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# Books

## Fighting with Water in Northwestern Portugal

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*Partager l'eau: Irrigation et conflits au nord-ouest du Portugal.* By Fabienne Wateau. Paris: CNRS Editions, Editions de la Maison des Sciences de l'Homme, 2002.

The province of Minho in northwestern Portugal is one of the best-researched typically peripheral areas of Europe. It has been the locus and focus of prolonged ethnographic fieldwork by Portuguese as well as foreign anthropologists. There are studies on the peasant worldview, the stem family, life transitions and changing family dynamics, manor houses, rural development, migration, and gender. The image of the area that emerges from these studies is one of marginal agriculture by conservative and individualist smallholders who grow corn, vegetables, and grapes on scattered tiny parcels of land (minifundia) and live in (semi-)dispersed settlements. Minho is also an area of out-migration and of emigrants maintaining close ties with their native communities by buying land, building houses, and returning "home" for the annual patron saint's festival. In this remote corner of Europe we find people struggling against depopulation and social disintegration brought about by the state, the market, and the European Union.

*Partager l'eau* brings a surprising element—conflictive irrigation—into this general picture of Minho. Based on five successive summers of fieldwork in a rural district with a population of 10,000, this ethnographic monograph departs from a striking paradox: water for irrigation is a highly conflictive resource in this water-abundant area. Why should this be so? Moreover, if the Minho smallholders (in contrast to their colleagues in the semiarid and arid parts of the Mediterranean) do not fight for water as such but rather fight each other with water as a means to other ends, then why water as a medium?

The book consists of six parts. The first introduces the land and people, discusses strategies of acquiring water and land, and goes into the need for communal labor to maintain the irrigation channels and basins, which are used only during the summer months. A striking feature of Minho society is the dominance of the female gender: women prevail not only statistically (there is an average surplus of women of 15%, in some mountain villages and hamlets even 40%) but also culturally and socially because water rights are transmitted through women. Matrilineality, matrilocality, and matrifocality are the key terms of summer sociability, which very much revolves around irrigation.

Part 2 examines the complexity of irrigation relationships, systems of water distribution, and rotation principles. The local motto seems to be: Why do it in a simple way if it can also be done in a complicated one? Here the author could have discussed the notion of involution as proposed by Clifford Geertz in his study of agriculture and ecological change in Indonesia. An extreme pointillism is indeed part of what could be called irrigational involution in this part of Portugal. Part 3 describes the technology and symbolism of mnemonic devices in measuring irrigation time as well as the logic of turn taking in the tapping of water.

Part 4 deals with rules of transmitting land and water rights from one generation to the next and considers the shift from impartible to partible inheritance. The author uses the notion of "water kinship" or "water filiation" to indicate the importance of water in creating lasting ties through females. The main topic of parts 5 and 6 is water conflicts. It is during the irrigation season, which is a time of thick sociability, that tensions come into the open, frequently in the form of ritualized challenges and provocations. Inspired by Simmel, Coser, Freund, Caplow, and Bourdieu, the author develops a neo-functionalist perspective on conflict and defiance which she illustrates with three extended cases. This leads her to a discussion of internal and external forces of change.

The strength of this study is clearly in the elaboration of ethnographic detail and the evocation of rural life during the summer season. You can almost hear and smell the water running through the irrigation channels. Written in the best tradition of French ethnology and human geography, *Partager l'eau* breathes ante-postmodernism, recalling a time when writing (and reading) ethnography was still rather unproblematic. This impression of "good old ethnography" is reinforced by the strong presence in this study of Marcel Mauss, in particular in the notion of water as a "total social fact" and in the idea of seasonal variations of life. Charming as such ideas may be, they have their drawbacks. There is always an element of arbitrariness in the selection of a "cultural focus" à la Mauss. The author's claim that the collective identity of this Minho society rests on water rights is probably too strong and may have been inspired by the fact that she conducted her fieldwork only during the irrigation season. Participant observation during the annual round of life could have yielded a more nuanced representation of local society.

