

Science, Erudition and Relevant Connections

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Paulo Sousa makes a strong case for applying the best anthropological models of cultural transmission (epidemiological models) to anthropology itself, to the ways in which anthropologists' choices of topics and methods have changed. Sousa's model of how kinship got gradually pushed away from its central position in anthropological inquiry is quite persuasive. Here I only propose a slight modification of this general model, in the hope of making some of its predictions more specific.

As Sousa points out, scientists' general statements about their own field generally consist of interpretative statements that cannot be taken as a straightforward expression of what they actually do. Also, most scientists and academics are generally unaware of (or unconcerned with) the dynamics of *authority transmission* that organise their own field, that is, the set of criteria that people actually use when deciding that a given person is a member of a professional guild or community.

Authority transmission is important because it has crucial consequences for how fields evolve. Scientific or more generally academic and scholarly activity is a highly regulated social activity. Each specific community (generally co-extensive with what is called a "field") has shared criteria for who is allowed to join and what counts as a valid contribution. In the same way as a guild, members of a "field" protect their common interest (the reputation of their activity) by restricting entry to those who fulfil certain conditions. In the case at hand, this means the set of criteria used to decide that this or that graduate students have become anthropologists. With a little idealisation, this amounts to: How does the community of

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anthropologists actually decide that this person could be considered for a job as an anthropologist, or decide that their publications count as contributions to anthropology?

In terms of such criteria, it seems to me that the opposition between "scientific" and "non-scientific" modes, or perhaps "humanities" vs. "science" are too simple, and that there are *three* clearly distinct ideal types here. I call them ideal-types because I do not want to suggest that any discipline or field is organised exclusively around one of these possible modes of transmission, although in many fields one of them is clearly dominant. I will call these three modes *science*, *erudition* and *relevant-connections* respectively. In what follows I will try briefly to describe these three modes before returning to the specific case of kinship and anthropology.

The Scientific Mode

The 'scientific' mode should not take too long to describe. This is not because scientific authority and authoritativeness are simple matters, far from it. Philosophy of science is difficult precisely because it is not easy to explain what this particular mode of transmission consists of, what really makes it different from (and vastly more successful than) all other ways of gathering knowledge. This however does not matter for our purpose, because the scientific mode, if difficult to explain, is very easy to recognise. You know it when you see it. Here is a short list of the common "symptoms" by which we recognise a field organised by this particular mode of transmission:

- (1) There is an agreed corpus of knowledge. What has been achieved so far is taken as achieved by most practitioners. The common corpus also includes a set of recognised methods, and most important for my purpose here, a list of outstanding questions and puzzles to solve. People also tend to agree on which of these questions are important and which only require some puzzle-solving and some tidying up of the theoretical landscape.
- (2) The fundamentals of the discipline and its results are explained in textbooks and manuals that are all extraordinarily similar, as the essential points and the way to get there are agreed in the discipline.
- (3) It does not really matter who said what or when. Indeed, many practitioners have a rather hazy picture of the history of their dis-

ciplines. Many young biologists would have a hard time explaining what the New Synthesis was, who was involved, and why a synthesis was needed in the first place. Revered figures from the past may be a source of inspiration, demonstrating how to make great discoveries, but they are not a source of truth. Darwin believed in continuous rather than particulate heredity and in some transmission of acquired traits – on both counts we think he was simply wrong, great man though he was.

- (4) People typically publish short contributions. They do not need to establish why the specific problem addressed is a problem or why the methods are appropriate, since all that is part of the agreed background.
- (5) The typical biographical pattern is that the aspiring member of the guild is intensively trained from an early age in the specialised field and makes important contributions after only a few years of training.
- (6) There is a large degree of agreement (because of the various features above) on whether a given person meets the requirements for being a practitioner of the particular field, and there is also a large agreement on how important each individual's contribution is.

The Erudition Mode

A second possible mode is characterised by *erudition*, understood as the requirement that specialists of the discipline should have extensive knowledge of a (generally extremely narrow) domain of facts. Consider for instance Byzantine numismatics or the history of Late Renaissance painting. We expect specialists of these fields to have extensive knowledge of the corpus of coins or paintings. We turn to them to identify new findings. The erudition mode was essential to (and still plays a great part in) the development of many scientific fields. For instance biology started as natural history and still includes a large part of it. Many biologists gain entry to the guild by virtue of their extensive knowledge of a particular family or class of living things. In the same way, many linguists are also philologists in the sense that they have studied and are the recognised experts in a particular language family.

