social anthropology approach. We see here a classic case of swings and roundabouts that operate in academia: disfavored by some, supported by others, Barth swiftly made his way to the fore, and he did so in Anglophone contexts of publication, which were taking the place of the German language previously used for dissertation-writing in Scandinavia (Eriksen, p. 49), which probably also reflected an untheorized holism or omnium-gatherum approach of the kind we noted above.

The structure of the argument in Barth's Kurdish study was as we have already remarked, the forerunner of his famous treatment of ethnicity in *Ethnic Groups and Boundaries* (1969). Here, too, he contrasted social structures and cultures. Eriksen notes (p. 103) that Barth argued in this volume that "ethnic differences do not correspond to cultural differences." Instead what constitutes ethnic groups is the social boundaries that they maintain and the mutual stereotypes of one another they develop (p. 103) and maintain. They may actually be culturally similar, yet by investing in social boundaries they keep separate in social terms, and thus maintain separate identities. This innovative concentration on the creation and maintenance of social boundaries gave a considerable boost to the analytical understanding of ethnicity in many different places around the world. Boundaries fall within the realm of social structure. Of course, it remains true that cultural aspects are often put forward as reasons for separate identities. But this holds only if some differences are deliberately made into a discourse of reasons for separate identities, intermarriage often being a prime site of contestations in this sphere.

Barth's early difficulties with his Ph.D. Committee reveal the site in which paradigms are regularly reproduced or contested. Written in broader forums, such processes show how '-isms' themselves emerge, for example, the trail of structural-functionalism, Marxism, and structuralism, along with various 'posts' that have been applied to modify and update

such wholesale theoretical systems, like patches put on burst bicycle tires. The overall example of Barth's work is inspiring because, as Eriksen makes very clear and as applies notably also to his own corpus of well-balanced writings, Barth never joined any bandwagon. He was driven instead by a variety of interests in different areas, by the challenges that each field-site proved to present, and by an untiring pursuit of appropriate forms of analysis and theory that could be attained precisely through attention to the particular case. Eriksen surveys this point through a discussion of the situation in the 1960s, when social anthropology was in an exciting phase of growth (pp. 98 ff). He writes that Barth thought that Levi-Strauss's structuralism, with its primary object of elucidating the features of the human mind, was "too intellectualizing and detached from ongoing social process and tangible life-worlds" (p. 99). He felt the same about the Oxford debates on the universality or otherwise of rationality, although Eriksen points out that Barth's own work on the importance of 'transactions' as constitutive of social relationships implicitly depends on an assumption of strategic rationality.

In addition, Eriksen reports that Barth was not taken with the resurgence of interest in Marxist theory within anthropology. This resurgence came in two different forms, the American one based on cultural evolution and the French one based on a combination of Marxism and Structuralism. Characteristically, Barth did not bend his energies to any formal engagement with, or refutation of, either Marxism or Structuralism as such. His response was much more immediate and personal. Eriksen writes that Barth found these approaches lacking in "the curiosity-driven process of discovery which he considered to be science at its best" (p. 102). Once he exclaimed that students following a Marxist line did not need to go to the field at all because before doing the empirical work "they already knew all the answers" (p. 102).

There is much to be learned from these comments. Barth himself is credited (or saddled, perhaps) with founding an -ism of his own, transactionalism, but this is something of a misnomer. What Barth actually proposed was that in understanding social processes the specific transactions people make with one another are formative. The question then becomes, what is a transaction? Is any social action a transaction? Not quite. A transaction is an activity of exchange, premised on the idea that values are established or altered by this means. The actors involved may be attempting to maximize advantages and gain power from such transactions, and the approach is therefore suitable to the analysis of some

contexts of political competition. It does not follow that it can by itself explain everything. Attempts to make it do so turn it into an -ism. Barth does not do this, although he did explore the potentially enlightening applications of game theory to Pathan politics. Even if, as is the case in the New Guinea Highlands, exchange in general and competitive exchanges of wealth in particular are very salient and important, this again does not mean that all social life in these societies is to be seen as composed of maximizing behavior through transactions. This, too, would be adopting an 'ismatic' approach. An 'unismatic' approach, by contrast, seeks to find insights from the ethnographic materials, and to use theoretical ideas from whatever source that seem helpful in this enterprise, regardless of whether this means borrowing from theories that are in their totalizing forms incompatible with one another.