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A second drawback is a failure to situate the book's functionalist perspective on defiance and conflict in Minho in terms of the largely Anglophone literature on "atomistic" peasant societies. The agonistic view of life which the author attributes to the people of Minho was first described by Ernestine Friedl in her pioneering study of a small Greek village and further elaborated by Julian Pitt-Rivers in his work on honor and shame, and more recently David Gilmore has argued that interpersonal defiance in Andalusia can work to enhance, rather than weaken, the stability of the local community. From these studies it is also clear that the ethos of agonism is linked to a set of convergent material circumstances. This interplay between ethos and material conditions, between rivalries and water, is not systematically addressed, nor is there a convincing account of how water conflicts fit into the more general pattern of defiance, for instance, competitive drinking in bars. This is also the reason that in the end there is no satisfactory answer to the question "why water?" Nevertheless, *Partager l'eau* is a valuable resource for those interested in the politics of irrigation and water management and in the transformation of rural communities in the fringes of Europe.

# Learning from the Yoruba

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*Science and an African Logic*. By Helen Verran. Chicago: University of Chicago Press, 2001. 277 pp.

*Science and an African Logic* adds to the growing literature in what are called "postcolonial" science studies. The stated goal of the author, currently senior lecturer in the Department of History and Philosophy of Science at the University of Melbourne, is to present a new understanding of numbering, generalizing, and certainty and a new mode of viewing them.

Verran's methodology is a central focus of the book. She first presents her original encounters with Yoruba elementary school students and teachers in her role as a science and math educator. Then she presents papers emanating from these experiences, followed by critiques of these papers as she moves, and hopes to move the readers, toward new explanations and understandings. Her encounters in Nigeria took place starting in the late 1970s; the papers were written in the 1980s, although many of their footnotes are later emendations. She characterizes the final form of her critique of her interactions, writings, and changing beliefs as "transitional and transactional," involving decomposition following Marilyn Strathern (1992). Thus, the reader must try to follow the author carefully and *fully* as she moves back and forth in time and in philosophical positions. This approach is frustrating because some of what is read is later derided, discarded, or repeated. For example, we read at the beginning of the ninth chapter of eleven: "*In cartoon style* [my italics], I have been contrasting two versions of foundationism that I style universalism and relativism." Chapter 9, we then learn, is a paper she published in 1990 as "a final synthesis of my project of demolishing the universalist account of natural number" and, further, that chapter 9 will repeat portions of what was written in chapters 3 and 6 (which are, respectively, a 1986 manuscript and a paper published in 1987)!

Verran describes the words used for numbers among the Yoruba, particularly focusing on the implied base of twenty with subsidiary bases of five and ten, as contrasted with English number words with the implied base of ten. Where the word *seventy-five* implies $[7 \times 10 + 5]$, the Yoruba word for the same value implies $[-5 + (20 \times 4)]$, and for the same value as our *seventy-two* their word implies $[+2-10+(20 \times 4)]$. She also states as an important difference that among the Yoruba there can be multiple expressions for higher values. Some of the expressions are said to be considered more elegant than others, but we are not told which ones or why.

In her 1986 manuscript (chap. 3) Verran claims that the reason graphic numerals evolved for Indo-European number systems and not for the Yoruba system is that the Yoruba multibase word form leads to ease of mental calculation while the base-ten form does not. Later she critiques this comparison by saying that it showed that she was trying to explain away differences. I, too, am critical of the statement. The issue should not be *why* the Yoruba did not develop along the same lines as we did. Further, to imply, as she does, that numeral recordings developed for purposes of ease of calculation is to ignore what is known about the recording systems of, for example, the Inca, the Mesopotamians, or the Maya.

Although not easily encapsulated, Verran's major point seems to be that for Yoruba-speakers and English-speakers there is a fundamental difference in the way material is manifested. It is this difference that gives rise to different conceptualizations of number. For the Yoruba, numbers refer to collections of material that can be subdivided into other collections, whereas English-speakers characterize material as units having length, area, capacity, and weight.