The features of erudition are partly different from those of science:

- (1) There is an agreed corpus of knowledge. There is also a large agreement on what remains to be done. For instance, only a small part of the extant corpus of Mesopotamian tablets has been deciphered. A great number of languages remain to describe. So the remaining tablets or languages are offered to the aspiring specialist as a possible domain of study.
- (2) A great deal of knowledge is not made explicit in manuals. One picks it up by working under the tutelage of more experienced practitioners and immersing oneself in the material for many years.
- (3) The history of the field matters and practitioners generally know it. There are some Masters, whose intuitions matter a lot although they may have been wrong. For instance, to this day (some) linguists read Jakobson or even Saussure, classical scholars certainly know their Bachofen or Straus, religious scholars cite Otto. But they are not considered as infallible sources.
- (4) People publish short descriptive contributions, e.g. the first description of a new insect genus or the phonology of a specific language. They also compile monographs that incorporate vast amounts of information about a particular domain, e.g. the comparative morphology of ant species, an encyclopaedia of New-Guinean languages, a concordance of Ben Jonson's plays, a catalogue raisonne of Guido Reni.
- (5) Age is a necessary component of competence. Older experts are generally better, partly because expertise consists in the accumulation of vast amounts of specific facts, but also because an expert needs the kind of intuition that is only shaped by long-lasting familiarity with the material. Only a seasoned Renaissance scholar can tell you that this painting is closer to the Venetian than the Milanese school. A younger scholar may be misled by superficial features.
- (6) Within a narrow field, people agree on whether a given individual is competent or not, generally based on that person's extensive knowledge of a monograph-sized sub-field.

Science and erudition are frequently found side by side in modern disciplines, like linguistics or archaeology or biology. Academic fields (and their national variants) differ in the particular mix of erudition and science

that is required for admission.¹ Molecular biologists these days are recruited mostly in the scientific mode. Evolutionary biologists by contrast are supposed to have a “field” (e.g. lekking in antelopes, social coordination in wasps) so that erudition is required as well as science. Some linguists work purely in the science mode (e.g. asking what formal models can account for regularity in language) and others are more field-oriented (e.g. describing Amazonian languages) and many do both. There is sometimes a fruitful exchange of information between activities belonging to these two modes. Molecular biology and evolutionary theory feed back into each other. The same is true in some scientific accounts of linguistic evolution, inspired by erudite attempts to classify linguistic families.

Although one can find both modes in the same field, even in the same person, they remain different in terms both of the purpose of people’s activities and the manner in which they are conducted. When they are doing science, biologists or linguists focus on the empirical support that can be given to a particular hypothesis that would explain a set of data. They also create the relevant domain of data either by performing experiments or by selecting relevant evidence from a corpus (e.g. testing the hypothesis that all languages have a noun-verb distinction by going through hundreds of grammars). Erudition is not hypothesis- or explanation-driven but description-driven. For instance the aspirant specialist is enjoined to catalogue all coins found in a particular Byzantine palace (or all forms of this specific genus of orchid) because this particular collection (or species) had not been described before.²

¹Technical change too can have dramatic effects on the mix of modes. Classics used to be strongly based on erudition in the corpus. Knowing obscure (but relevant) textual sources was a *sine qua non*. Now that the entire canon is available (and searchable) on a single CD-ROM, it would seem that this knowledge cannot be used as a criterion for admission. To take an equivalent (non-academic) example, London taxi-drivers used to require “The Knowledge” (of streets) of aspiring drivers, a requirement that may soon disappear with widespread guidance-systems.

²Obviously, there is no such thing as “pure” or “atheoretical” description. Specific hypotheses about what is and what is not relevant are generally embedded in the agreed descriptive methods of the discipline.

Relevant Connections

The third mode is the most elusive one as it has not been systematically described, yet it is also most important to our understanding of many modern disciplines. Here people assess new contributions in terms of the connections they establish between facts or ideas which, by themselves, are not necessarily novel or even interesting. Although this way of judging new work has been around for a long time, it has become characteristic of many academic fields of a recent vintage and of the recent evolution of older disciplines. I will call this "relevant connections."