It is also the case that a particular theory suits better the analysis of one society as against another. Transactional theory has obvious merits when one is analyzing New Guinea Highlands exchange systems. Marxist theory holds insights for the study of class relations and exploitation in capitalist contexts. Structuralism suits the analysis of societies where many values are expressed in terms of complementary dyads. A postmodernist approach easily fits with working in a society in which social relations are fluid and there are no obvious determining principles. We cannot use the facts about one society to refute the theory applied to another. Of course, what we call 'facts' are already inflected in some way by theory, as Jarvie argued, or by cultural bias (as Mary Douglas might put it). Everything is indeed, as phenomenology claims, a matter of perception. But there remains observation based on curiosity, as Barth maintains, so that not everything is lost in solipsistic subjectivism. Pathan leaders in the Swat valley operated in certain observable competitive ways and had resources to hand based on class differences. If we were to compare political competition in Mount Hagen in Papua New Guinea with competition in the Pathan case, we would soon find that these two societies had different structural bases. Pathan society was more stratified, Hagen society more egalitarian. Therefore, while in both cases leaders may have sought to maximize their advantages, both cultural rules and economic constraints were different in the two cases. The result was that accumulation of power could be greater in the Pathan case, and the use of violence was orchestrated in accordance with this point. The Pathans had, and have, 'warlords'. The Hageners experienced something like an emergence of these characters with the introduction of guns into their warfare, but their

egalitarian ethos leads them to make peace by compensation payments more readily than happens with the Pathans. Maximization and power, then, mean different things in different societies, so that power is not a standard quantum, but is also a matter of qualities. Marxist theory would make the Hageners more like the Pathans; trying to make the Pathan egalitarian would make them more like the Hageners than they are.

An unismatic anthropology is not a-theoretical or anti-theoretical. It simply is one that will put theory to its uses rather than the theory putting things to its use. Here we arrive also at one of the things we mean by talking about a mindful anthropology. It would be mindless to imagine that we can do without theory, just as it is mindless to suppose that theory must dominate data. A mindful practice is to bring theory and description together in such a way that a further truth or insight emerges from their interactions.

CHAPTER 10

For a Mindful Anthropology

Abstract Mindful anthropology brings together theory and description. Thus, whatever theory we adopt should apply cogently to the data rather than being foisted fashionably on it. It is based on engagement with the data, and a raising of consciousness about it. We must not allow theory to become a straitjacket or a dogma. As new theories come up, practitioners should not reify them. A mindful anthropology remains open-ended in its views of theories, and is open to new twists of analysis. Mindfulness maps well onto embodiment theory as well as phenomenology, and depends always on detail. Mindful anthropology promotes creativity and thoughtful analysis, focusing on people and their actions. Mindful anthropology is not a rigid form of theory but an orientation towards all forms of theorizing and analysis.

Keywords Consciousness · Details · Dogma · Engagement · Marxism · Now time

Mindful anthropology is not a theory, but an aspiration for the discipline as a whole which can influence the ways in which theories are constructed and evaluated. Mindfulness implies keeping in mind that our topic of enquiry is the lives that people lead, so the investigation has to be people-centered, and it has to take into account that we deal both with individuals and with collectivities. This may seem a needlessly obvious

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statement, but our focus is on understanding society as something that, as Fredrik Barth has said, "happens between people". By keeping our empirical attention at this level we can avoid taking flight into abstractions. This in turn does not mean that we restrict ourselves to the sphere of descriptive work. It does mean that whatever theory we adopt should be shown to apply cogently to our data rather than being flourished or waved at the data, foisted upon the facts, or added seductively to them by contiguous word magic. What we would like to add to this position is a further element, particularly recognizable today, a sense of 'engagement' with issues. Engagement is not the same as 'activism'. Activism implies a degree of partisanship. Engagement by contrast implies an open-ness of assessment but an attention to political and moral issues that any piece of contemporary research can potentially be implicated in.