Despite Verran's extensive discussion, Yoruba numbering concepts remain skeletal. There is no cultural context surrounding and fortifying them beyond what she extracts from classroom experiences, structured interviews with children, and syntactic analyses of Yoruba utterances. (The children differ in educational levels and in their knowledge of Yoruba and English.) This sharply contrasts with some books with which she identifies her work. *Intimations of Infinity* (Mimica 1988), for example, has as a major concern the relationship of number and the counting system to the Iqwaye view of the cosmos. To me the best of such books, although dealing

primarily with space/time rather than number, is *The Anthropology of Space* (Pinxten, Van Dooren, and Harvey 1983) which shows the Navajo's conceptualization to be intimately connected to their worldview.

Much of Verran's discussion of numbering and certainty relies on contrasts with "English-language" numbers and with the ideas of "English-language-speakers." But "English-language-speakers" is not a singular group. Even if it were, with regard to ideas about number, space, time, and certainty, there are numerous subgroups within it. There are decided differences between, for example, the views of members of the public-at-large as they act in their real daily world and the subject matter and views of professional mathematicians, philosophers of mathematics, and working scientists. (*Cognition in Practice* [Lave 1988], which is not cited, is one of the very few books about mathematics in daily practice among an English-speaking group—namely, American shoppers.) As a result, I find her characterization of the views of English-speakers unconvincing and similarly find unconvincing her models and presumptions about how English-speakers come to their system of numbering.

Verran's final position, one that I believe is important, is what some call "a deeper relativism." The article "Mathematical Ideas and Indigenous Languages" can help put Verran's work in the context of other writings during the 1980s and 1990s. The article is a thoughtful overview and discussion of this emerging view of the relativity of mathematical ideas. The authors are Bill Barton, a mathematics educator who works with the Maori, and Roslyn Frank, a linguist and Basque specialist (2000). Both are involved in ethnomathematics. Although Verran seems unfamiliar with the contents and scope of ethnomathematics, the book falls well within that endeavor. As a field of study, ethnomathematics began in the early 1980s (see, for example, Ascher 1991 and D'Ambrosio 1985). The basic tenet uniting the field is that mathematical ideas and their expression are neither culture-free nor culture-neutral.

Verran's desire to revise her views in light of her encounters with Yoruba students and teachers is laudable. In particular, her odyssey, although mired in the telling, highlights the conceptual mismatch that has so often been found when educational paradigms that arise in Western-derived educational settings are brought to bear elsewhere.

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# The Americas: Perhaps Square But Never Boring

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*Quadratura Americana: Essai d'anthropologie lévi-straussienne*. By Emmanuel Désveaux. Geneva: Georg Editeur, 2001. 641 pp.

This book is the result of a monumental enterprise and a testimony to limitless ambition. At the end of more than 600 pages of relatively small print, Désveaux concludes that each original native culture of the Americas, South and North, represents but one particular instance or moment within a single general system of transformation. Ultimately, every detail of any society between Alaska and Cape Horn is to be considered and understood in its relation to each and every detail of any society between Alaska and Cape Horn. In other words, as is implied by his choice of subtitle, Désveaux tries to expand on what Lévi-Strauss said about American myths to include ritual and many other somewhat more "concrete" aspects of social organization. This is structural analysis on a very grand scale.

In four sections and 25 chapters, plus introduction and conclusion, the book covers an astonishing array of ethnographic situations on which the author offers multiple comments and analytical considerations. Whether discussing cannibalism or Inuit adoption, Californian basketry, Yupik masks, or Miwok marriages, the surprising absence of alcoholic beverages in North America or the appearance of what he calls "pseudo-Dravidian" kinship terminologies, and whether revisiting the intriguing case of the Natchez or the well-known myth of the man chasing parrots, Désveaux communicates with passionate energy his enthusiasm for the field. Readers may be reminded of the fervor typical of the founding ancestors of anthropology. The author shows a deep fascination with

the native inhabitants of the Americas. The breadth of his interests is impressive, and his knowledge of the ethnography often seems remarkable, but critics are likely to make distinctions in this abundance of interpretations: some of his insights are unquestionably brilliant, others seem much less convincing, and a few would almost appear downright silly. It will be for the experts and specialists in each cultural context to discuss the details and to debate with Désveaux so as to prove him right or wrong, but they should all be grateful, for this book has certainly given them much to think about.

