Alan Barnard
21 days ago

This is a very interesting piece. Nevertheless, it does highlight the fact that 'kinship' is simply not a clearly definable thing. Taking my former colleagues at Edinburgh as examples, we have Janet Carsten at one end of a spectrum and perhaps Anthony Good at the other. It is not that they disagree about what 'kinship' is; they have different understandings of what we as anthropologists are investigating. In this, I suppose I am on the Anthony Good side. But the crunch comes in the way we do these investigations. It really depends on what interests us. Are we interested in culture (Carsten) or structure (Good)? We cannot really be interested in both equally. It seems Read and El Guindi have my interests too, and this should be recognized as a fundamental divide. Maybe we are talking about two different kinships? Perhaps even a few more?

> **Dwight Read**
> 20 days ago
>
> There will always be differences among researchers with regard to the issues that are of personal interest. Normally this is healthy, for as Alan suggests, a single person cannot deal with all issues equally, yet all of these different issues need to be developed and so division of labor according to different interests of researchers can be healthy. Unhealthy is for one group to assume that the issues of interest to them supersede other issues. Within the study of kinship systems, there needs to be both research focusing on the phenomenal level of performance as a means by which kinship symbols are instantiated and research at the ideational level focusing on the formation of the systems of kinship symbols that are instantiated through performance. However, the former is not one kind of kinship and the latter another kind of kinship. Rather, kinship, as a totality, incorporates both. Knowing how a kinship terminology is conceptually generated from primary kin terms, for example, leaves unanswered the full range of how the conceptually generated kin terms can properly be instantiated. If I, as an English speaker, refer to him as uncle, and he refers to her as daughter, then I know to refer to her properly as cousin as part of my cultural knowledge regarding the generative logic underlying the English kinship terminology, but this does not inform me as to all of those who I can properly refer to as uncle and who I can properly refers to as cousin. In the Trobriand example, the instantiation of the Trobriand kin terms with genealogical relations is predictable from the generative logic of their terminology and accordingly, their kin term *tama* can be instantiated as genealogical father but, (as I understand the matter) when taking into account their culturally determined system of feeding, I will refer to the man who properly feeds me by the kin term *tama* to the exclusion of my genealogical father. From a performance perspective, it is performance that makes the man feeding me my *tama*, but when he dies, I will refer to my genealogical father as *tama* in accordance with the genealogical instantiation of the kin term *tama*. Neither the performance kinship account nor the structural kinship account is a complete account and both are needed. The problem with the handbook is that

its title implies that performance kinship – the topic of the handbook -- is kinship. Had the handbook been titled something like, The Handbook of Performative Kinship, then there would have been no need for this critique.

Mauro W . B . Almeida
11 days ago

Of course kinship is not a clearly definable object. A possible explanation for that was forwarded by Lévi-Strauss in his essay on what totemis is. In this essay the point was that so-called totemism was a bundle of separate phenomena: sociological institutions (kinship rules), cosmological systems (taxonomies of beings), and logical systems (isomorphism between social groupings and animal groupings). Similarly, "kinship" is clearly a bundle of distinct phenomena: (1) social institutions (descent rules, property transmission, office transmission), (2) cosmology (socio-cosmological dualism and variants of these), (3) logic (the grammar of kinship terms-concepts that results into kinship terms: for example, "a same-sex genitor'ś brother is (terminologically) a same-sex genitor./"

Franklin E Tjon Sie Fat
17 days ago

"Conceiving" kinship as gendered social relatedness?

The Mouse's Tale. In Ed Young's retelling of the fable of the blind men trying to identify an elephant (1), seven different coloured blind mice discover a "strange Something" by their pond. Each day one of the mice investigates part of the Something, and one by one they present a different theory to the group. It is only when the seventh mouse attempts a different approach, running up one side and down the other and then across the top, exploring the strange Something from end to end that she sees the whole, different truth. "Ah," said White Mouse. "Now, I see. The something is as sturdy as a pillar, supple as a snake, wide as a cliff, sharp as a spear, breezy as a fan, stringy as a rope, but altogether the Something is…an elephant!" The other mice try her method and concur. The Mouse Moral: "Knowing in part may make a fine tale, but wisdom comes from seeing the whole."