Again, I should provide examples before a model, because this is a phenomenon we all know when we see it, even if we do not always reflect on the mechanism at work. For instance, a colleague tells me of a recent book that re-frames the discourse of love in Shakespeare's plays and sonnets as an expression of the colonial outlook. The lover's loving gaze transparently expresses the conqueror's prospect on a recently discovered, clearly gendered and mythically virginal New World. A student is planning to work on Indian public executions during the Raj as a form of theatre, a ritualised performance that constructs colonial power at the same time as it undermines it by exhibiting the gossamer of its dramatic texture. Another colleague has recently finished a study of gay fathers in the Caribbean in the framework of Benjamin's and Bourdieu's accounts of culture, technology and late capitalism. Steel drums and cane rum punctuate the local habitus of globalized self-empowerment.

What is the common thread in these disparate examples? They all seem to offer a new connection between elements that were previously known to everyone in the field and indeed, in many cases, to everyone. For instance, all literary scholars presumably know their Shakespeare and all educated people know a little about the conquest of America. But they (supposedly) had never considered Ophelia as an American Indian. In the same way, most historians know about the political organisation of the Raj and its fondness for state pagentry. They are also cognisant, from Diderot or some other source, of the "co-median's paradox." The author's hope is in the fact that the connection is new, between state ceremonial and dangerously precarious fictitious performance. In the same way, most anthropologists or readers of anthropology have some notion of the Caribbean as a place of modernity, of contrasting influence and original cultural mixes. They also

know a little about the various ways in which homosexuality is construed in various places, as well as cultural differences in father's duties or roles. The innovative point is to put all these together, creating new associations, especially by throwing in Bourdieu and Benjamin, two rather dour, bookish and strait-laced dead Europeans that seem far removed, but that may just be an impression, from your typical Trinidadian gay dad.

One could multiply the examples, but it may be of more help to compare the features of this with the other two modes:

- (1) In relevant connections fields, there is no agreed corpus of knowledge. Indeed, there is no "knowledge" in the sense of accumulated and organised information but rather a juxtaposition of different views on different topics.
- (2) There are no manuals, no agreed techniques or methods. Indeed, each contribution constitutes (ideally) a new paradigm or method, each author is an island.
- (3) The history of the field, or rather the reframing of past theories, is crucial. A lot of scholarly activity in relevance-driven fields consists in citing various Masters, commenting on their texts, finding some connection between what one has done and what they said, etc. In cultural and anthropological studies, authors like Walter Benjamin or Pierre Bourdieu or the entire Frankfurt school are part of this Pantheon (a very ephemeral one, with a high turnover rate – Freud and Marx used to be in their place). The Masters are generally invoked as validating authority. That is, the particular fact that one is describing (the gay Caribbean dad, etc.) is presented as illustration of the general principle laid down by Benjamin or some other authority.³ Also, there is a great deal of emphasis on the self-definition of the field, the ideas various practitioners have about what they do and what they ought to do, compared to what others do. Indeed, most important works are supposed to be, not just contributions to the field but also reflections on the field itself. For instance, a study of German post-Expressionist 1960s cinema will be praised,

³Importantly, *these authors are never shown to have been wrong*. Indeed, their work is never discussed as having any connection to empirical fact that could make them right or wrong. Benjamin's conception of culture or Adorno's views about music are what they are, not to be judged as explaining a certain amount of variance in a particular phenomenon.

not just because it tells us a lot that we want to know about that specific genre, but also because it re-frames our views of the connections between cinema or society. A study of recent rap songs is good because it establishes a new approach to popular culture.

- (4) Books are more important than articles. This in part reflects the fact that each contribution should ideally re-frame the field as a whole, introduce a new way of looking at issues, etc., something that cannot be done in a short article. This is also why students are offered *readers* rather than *text-books* to get themselves acquainted with the field.
- (5) There is no specific developmental curve. Some authors produce interesting connections in their first piece of work, others are seasoned specialists of the erudite mode who at some point decide to let their hair down, as it were, and let free-association govern their next project.
- (6) There is no agreement whatsoever on who is a competent performer in this mode, apart from the (generally dead) Masters like Bakhtin or Benjamin or Raymond Williams for cultural studies, Derrida or Foucault for literary critics. A consequence is that there are tightly coalitional cliques and exceedingly bitter feuds about who should get what jobs, who is allowed to publish and where, etc.