A book review should avoid falling into narrow discussions of too specific issues or playing superficial psychoanalytical games—commenting, for example, on how Désveaux expresses profound admiration for the work of Claude Lévi-Strauss (considering the *Mythologiques* the masterpiece of anthropological analysis of the past century) and at the same time allows himself to criticize his mentor with an attitude which connotes an arrogance bordering on rage. One could note, however, that it requires some nerve to dismiss Lévi-Strauss in a few lines, claiming that he never truly understood the logical consequences of his own analyses and promising to improve on the old master.

I would simply say that this book has the power both to fascinate and to irritate, often on the same page. Désveaux provides stimulating intuitions and, at times, meticulous consideration of the facts, but he will certainly be criticized for much the same qualities and faults which have, in the past, been invoked by reviewers of the *Mythologiques*. For example, his mastery of the ethnographic information and the intellectual elegance of many of his arguments are often admirable and sometimes even prodigious. But too many corners are cut very short indeed, mostly as a result of an annoying tendency to extract out of context whatever ethnographic information fits the argument at hand.

More important, perhaps, is the broader theoretical statement that the cultures of both continents, North and South America, should be understood as part of a single system of transformation that takes the form of a series of quadrants, relations systems of four oppositions: hierarchy/sexual duality, continuity/discontinuity; or myth/ritual, kinship terminology/social organization; or again tattooed face/naked face, mask/cap, and so on. Edmund Leach, who was once a careful and sympathetic reader of Lévi-Strauss, would have called this gibberish and quickly recommended that it be dismissed as useless. Without being so rash, the least we should ask is whether the use of such formal abstract logic, translated here in the shape of quadrants, helps us in any way to understand the way people live or whether it simply demonstrates that it is possible to look at ethnography and then phrase it in this peculiar way.

Nevertheless, on many specific questions familiar to specialists of the Americas, Désveaux's interpretations will appear original and helpful, mostly because he often proposes novel ways to look at old problems or, more simply, because he brings back to our attention issues left unresolved. Perhaps his most interesting suggestion is the claim that there was something so original and unique throughout the native cultures of the Americas that the general conclusions from anthropological analyses there may not be applicable elsewhere, and vice versa. This is how Désveaux concludes that Lévi-Strauss's earlier work on kinship, inspired by the sociological functionalism of Rivers, has been largely contradicted by his later work on myth, closer to the position defended by Kroeber. In fact, he is almost suggesting that the *Mythologiques* represent a by-product of native American myths. I would have added that structural analysis is an American way of thinking.

It would be difficult to find a more vibrant tribute to the genius and industry of the native inhabitants of the Americas. The book ends on a recognition of the profound melancholy felt by anyone who knows the history of the continents. To Emmanuel Désveaux, beyond intellectual fascination, this is the source of an incurable wound. For reasons that must be obvious, it would be valuable to translate this book at least into Spanish, Portuguese, and English.

# Religion, Evolution, and Cognition

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*In Gods We Trust: The Evolutionary Landscape of Religion*. By S. Atran. Oxford: Oxford University Press, 2002.

*Darwin's Cathedral: Evolution, Religion, and the Nature of Society*. By D. S. Wilson. Chicago: University of Chicago Press, 2002.

In recent years, cognitive scientists have put forward synthetic accounts of religious concepts based on the premise that, although religious representations are clearly culturally diverse, their foundations are not, consisting in a small catalogue of templates made salient and easily learnable by general properties of human cognition (Hinde 1999, Boyer 2001, Pyysiainen 2001, McCauley and Lawson 2002). If religious representations are grounded in ordinary brain function, what is their evolutionary background? If many cognitive capacities consist in domain-specific adaptations to problems of adaptive relevance in ancestral environments, does this extend to the creation of supernatural agency and religious ritual?

Atran's and Wilson's books respectively express and defend diametrically opposite ways of considering this issue. On the one hand, most cognitive scientists and evolutionary biologists consider religious concepts and

norms not as adaptations but as a probable by-product of adaptations. In this view, religious concepts are comparable to music or visual art; they may "tweak" entrenched ontological assumptions in a way that makes them both salient and learnable. On the other hand, some anthropologists and biologists would argue that the quasi-universality of religious concepts, their ease of acquisition, their spread, and above all their social consequences make the notion of religion as an adaptation at least prima facie plausible.