When Buddhist writers refer to the mind and mindfulness, they are talking about raising levels of consciousness, and saying that this consciousness should apply to daily practice. Attention paid in the 'now' time to whatever action we are taking can give us a greater appreciation for such actions and improve the ways we do them, as well as the ways we interact with others who are involved. The same idea can be applied both to the conduct of fieldwork and to the writing of our account built on fieldwork. Engagement means we do our best to give meaning and find meaning in people's actions and the processes that they give rise to. It may lead us in the direction of an aspect of grand theory (structuralist, Marxist, functionalist, cognitivist), but it is free to take theory in any other direction that is illuminating, or even to combine parts of theoretical approaches to illuminate different facets of the material. Such a 'free' approach to theorizing is, of course, anathema to adherents of 'one theory to fit all' viewpoints, but our argument is that we must be constrained by and faithful to our observations, but we must not be constrained by a theoretical straitjacket. Consider what happened to Marxist theory in the 1960s and subsequently. Marxist theory was used to dismantle the remnants of structural-functionalist theory, but over-reached itself subsequently by claiming an exclusive dominance and pushing out every other theory. One of us (AJS), while teaching and serving as Head of Department in University College London, took a considerable interest in the trends towards Marxist theory and produced work using parts of it (e.g., Strathern 1981; reissued 2009). His enthusiasm, however, waned when he saw how Marxist anthropologists were intolerant of other positions, and were basically working with political activism to take over

Departments. The Marxist trend was then superseded successively by post-structuralist, postmodernist, interpretivist, and most recently cognitivist schools of thought, all of which back away from analysis in terms of class and exploitation and move towards reflexive and occasionally meditative positions. Postmodernism, however, proved too vulnerable to the doubt that it itself generated. Interpretivism, the search for meaning, also assisted in its own transcendence when cognitivist viewpoints reasserted a kind of scientific approach as opposed to a humanistic standpoint. Throughout all this, the works that have proved most resilient are ones solidly grounded on both fieldwork and imaginative thought placing the work in a comparative context.

What, then, of breaking the frames as a mark of movement and change in our discipline? Our argument has been that as new theories come up, they have tended to do two things: declare a break with the past and impose a new dichotomy on the present. While this may be seen as an unfolding dialectical process, the danger is always that the new frame quickly becomes reified and then slips into dogma. A mindful anthropology must always resist such a trend, in the name of mindfulness itself. It must also refuse to be seduced by dichotomies and look to cross-cut them with recourse to our own thoughts. Referring again to a conundrum in the ethnography of the South-West Pacific, whatever became of the 'individuals' who appeared so clearly and vividly in an early generation of writings on the New Guinea Highlands? In another generation all the individuals vanished and were replaced by dividuals, at least on the pages of anthropological monographs. Subsequently, emphasis on the dividual as a somewhat static stereotype has receded in the face of greater concern about the pressing importance of understanding change, violence, peacemaking, Christianity, and transnational influences. Our own modest intervention in the personhood debate by proposing that people are *relational–individuals* did not exactly replace the dividual fad, but perhaps it helped to make it possible for ethnographers to move off in a number of directions in search of new frames, without having to pull their forelocks to paradigms past.

A modifying word here. It pays not to genuflect before the past, but it pays also not to scorn it, especially in relation to the central historical players in the creation of theory. At this stage much can be learned by actually reading the works of, dare we declare it, Frazer, or Radcliffe-Brown, or Levi-Strauss, for the many mindful insights they produced in bending their thoughts to classic topics such as dying gods, taboos, and

shamanism. Mindful anthropology needs to embrace their work as well as continuing to break out from it into worlds of our own creation.

Often, in analysis, a single twist in conceptual approach can have large implications. This has been the case in a number of contexts in which we have shifted a focus of interpretation in a different direction. For example, we argued against characterizations of New Guinea Highlands societies as marked by sexual antagonism by pointing out important ritual contexts in which males and females collaborated and played complementary roles in our *collaborative model of gender relations.* In another context, discussions about embodiment, we pointed to the sophisticated concepts of body and mind that are exemplified by the views of the Hageners on how humans are configured in relation to their bodies. Discussing the concepts of will and agency, we again set New Guinea ideas in terms of a cross-cutting comparison with a wide range of cases. In a historical examination of changes in patterns of conflict in Hagen, we stressed the continuing importance of compensation payments for killings by comparison with state methods of control, in *peace-making* discussions. Each of these contributions pushed understanding of these societies in a definite direction, in contrast with other analyses. These kinds of debates are, of course, the stuff of academic activity and progress. The difference here was that we were in each case thinking outside of the box of current ideas on these topics, so as to open up new possibilities of thought (Note 1).