In the last three decades or so, some fields have dramatically evolved from almost pure erudition mode to the relevant-connection mode. Consider literary criticism for instance. In the past, one could not really expatiate on Shakespeare's plays without some thorough knowledge of the First Folio and Quartos and other such recondite source-criticism. This kind of erudition is still practiced, but it is not the major criterion of a valid contribution to Elizabethan studies. Saying something *new* about the plays is what matters.⁴ One could say that the specialists have (perhaps excessively) taken to heart Forster's dictum. They only connect.

⁴There are various accounts of why this happened to literary studies, whether this is a Good Thing or not, and in the latter case whether it is all the fault of that awful F.R. Leavis or of the dreaded French structuralists. I am not enough of an erudite to adjudicate between these normative interpretations of history. I can only comment that polemical narratives generally get in the way of a proper epidemiological explanation. Neither jeremiad ("No-one knows the Canon anymore!") nor triumphalist epic ("We have overcome! The Canon is dead") are of great help here.

How does all this work? Outsiders – and disgruntled guardians of the erudite faith, in recently transformed fields – will say that *anything goes* in terms of relevant connections. Although one can see some merit in this interpretation – a null hypothesis that seems to explain a great deal of the evidence – it is not quite sufficient. In a relevant-connections field, people often have a *definite intuition* that So-and-so is or is not potentially great specialist, yet are at a loss to define (clearly, explicitly, consistently) what the criteria are. But this combination of definite normative judgement with no explicit criterion to justify it, is not necessarily evidence for free-wheeling, random decision-making. For instance, we all know that certain sentences are non-grammatical although we could not say why. Having no explicit justification for one's judgement may simply mean that the relevant cognitive processes work on the basis of efficient, definite and *tacit* criteria.

I propose that the criterion of valid contribution is a (tacit) one of *relevance*. I use the term in a fairly precise way, as explained in Sperber and Wilson's "Relevance Theory" of cognition and communication (Sperber & Wilson 1995). Relevance in this sense is not a property of utterances as such but a property of the cognitive processes involved in representing what is being said and what inferences to draw from it. One of the central assumptions of Relevance Theory is that the cognitive processes involved in creating an interpretation of a particular utterance are goal-driven. They function in such a way that they select, among many possible alternatives, one interpretation of the speaker's communicative intentions that produces more or richer inferences with less processing cost.⁵ The prediction is that (a) at equivalent processing cost, an inferentially richer interpretation is selected, and (b) if the inferential is the same, the less costly interpretation is selected.

Relevance, being a general feature of people's communicative behaviour, is of course a feature of communication in the scientific mode and in the erudition mode, as well as in any social interaction situation. But the relevant connection mode is special in that contributions are judged mostly in terms of the relevance of a new mixture of already known bits of

⁵For lack of space I cannot offer more than a very summary description of the framework. All these ideas are defined in a fairly technical and precise way in the original formulation of the theory and in experimental applications (Sperber, Cara & Girotto 1995; Sperber & Wilson 1995).

information. I call this *relevant* connections because I think the authors are judged on the ratio between the extra processing work that their connection requires and the richness of the inferences it triggers.⁶

This accounts predicts, obviously, that associations with more inferential potential will be greatly favoured. Importantly, inferential potential is no at all the same as explanatory power. A great deal of inferences that we produce on the basis of people's utterances may consist in semi-propositional evocation rather than propositional demonstration (Sperber 1994). Think of the many ways in which metaphorical thoughts illuminate a particular situation (the State as a big agent with specific goals) while keeping certain aspects indeterminate (does the State have memories? conflicting desires? does it change its mind? etc.). So in a field dominated by relevant connections, it does not really matter that, for instance, very little of Shakespeare is explained by the colonial outlook, or that only limited aspects of the gay Caribbean dads' behaviour illustrates German social theory. What matters is that at least some aspects of these empirical phenomena, when mixed with the scholarly background, produce novel inferences.

This is why there is a definite trend towards the bizarre and the extravagant. Given a certain stock of relevant connections, new specialists have to produce something more evocative, which often means some less expected association. From colonial Shakespeare or imperialist Jane Austen, it is only a small step to anal retention in Verdi and poetry as orgasm in T.S. Eliot.⁷ It is of course easy to make fun of such phenomena. That is perhaps unfair, though, because relevance-driven modes are essential to the beginnings of any discipline. After all, many classical Greek advances in science and what we call philosophy were driven by the desire to say something new, unexpected, full of import and consequences, about known phenomena.