*Unnamed predecessors and absent inspirations*. Atran's work is a brilliant exposition of the "evolutionary by-product" interpretation as well as a mine of references for empirical research into the psychology of religion. The substance of Atran's argument consists in a combination of three complex cognitive theories developed in other places (and in some cases for purposes other than explaining religion).

The first component is the idea that religious and other supernatural representations are "counterintuitive," that is, express salient violations of tacit assumptions that govern ordinary thinking about such concepts as intentional agents, man-made objects, animals, or plants (Barrett 1996). Concepts that violate such assumptions (e.g., the concept of a ghost, an intentional agent without standard physical properties) are salient and memorable, regardless of cultural familiarity, provided that they also allow rich inferences (e.g., about the ghost as an agent with beliefs, memories, intentions).

A second thread is the model known in social psychology as "terror management theory" (Rosenblatt et al. 1989). Social attitudes are substantially affected by heightened awareness of one's own mortality. After priming with mortality-related themes, subjects are more punishing, less tolerant of ethnic differences or misfortune, and more attached to cultural symbols than controls. Naturally, symbolic promises of immortality can also be construed as a terror-management tool (Florian and Mikulincer 1998). This model gives some empirical substance to the "comfort against uncertainty" interpretation of religion popular ever since philosophers first reflected upon religion.

A third component relies on a logic of commitment developed to explain a variety of economic behaviors that deviate from utility maximization (Schelling 1960). Subjects in experiments or agents in markets often seem driven by "passion" or economically irrelevant feelings such as a sense of "fairness." Naturalized economics shows how such apparently suboptimal drives make evolutionary sense and help bypass rational obstacles to long-term cooperation (Frank 1988, McCabe and Smith 2001). This logic of commitment also makes sense of other human behaviors from self-damaging spite to punishing attitudes to the willingness to undergo painful ordeals to join particular groups or factions (Ridley 1996).

Atran uses these three ingredients to put forth an impressive picture of religious thought and behavior as probable by-products of evolved mental capacities. This composite picture is quite close to that presented by others (see, e.g., Hinde 1999, Pyysiainen 2001), suggesting an emerging consensus in the field.

The cognitive account aims to explain the compelling character of religious understandings. Most religious beliefs are justified by the authority of *unnamed predecessors* such as "the ancestors," whose standards the living are supposed to live by, or by *absent inspiration*, a supernatural agent supposed to talk though some privileged medium. This, however, is the rationalization of inaccessible cognitive processes whereby particular thoughts are made both salient and plausible. Atran's work is a striking demonstration of how these processes operate.

*Obscuring the origin of one's own thoughts*. In religious concepts as in other domains, most relevant cognitive processes are simply not accessible to conscious inspection. In other words, people do not necessarily believe what they believe they believe. In religion, this was demonstrated by Justin Barrett's research into concepts of supernatural agency (Barrett 1996). Although people's explicit concepts of divine agents are focused on extraordinary properties (such as the capacity to attend to many events at once or to be everywhere), their intuitive expectations are based on a more intuitive concept of intentional agency similar to that routinely (and tacitly) used by our "theory of mind."

This is a crucial point, for a great deal of anthropological reflection consists in attempts to give a rational or at least sensible interpretation of consciously accessible formulations of religious concepts and norms. If the cognitive results are to be trusted, such explicit formulations are only a very fragmentary aspect of religious cognition and for that reason a highly unreliable basis for explanations of religion. Explanation requires an entirely different kind of evidence, produced by controlled experiments aimed at processes beyond conscious access.

Atran's book provides many examples of this phenomenon, whereby the explicit content of one's thought does not transparently reveal its origins. A central point of "by-product" accounts like Atran's is that religion is generally *parasitic* upon capacities that would be part of the human cognitive equipment, religion or not. Religious morality associates supernatural agency with intuitions that develop with or without such agents (Frank 1988). Religious affiliation recruits coalitional capacities that operate outside religion (Kurzban, Tooby, and Cosmides 2001). Religious notions of sacred and pollution owe their emotional tenor to evolved fears of contaminants and pathogens (Boyer 2001).