The elephants' reduction. In another modern transformation, an elephant joke inverts the fable, with the prescribed act of observation severely and fatally altering the subject of investigation (2):

"Six blind elephants were discussing what men were like. After arguing they decided to find one and determine what it was like by direct experience. The first blind elephant felt the man and declared, 'Men are flat.' After the other blind elephants felt the man, they agreed".

By and large, my understanding of what "kinship" is about is in agreement with the framework developed by Read, El Guindi and others: a strange Something, a category at once biological, societal, and cultural with an underlying transformational quality. I am also fascinated with the possibility of formulating the culturally grounded logic(s) modeling aspects of "kinship" as abstract algebras. Perhaps more to the point, I agree with the authors' fundamental critique of the Cambridge Handbook. While the themes and positions presented are interesting, the Handbook only provides an extremely narrow and reductive view, omitting most of the interesting new developments in kinship research (see the references in the critique).

Franklin Tjon Sie Fat ————————————-

(1) Young, Ed (1992) *Seven Blind Mice*. Philomel Books, New York.

(2) https://en.wikipedia.org/wiki/Blind_men_and_an_elephant

> **Sayres Rudy**
> 17 days ago
> lots of elephants and whatnot to just end up agreeing.
>
> **Sheila Newman**
> 17 days ago
> I think you sum the problem up well, Franklin. And the way you describe it applies to the over-specialisation of all disciplines.
>
> To Dwight and Fadwa: I find that you describe a problem accurately. This sounds like an impoverished handbook. Personally, I have found that putting things in traditional kinship/structural terms (as well as biological) has provided a logic with which to analyse the organisation of other species and our own, and to situate land-tenure and inheritance practices and laws, which underpin political systems. The biological intersects the political. I have found the notion of viscosity (sedentism) vs dispersal (atomisation) extremely useful in analysing, for instance, how the political systems of France and Britain differ. I am probably using mixed and confusing terminology here, since I was never formally trained in anthropology; it is one of many disciplines I find useful.
>
> With regard to only considering kinship as a socio-political construct, without biophysical roots and patterns in time, space and genes (recognising also the Westermarck effect) - this change in anthropology has become very obvious to me. It seems to me that it came about in the early 1970s, along with the oil shocks. I have some ideas as to the why of this, but suffice to say that it seems to have had the effect of largely fading out work that took non-industrial societies seriously, and explained how they worked and the signifi-

cance of their internal values. Once faded out, there is little to justify the preservation of those societies vs their integration into 'modern' values. These changes have also faded out the importance of families, clans and tribes within industrial societies, where in some societies there have been centuries of dispersal and consequent political disorganisation of the lower classes, in contrast to the viscosity of the upper classes, which facilitates their organisation. If my meaning is not clear, I would point to the extreme viscosity of dynasties (including old families without titles and private corporations) and the extreme atomisation of the bulk of the British and Irish, and the effect these different structures have had on distribution of land and power.

Maximilian Holland
20 days ago

There's a confusion around much of the topic of kinship, I suspect ultimately coming from entrenched anthropocentric supremicism, and especially the fact that humans have language, and thus (try to) find meanings in common everyday patterns of life.

It seems to me that it is often (deliberately) forgotten that ALL mammals have basic social relationships (minimally, with the mother/wet-nurse and nest-mates) in infancy in order to simply survive. Social mammals, especially primates, and humans, have more enduring and complex group/social relations throughout the lifespan, again, in order to survive. This is just basic stuff. These social ties are accompanied by (emotional/motivational) attachments, which are brought into being by processes of familiarity arising from group-living and shared nurturance (feeding, sleeping, warmth, etc.). There's overlap with these interactions, and what Schneider looked at as processes of kinship.

Note that - contra simplistic sociobiology - while both infant-care and extended nurture have often coincided with genetic ties, in mammals, primates and humans, there's no necessity for this in the healthy functioning of a new human life. That is, the attachments associated with the formation of functioning primary social relationships can snap to other figures than close genetic relatives, or even specific genders, and aren't per-deterministic. (Distinction in biology between proximal causes and distal causes).

I have called this "nurture kinship".