Mindful anthropology aims therefore to be an enhanced version of thoughtful anthropology. It also encompasses what has been called reflexivity. Reflexivity entrails thinking about oneself. Mindfulness entails more of a suspension of the self while thinking about all aspects of a phenomenon. Of course, the self is always still involved, but it is not the central focus. Mindfulness is therefore a kind of orientation towards observation, using all the aspects of one's embodied faculties. Here, then, mindfulness maps well onto embodiment theory. Mindfulness is also very close to phenomenology, trying to reflect on one's observations while still making them.

The relationship with phenomenology can be explored a little further. Phenomenology is about perception. It deals with 'things as they are', that is, things as they present themselves to the subjectivity of an observer. Mindful anthropology aims to go one step further. Our observations are inevitably subjective, but they are better if we carry them out in a concentrated and directed way. Concentration on the embodied experience of doing fieldwork, of keeping our minds open to what we are training our

sights on, is the pathway to mindfulness. Naturally, we are likely to bring to bear on our observations a background of theory and comparison derived from our professional training. However, we should try to hold the specifics of any theories we have encountered in the background and keep what we are actually observing or participating in firmly in the foreground. This procedure will enable us to follow whatever clues suggest themselves to us. The importance of this relates to minutiae of observation, translation, or interpretation that can have an immediate effect on analysis. Whether it is in the sphere of understanding the cosmological ideas of the Duna people or the detailed specifications of kin group affiliation among the Melpa people in Papua New Guinea, it is seemingly small details that contain large clues to turning around major modes of interpretation, and it is in interrogating such details that the nuances emerge. If, by contrast, there were to be an immediate theoretical straightjacket placed on the data, such insights could not emerge. Theory would then preempt the arena, and in doing so would defeat itself. Quite often, when one has been explaining the intricacies of a piece of fieldwork to an interlocutor, the listener will ask: "That is all very interesting, but how do you theorize it all?" A quest for theory is entirely appropriate, but too often the question reveals that the questioner has not been listening. Instead, they want to know about a particular theoretical position *before* hearing the findings so that everything can fit under the theory. Our approach is the opposite: let the theory emerge out of the account, bearing in mind the potentialities in the data. This is also a part of what we call mindful anthropology. A mindful approach enables us to consider materials creatively, to look for the unexpected, to find a different way of considering the problems. The challenge for a mindful anthropology is how to infuse into graduate training programs this kind of creative spirit, in the face of the fact that increasing 'professionalization' (as it is called) moves everything in the opposite direction. Graduates become products, rather than producers of new ideas. Training is, of course, very important, but the most significant training only happens in field situations themselves. How much of the coursework that students are required to take turns out to be relevant for their further development? This, of course, is a question that cannot have a simple answer. Background knowledge is important, and this is what coursework is supposed to give. The question of relevance continues, however, into further realms, for example, that of the awarding of grants for fieldwork. In one case we know, two anonymous external evaluators out of three said a student's project was

excellent and should be funded. The third evaluator said that they were not convinced the student had enough background in the study of capitalism in general to receive funding – this for a proposal that was primarily about religious conversion in China. How much would have been enough? And why was the 'background' deemed more important than the foreground of the topic itself, its theory, and its methodology? Competition for grant funding can become so extreme that *any* negative view can be used to prevail against even a well-informed majority of positive views. This is not rational, it is not fair, and it is certainly not mindful, especially it is not mindful of the futures of students whose work receives an unjustified evaluation of this kind.

This example reinforces much of what we have expressed in this book. We want to conclude by drawing briefly on the work of a historian, G. R. Elton, in his book *The Practice of History* (Elton 1987). He wrote (p. 131): "History does not exist without people and whatever is described happens through and to people. Therefore, let us talk about people, by all means imposing categories on them and abstracting generalizations from them, but not about large miasmic clouds like forces or busy little gnomes like 'trends.'" What Elton says about history here applies also to anthropology. Forces and trends can certainly be invoked, but human agency is what always underlies and constitutes them, so it is to that agency that we must mindfully address ourselves in our ethnographic work and our theorizing.

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Note

1. The following are a set of references from our own work on a number of topics that we have developed in what we see as a mindful approach to central issues in analysis: Relational-Individual; Collaborative Model; Human Transplacements; Disaster Anthropology; Bodily Humors and Substances; Ritual and Performance; and Peace-making and Conflict:

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