*For the benefit of the group.* Wilson's arguments, contrary to Atran's, are not rooted in an emerging consensus—quite the opposite. In a rather heroic mode, Wilson combines marginal (or, to some, heretical) biological models with a certainly original but not very popular view of the social effects of religion. Wilson is well-known among evolutionary biologists as one of the few theoreticians convinced of both the possibility and the importance of group selection as opposed to the individual- or gene-level selection model offered by the New Synthesis. Most biologists would concede that group se-

lection is in principle possible, although most also assume that such effects are in fact very rare.

The book extends this notion to put forward a speculative interpretation of religious beliefs and norms in terms of their consequences for social exchange and cooperation. Wilson contends that religious ideologies provide group cohesion and group solidarity of a kind that transcends rational, opportunistic utility maximization. However—and this is the crux of the argument—heightened trust and cooperation supported by religious morality are exclusively in-group advantages. Wilson describes the sharp boundaries of religious affiliation as the limits of morality-informed and trust-based social cooperation. He also describes the many social advantages of such exclusive solidarity and the selective models that would account for its evolutionary stability.

The model is based on three assumptions: that group selection does occur, that religious ideologies do promote intragroup solidarity, and that religiously based groups are genetically isolated in such a way that fitness differences could appear. The first is beyond the limits of this review, although it must be noted that gene-selection models of altruistic behavior are a much-favored, conceptually coherent alternative to this group-level account.

Wilson's (quite justified) complaint that the study of religion is not scientific enough could be with some justice turned against its author. A major problem with his account is the very notion that people belong to exclusive groups defined as "religions." Wilson describes as the standard situation the case of groups with a coherent, institutionalized, and integrated religious ideology and typically a body of specialists that maintains this doctrinal coherence. Now this situation, far from being general and common to religious ideologies, is recent, exceptional, and unstable, as anthropological and historical evidence amply demonstrates. The integration of religious concepts into a coherent and explicit doctrine is only one of the possible ways in which religious ideas are transmitted. It appeared only with the development of complex states with institutionalized guilds or corporations of religious specialists. Before or outside such state structures, what can be observed is locally based modes of interaction with supernatural agency. In such groups there certainly are ethnically based trust signals, but these have nothing to do with religious doctrine. Therefore distinct "religions" with their distinct codes may be too recent an invention to be of any evolutionary importance. Established or organized religion is a very poor example for the evolution of religion, since it depends on very recent social arrangements.

Wilson's argument requires that the religious group members' behaviors be informed by the officially sanctioned moral code. This is where the empirical support for his claims is less than compelling. Wilson tends to treat religious normative codes as descriptively reliable. Although he clearly sees the difference, he does not seem to appreciate that extensive empirical research has documented the gap between norm and behavior. Empirical studies of trust and cooperation suggest that people calibrate cooperation on the basis of complex trust signals (Bacharach and Gambetta 1999) and assumptions about exchange (Henrich et al. 2001) that do not quite follow the lines of religious affiliation. In other words, religious affiliation in some situations may provide some reliability signals that facilitate exchange, but there is no evidence that this is an automatic or general effect of comembership in a religious group. It is difficult to see actual economic effects as generally supporting the in-group-benefit hypothesis.

Wilson assumes that explicit religious concepts are the whole of religion, although they are like other conceptual domains organized by principles that largely escape the believers' awareness. The believers' notion that trust and cooperation are based on common religious affiliation may reduce to a rationalization of cooperation mechanisms that they are not aware of and that the commitment literature may help to explain.

*Whither the science of religion?* Much progress has been made and much remains to be done in the cognitive study of religion in terms of the connections between mortality and religion, between commitment and intuitive economics, between natural ontology and counterintuitive concepts. One major difficulty is that cognitive hypotheses cannot be properly evaluated against the anthropological literature, since this requires experimental evidence. The value of this paradigm should become clearer as a second generation of studies explores the empirical implications of these models.

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