Scholars will continue to stumble around, as if blindfolded in a dark room, if they ignore these foundational aspects of obligatory (primate and) human social relationships - whether through ignorance, or ideology, or some kind of unexamined anthropocentric supremicism.

With sexual reproduction and varying degrees of exogamy (like all mammals), then of course some common constellations of social patterns will develop. Humans, with language, will also tend to employ words to refer to the most frequent aspects of these patterns, which will of course vary by a culture's economic/household/living arrangements.

Since household living (co-residence) patterns are economically and technologically influenced, and change over time, and spread, of course we will see some new forms of social constellations and the adoption-and-adaptation of (the meaning of) words over time.

I would remind older scholars that what curious minds consider to be their most pressing questions, change over time, and every domain of inquiry has a socio-historical context. It makes sense that, e.g. new family forms, are considered pressing topics by a new generation of kinship researchers.

Keith Hart
20 days ago

This critique has merit, but, given its scope, commenting on its detailed substance should be secondary to asking questions of form, logic and method. 1. Kinship is not part of demotic speech in any language. As a professional construct it can easily be diverted into scholasticism. 2. An argument that starts from negation often incorporates enough of the opponent's thinking to obscure its own positive conclusions. This is the way of dialectic. 3. Kinship as an object of study is a collective noun; but the active processes subsumed under it are better described using verb-phrases.

R.G. Collingwood's logic of question/answer would help. His methodological principle was to keep asking, "What question is this the answer to?" Rather than assume that anthropologists using the term kinship belong in and dispute a single category, we might identify some of the field's leading figures and ask what questions they tried to answer and how effectively. My mentors were Meyer Fortes and Jack Goody. I have come to admire William Rivers greatly. There seems little doubt that they contributed to the study of kinship. But I take from them what I need for my own purposes; and collective nouns are less helpful in that respect than asking what questions motivated their work and how their question/answer logic evolved over time.

Fortes' development cycle idea was temporarily taken up by Goody. I think it's a pity that it has been dropped. But their approaches to social anthropology as a discipline were wildly different. Meyer was a trade union organizer manqué, whereas Jack would tell us to "find a good question and follow it wherever it goes regardless of discipline". Both had a point. Rivers saw psychology and sociology/ethnology as separate sciences, until he discovered their underlying unity in himself. "You never step in the same river twice".

I share these authors' view that there is too much of value in the study of kinship to write it off. But our metaphysics need to be more flexible. I see no reason why kinship as a collective noun should persist independently of the example of its great practitioners unless another generation finds fertile grounds for renewal in its history and new ways of thinking and writing about them.

Sayres Rudy
20 days ago
This comment is brilliant. ps Collingwood reference?

Keith Hart
19 days ago
R.G. Collingwood (1889-1943) was born and died in the Lake District. His father worked closely with John Ruskin. He went to Rugby School and read classics ('greats') at Oxford. He became a philosopher, historian and archaeologist. His Principles of Art (1938) was influenced by Benedetto Croce. Collingwood's aesthetic theory held that the artist's role in his community is to help people focus on their emotions. His posthumous The Idea of History (1946) was an important contribution to the philosophy of history. He held that history was a science in the European sense of organized knowledge, here a science of human thought and action. He liked Vico a lot.

"Every day I walked past the Albert Memorial. Everything about it was visibly misshapen, corrupt, crawling, verminous. Why had Scott done it? What was the relationship between what he did and what he tried to do?...Truth consists of questions and answers. Each answer must be 'the right' one to the question it professes to answer". (Autobiography 1939: 29)

The Albert Memorial story is good for me to think with, but in ways unintended by its author. A reader triangulates text and author with what is already stored in their memory. Collingwood wanted to displace the 'realists' in his field. He was a logician and I am not. I find it pointless to ask whether the end result is the 'right' answer to what I had in mind when I started out. I prefer the analogy of climbing a mountain. We can see the top from the bottom and get a great view from the top. In between, all we see is one foot stepping in front of the other. Any large work of the mind is like this, whatever vision we once had and might have when we finish. Most of the actual work is tedious; any guiding perspective goes missing.

I do, however, have an answer for 'What relation was there between what I have done and what I tried to do?' I take from Collingwood the idea that it sometimes

pays to step back and ask ‘What question was this supposed to be the answer to?’ I have done that a dozen times while writing Self in the World: Connecting Life’s Extremes (Berghahn, March 2022). What we start with is a prospectus, a plausible fiction. How could anyone know how the finished article will turn out, even less why?