⁶Note that this fairly technical meaning of "relevant" as a property of cognitive processes is not the same as the ordinary sense in which scholarly work could be "relevant" to one's life, interests or problems. Indeed, many works that are based on relevant connections in this technical sense do not address anybody's pressing existential problems.

⁷I did not make these up. I heard both talks while at Cambridge. It is perhaps not a coincidence that the titles would make much more sense the other way around, with anal Eliot and orgiastic Verdi...

The real problem is not in the exuberance of novelty. It lies, in my view, in another feature of relevance-driven transmission that is predicted by Sperber & Wilson's model. Relevance theory does not just predict that, all else being equal, a contribution with more inferential potential will be chosen. It also suggests that, given equal inferential potential, an interpretation that requires *less processing effort* will be selected. This is, in my sense, a much greater obstacle to progress in knowledge. It is also a crucial factor in what happened to kinship in anthropology.

Anthropology and the Study of Kinship

Why did anthropology abandon the study of kinship or push it to the periphery of its theoretical landscape? It seems that most fields of anthropology, in the past, were organised principally around erudition, and to a small but significant extent around science. Anthropologists used to have a "field," which typically meant a portion of the colonised world that they could declare theirs for the purpose of serious investigation of society and culture. Many sectors of cultural anthropology remained, and remain to this-day largely erudition-driven. But the science mode was also present. Indeed, a lot of cultural anthropology gradually became hypothesis-driven, most clearly in the heyday of British functional-structuralism.

Cultural or social anthropology never quite achieved the kind of consensus on description and evidence that would allow proper scientific hypothesis-testing. However, there was a definite step in this direction. The study of kinship was one manifestation of this trend. It occurred in the one domain that lent itself most easily to the scientific treatment. Kinship terminologies for instance, or alliance networks, could be described in terms that were in principle applicable to many different cultures.

As Paulo Sousa suggests, the study of kinship did not disappear from this eminent position because of internal, theoretical problems with the very notion of kinship or the "foundations" of the field. If, for instance, all that was "wrong" with kinship was that some common terms (e.g. marriage) seem to require a polythetic definition, then the discipline would be in great shape. Indeed, the very description of such a small technical difficulty as a gigantic obstacle or foundational earthquake would strike most scientists as rather outlandish. Modern evolutionary biology for instance is full of

familiar terms – territory, memory, resource, fight, courtship, etc. – that required and in due course received extensive, precise, theory-grounded re-definition. If that was the most difficult part of evolutionary biology, any twelve year-old could be the next Haldane or Hamilton.

No, the main reason why kinship disappeared may be rather in the ways in which authority transmission evolved. The grounding in erudition and the smattering of scientific hypothesis-testing that used to be required of budding anthropologists were gradually replaced with an assessment in terms of relevant connections. Now one consequence of this mode, as I suggested above, is that new contributions are judged valuable not just if they produce *more* inferences with the same processing work, but also if they produce the same richness of effects with *less* processing work. Given a certain type of material, a proposal that requires less technical training or fewer complicated diagrams should have a competitive edge.

I would submit that this is precisely what happened in the domain of kinship. Consider the models put forward to account for “semi-complex” matrimonial regimes that share some features of prescriptive elementary structures and many aspects of complex regimes. In terms of kinship terminology or preferential alliances, these require complicated network models, including such considerations as the dynamics of networks, their density, their topology, etc. (Houseman 1998). Now consider, by contrast, any article or book on kinship that attacks similar problems from a relevant-connection angle, e.g. “the cultural construction of the father in the context of global culture” – in other words, how do African peasants combine traditional norms with the soap-operas they watch on television? Both ways of approaching kinship have their merits. The difference is that the latter requires less technical spadework, as it were. There are no techniques to acquire, no previous models to learn, no complex diagrams to follow, etc.

Because cultural anthropology is now dominated by the relevant-connection mode, it tends to evolve either towards extracting more inferential potential for a similar amount of technical work (for instance, more striking inferences from fieldwork that is still substantially the same) or the same inferential potential for much less work (so for instance kinship systems are said to be “incommensurable” but that is not based on difficult technical analyses of terminology and alliance networks).