Sayres Rudy
19 days ago

I have returned to The Idea of History often but haven't read his autobiography. This claim seems manifestly implausible but as always with Collingwood, fruitful: "Each answer must be ‘the right’ one to the question it professes to answer." That sort of correspondence dates him but I like a great deal your own use of this general methodological point to insist on the centrality of posing and then responding to clearly articulated questions and the mountain-climbing analogy. Many thanks, Keith, for a nice exchange, and I look forward to reading Self in the World based on your insights here. Congrats on it, too.

Keith Hart
19 days ago

Thank you. The Autobiography is available on Kindle for almost nothing. If I may be permitted some auto of my own, Ruskin developed Coniston village and its lake with Collingwood Snr's help. Collingwood Jnr died at 53 of multiple strokes in Coniston in summer 1943 within a month of my birth. We spent our summer holidays there when I was very young. I began to walk in the garden of Mrs Battie’s Coniston guest house on my first birthday and have a photograph to prove it. I don’t believe in the transmigration of souls; but magical thinking is a reliable writing tool for me.

Sayres Rudy
18 days ago

That is lovely & more than welcome. Thank you for sharing it.

Charles C H B Batjoens
11 days ago

Thank you very for your nice sharings. I think more specifically at the social reproduction by the different terminologies for the ranks, grades and status used in the australian's societies studied by A. Radcliffe-Brown !

Mauro W. B. Almeida
15 days ago

There is much more behind "kinship" phenomena besides semantic-ontological assumptions relating to gender, generation and identity. For kinship terminologies are inevitably related to a cal-

culus -- the operation which, given two terms, produces a third term (maybe one of the the previous terms). Thus: "A father's brother" is an "uncle", or, as in the Cashinahua language, the *ichun* of an *epa* is an *epa*.

Mauro W. B. Almeida
15 days ago

I side with Read and others on the one-sided approach of this entry. It is a dogmatic position which ignores the discussion on the main issue -- what is kinship. One preliminary step toward acknowledging the existence of a discussion is acknowledging the existence of multiple dimensions of kinship: as a cultural construction (the semantic dimension), as a semiotic construction (the syntactic dimension) and as a pragmatic fact (the sociopolitical dimension). At stake is whether these "dimensions" really constitute aspects of a single phenomenon -- for, alternatively, one could consider them as different objects of investigation. The problem here is the appropriation of "kinship" as the proprietary domain of the "pragmatic" approach -- where kinship is disconnected from its syntactical-semiotic dimension.

These alternative ways to approach the "kinship" phenomenon are linked to pragmatic approaches to kinship as facts: as bundles of meanings associated with word (the semantical approach) as rules for combining words in order to produce words (the algebraic-syntactical approach); the pragmatical approach (how words are used in actual interactions).

Jean-François guermonprez
11 days ago

I see in the reduction of "kinship" to "relatedness" an ideology which, like all ideologies, is based on beliefs that are resistant to rational arguments. With hindsight, time seems to be the most efficient antidote for neutralizing ideologies. So, let us be patient. The so-called "turn of relatedness" is only four decades old! In the meanwhile, the process of the erosion of "relatedness" may be, hopefully, accelerated by dedicated anthropologists like Dwight and Fadwa. Many thanks for their contributions and their spirit of resistance embodied in the Kinship Circle.

As a matter of fact, I find nothing new in the "new kinship studies". As far as I can see, it is a repetition of Heraclitus' well-known view on flowing rivers. Panta rhei was his mott: everything flows, nothing is given. More recently in the European history, German Romanticism was a radical reaction to the French Enlightenment: feelings and emotions against Reason. Likewise, Carsten tells us that she was dissatisfied with the dry and abstract studies of kinship. We, anthropologists, are advised to stay as close as possible to the daily life of the peoples we live with for a while. Note that anthropologists do not have informants anymore, only friends. This is ridiculous and paternalistic and sometimes so "goody-goody". There are men and women we like in the field and others we don't, and this is reciprocal. This is human and we are all humans, aren't we?

As a French person of a certain age (alas), I vividly remember the wonderful days when everything was "new" in Paris: cinema, philosophy, history, literature and, of course, the new cuisine (the only new thing I take very seriously). This was also the days of the emergence of a "new" generation. Paradigmatic of this "generalized new turn" was "la nouvelle vague". Truffaut and his buddies decided in the 1960's that all French movies made before their coup de force were good for the dustbin. This is laughable, except that it worked perfectly for several decades. As always in this kind of petty revolution, a guru was proposed to the admiration of the followers of the new movement; preferably an outsider, a kind of "stranger-king" if you like (see Foucault etc.). So, Alfred Hitchcock with his characteristic round profile was consecrated the godfather of "la nouvelle vague". Of course, Truffaut in praising Hitchcock was praising himself for having discovered the latter's genius.

Likewise, Janet Carsten, a British heiress to Malinowski famous for his "seminal" comments on "the bastard algebra of kinship", discovered the American David Schneider and made him her hero, while efficiently building up her career and reputation in the very competitive academic world of Thatcherian Britain (Bourdieu smiles). To be sure, Schneider was a phenomenon. Remarkably, he stimulated several respectful, careful and cogent critiques of his anti-kinship position. Among them, Robert McKinley's contribution is, I think, excellent; in addition, he was (is) a competent Malayist so that Carsten necessarily read his brilliant paper on "the philosophy of kinship". To no avail. She and her followers uncritically accepted Schneider's extravagant idea that kinship was a "non-subject". This kind of absurdity should not be acceptable in any serious scholarly discipline. But, on the contrary, it was received with much enthusiasm, at least by those, students and young academics, who, I imagine, hated algebra and abstractions and discovered with delight that the idea of relatedness was giving them an instant access to the elitist field of kinship studies. This seems to confirm that anthropology is a mixed bag of scientific, para-scientific and irrational approaches in variable proportions. Indeed, anthropology rarely rhymes with epistemology. Imagine Newton taking stock in his maturity, and concluding urbi et orbi that apples are not falling down for the simple reason that they do not even exist. However valid Schneider's strictures were against the Euro-American ethnocentric interpretations of kinship, his final posture is a particularly arrogant form of hubris that is reflecting his controversial personality and would have been, I suppose, severely punished by the Greek gods.

In my opinion, the strategy of the pro-relatedness "modernists" has much in common with the cancel culture: do not discuss contradicting facts nor opposite theories, just ignore them. This is certainly a striking inflection in a long tradition of scholarship, where contradictory debates were considered necessary and heuristically useful. However, the rules have changed within the academic world and outside of it. If I remember correctly, Richard Rorty once wrote that what we really need is to have better conversations. He was thinking of democracy ...

Dwight Read has demonstrated with style and determination the existence of generative logics in the classifications of kin. His rigorous use of mathematics proves that these logics are not an invention of the analyst. On the side of the afficionados of relatedness, the answer is a blatant silence. Strikingly, the recent "Cambridge Handbook of Kinship" implies that kinship terminologies do not exist. Hence the screams of Dwight and Fadwa and those, I imagine, even louder of Lewis Henry Morgan. Denial is of course the usual strategy of political ideologies. This sectarian view suppresses many interesting questions and further possible debates about "kinship". Suppose we admit that terminologies do evince the cognitive bedrock of kinship, "l'esprit humain" in action as Lévi-Strauss would put it, then what? Is there anything like "kinship" beyond that? For instance, is kinship a dimension of an encompassing cosmology or ontology?