From a technical viewpoint, then, anthropology has become easier. This does not mean that it has become easier to become an anthropologist, quite the opposite. A field that evolves from erudition or science to relevant connections is also a field where there is much greater uncertainty about the outcome of one's contribution, the reception of one's work. Since relevance depends on the information activated by a listener in the interpretation, and since there can be great variation in the nature and salience of that information, one cannot predict the most relevant interpretation others will select.

Fields that differ in their mode of authority transmission also differ in their choice of themes. Cultural anthropology and archaeology have evolved in symmetrical ways, the latter becoming more and more technical as the former became more fuzzy in its foundations. Budding archaeologists have to master great amounts of chemistry, genetics, geology and various other technical skills to practice their craft. Budding cultural anthropologists are not required to master any technical skill of that kind.

The Anxiety of Reduction: Another Reason for the Demotion of Kinship

This brings me to the question of the relationships between various disciplines, particularly to the question of reduction. In general, a field that is organised around science has few boundary disputes and is in no special fear of "reductionism." For instance, most psychologists are perfectly aware that all the cognitive processes they study reduce to neural events. Neuroscientists know fully well that all neural events are chemical ones. Being aware of this reducibility of events, or even of some laws, does not really spoil the party for them.

The same is true for fields organised around erudition. Although they often seem "old-fashioned" and are indeed very old, fields like archaeology or classics or comparative philology have generally adopted, indeed enthusiastically embraced, the latest technical developments as well as the reduction of "their" events to instances of reducing laws. For instance, all historical linguists know that the organisation of children's brains, by imposing very strong constraints on what languages can be learnt, is a major force in the evolution of language. In other words, one of the crucial causal factors in what the field studies is a series of principles

studied by another field, and no-one seems to mind. Fields organised by science or erudition do not mind purchasing, as it were, empirical laws or technical solutions or formal models from other disciplines, if these external products can help them solve their own problems.

By contrast, fields organised around relevant connections tend to live in a permanent "anxiety of influence." The fact that some technique or principle has been established or is being studied in another field is taken as a self-evident reason for rejection. This is most virulent in cultural anthropology, where many practitioners are committed to keeping "their" facts uncontaminated, as it were. One of my colleagues who had studied (with great panache) transsexual prostitutes in Europe was baffled by my suggestion that she should read recent neuro-endocrinal research on the developmental process that results in "male" bodies a "female" brain. The very suggestion to consider some non-cultural source of causation in human behaviour was rejected *a priori*.

Relevant-connection fields, instead of establishing causal bridges and borrowing techniques from other fields, generally adopt a kind of "peaceful coexistence" policy, whereby all disciplines are allowed to prosper as long as they do not tread on each other's turf and generally leave each other alone.

This is another reason for the demotion of kinship. Hypotheses about alliance networks, family relations, inheritance, etc., are by necessity largely based on evidence, principles and facts from other domains. It is clear for instance that no study of incest could ignore the evolved psychology of the species, and the connection between incest-avoidance and optimal outbreeding, the focus of great work and findings and models in evolutionary biology. In the same way, alliance networks are informed by independent factors such as the kind of economic situation people live in, the political structures that allow some to maximize their utility, and so on. This is where the tools of microeconomics and rational-choice political theory are particularly relevant. Finally, the kinds of emotions or trust that can be associated with certain genealogical positions is obviously related to inclusive fitness models.

The Future of Kinship

In all likelihood, kinship will not return to its central place in cultural anthropology. This does not however entail that the topic is dead. My optimistic prediction is that we will have more and more of a scientific field of kinship studies, that will take the input from evolutionary biology, evolved psychology, economics and decision theory and make good use of all this to explain commonalities and differences studied by cultural anthropologists. This may or may not happen in the institutional field of "cultural anthropology." It may happen in programmes called "human evolution" or "ecology and behaviour," "cognition and culture" or "experimental economics." These fields, being more science-oriented, will find it easier to co-opt research programmes that actually explain a lot of variance in interesting human behaviour in terms of a small number of general constraints.

Now this requires that kinship specialists *do* provide rich explanatory models, causally related to evolutionary, economic and political variables that impinge upon social interaction. The only way to achieve greater scientific pertinence is to instil a larger dose of opportunism, revisionism and disciplinary infidelity in kinship studies. In other words, use the tools that work, shed the theories that do not, and pay no attention whatsoever to the fact that it is or is not "anthropology."

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