As a matter of fact, I believe this is (or was) so for the cognatic systems of Western Austronesia (Philippines, Borneo, Sulawesi, most of Sumatra, the Malay Peninsula, Java, Bali, Lombok). Everywhere in these societies kinship is a relevant subject, even in the island of Langkawi where Carsten did her fieldwork in the early 1980's. A close reading of her ethnography shows that Malay kinship does exist in the sense of Dwight Read. It also shows how selective and partial is Carsten's way of writing ethnography. Seeing her Malay village through the kitchen "dominated" by women is not uninteresting, but this is a deeply skewed view from which men are absent and, even more surprising, the crucial dimension of Islam is completely ignored. In Carsten's ethnographical writings, the kitchen hides the mosque. In fact, the Malays of the Peninsula have been deeply Islamized since the foundation of the Malacca sultanate some 600 years ago. This is a Malay-Muslim culture within the Austronesian world. Comparison with other less Islamized Malay ethnic groups, in South Sumatra for instance, may shed much light on some issues discussed by Carsten; like, for instance, the representations of "breast milk" among the Malays who have integrated an Islamic conception into their own beliefs. However, "comparison" is not part of Carsten's discourse of "relatedness". More generally, the comparative dimensions of "Islam" and "Austronesian-ness" which are constitutive of Malay history and anthropology are completely ignored. We are left with stories told by Malay Muslim women to a female non-Muslim young anthropologist who, finally, confesses that she was uncomfortable with religious issues and was under pressure to convert to Islam. Carsten is, however, a brilliant rhetorician and gives herself a quick absolution for, as she writes without irony, completeness is not an appropriate aim for an anthropoligist (The Heat of the Hearth, p.29). Really?

Unfortunately, Carsten's narrative combines several fatal flaws. It is narrowly gynocentric, probably egocentric and certainly ethnocentric. For viewing Malay kinship through the lens of an anti-Euro-American perspective is to be still dependent on this model (I salute Keith Hart's comment). First, as a result of neglecting the temporality of the successive generations in the Malay system, she reifies "siblingship". In doing so, she implicitly construes this synchronic re-

lation as "given" in contradiction with her constructivist axiom. But siblings are first of all co-children in this system and, likewise, first cousins are co-grandchildren etc. Her spontaneous view of "cousins" as an extension of "siblings" is wrong and reveals her shallow knowledge of the field of kinship. In short, verticality here subordinates laterality. In Read's terms, the sibling-sibling relation is not primary, while the parent-child relation is. Second, her Schneiderian prejudice prevents her from recognizing kinship systems which may be very different from the Euro-American ones but may, nevertheless, share the same conception of consanguinity as a given category (see Viveiros de Castro). As far as I understand, this is the case of the cognatic systems of Western Austronesia, including Langkawi in Malay Malaysia. Third, there are reasons to suspect that Carsten's constructivist axiom is ultimately reflecting the current particular evolution of kinship in the West. Finally, I find striking the absence of reflection in Carsten's writings on her own approach. Her use of ethnographical data and bibliographical references is, it seems to me, opportunistic. They all serve preconceived ideas. Particularly striking in this perspective is Carsten's idiosyncratic appropriation of the Levi-Straussian House which she completely distorts to suit her own need. So, her Malay "community" becomes by the magic of her metonymic rhetoric a "phantasmagoric" House. That is a truely fantastic interpretation. This is also ethnocentric in another sense: Carsten like her master Schneider and some other anthropologists of the Anglosphere have misread, misunderstood, misinterpreted what is crystal clear in Lévi-Strauss writings: the revolutionary change of focus from consanguinity to affinity in his alliance theory. No, blood is not thicker than water in "Les structures élémentaires" (1949). A recently published copy (thanks to El Guindi, "Suckling", 2020) of a letter of Schneider to L-S in 1971 illustrates his misunderstanding (lack of interest?) for the alliance theory, not mentioning his naivety if he thought he could intimidate the French master. Despite its relevance, Schneider's critique of kinship remains americanocentric, while Lévi-Strauss magnum opus on kinship is best understood when it is placed in its deep historical context (Mauss, Durkheim, ideas on socialism etc. and ultimately Rousseau). Fortunately, Boon's superb "Other Tribes, Other Scribes" reminds us that cosmopolitan anthropologists do exist, even in tangentially americanocentric America.

All in all, the Malay-Muslim case is interesting inasmuch as there is little left of an inherited Austronesian cosmology centered on "ancestry", except a kinship terminology which is a variant of a Western Austronesian general type. But, as I suggested above, Carsten has completely missed "Malay kinship" in Read's sense. She has ignored the strict systemic generational order which is the backbone of these Austronesian cognatic systems. True, Western Austronesians do not think genealogically. However, they think generationally. This is what the Geertzes, then under the spell of Africanist influences, could not understand as evinced in their "Kinship in Bali." They wondered about the "Balinese genealogical amnesia" and pointed to teknonymy as the culprit. Wrong question, wrong answer. Teknonymy is distributing Balinese villagers in separate generational layers according to the dominant principle of order. Even more significant, this generational verticality in Bali, in Borneo, in South Celebes etc. is consonant with the prohibition of

intergenerational sexual intercourse and the preference for marrying "a cousin" (any type of cousin), that is a close collateral within Ego's generational level. This cognatic kin type - not Eskimo not Hawaiian not American but, for instance, "Iban" (why not?) - has further general implications which are beyond the scope of these comments ...

What is kinship? What is reality? Those are tough questions I will leave to competent thinkers provided they are wise enough to eschew the harmful scholastic disputes of the British masters of "social anthropology". Making mine the American tradition of pragmatism, the question that the ideology of relatedness inspires me is : what is its heuristic potential in practice? After all, bad models can produce good ethnographies (see Bornean studies) which, as we should never forget, are the very blood of anthropology. Hence my scrutinizing Carsten's ethnographical writings on her Malay village, Kuala Teriang, As I strongly suggested above, I can only conclude that I find difficult to "objectively" understand why Carsten became the main influencer of the so-called Schneiderian kinship studies. I tend to share Godelier's opinion on "relatedness" in his "Metamorphoses" : thin results but much flowing rhetoric to refer again to Heraclitus' philosophy of fluids. But, as I wrote in the beginning of my (too) long comment (sorry), ideologies rest on beliefs, not on a rigorous analysis of ethnographical data. Thanks to Fadwa and Dwight for their stimulating critical review of the Cambridge Handbook of Kinship.

Charles C H B Batjoens
11 days ago

Thank you very much for your nice sharings. I think more specifically at the social reproduction used by the different terminologies, as for the ranks, grades and status in the australian's societies studied by A.Radcliff Brown !

Mauro W . B . Almeida
15 days ago

The different answers to "what is an elephant" is amount to the empiricist approach to science: what you detect is what there is. On the other hand, there is the approach that says: what you detect by using one sense is one thing very different from what you detect using another sense: color for Goethe is not the same as color for Newton at all. Lévi-Strauss split the concept of "totemism" in several distinct objects: as a category of kinship structure, as a category of ontological classification, as a category of incest prescriptions. The same splitting should apply towards the supposed "kinship" object. It relates to ontology (taxonomy), to language (logic) and to pragmatics (social use). All these "kinship" realities should have been considered: semantic, syntactical, sociological.

> **Charles C H B Batjoens**
> 11 days ago

I agree with your analyses ...

Sayres Rudy
10 days ago

Check out Almeida's "Is There Mathematics in the Forest"

Robert Parkin
2 days ago

ROBERT PARKIN, COMMENT ON READ/EL GUINDI

I have not read Bamford's volume, and given existing commitments I'm unlikely to do so. However, in the context of this comment and Read and El Guindi's original review of Bamford, I think it would be opportune to remind ourselves briefly of how the relationship between gender and kinship in anthropology – and to an extent, therefore, between structure and culture – has evolved. This is because the arrival of gender in the discipline was historically associated with the elevation of culture as an explanation for kinship (in the supposed but increasingly maligned Schneiderian revolution), and that was no coincidence. In this respect, of course, gender arrived late at the table, much later than kinship, largely because most early ethnographers were men, and accordingly they had better access to male than female informants. Only later, as part of a struggle to establish gender more decisively as an anthropological topic, did authors such as Yanagisako and Collier (1987) and Howell and Melhuus (1993) start advocating a fusion of gender and kinship, both having been influenced by the cultural turn associated with David Schneider. There were also occasional voices advocating a rapprochement between culture and structure, including Janet Carsten, who also admits to problems with the culturally focused notion of 'relatedness' with which she is most identified (2000: 4-5; a response to Holy 1996: 169-72), and, in a more extended and perhaps rather more forthright manner, myself (Parkin 2009). Of course, to say 'gender' tends to suggest 'women' in practice, despite more recent work on so-called 'masculinities' (e.g. Vale de Almeida 1997). That must be avoided, as we are no longer in the early period when the previous failure to include women sufficiently in the ethnographic mix needs to be 'compensated' for by giving the pendulum an extra push in their direction.

However, I have no objection to such a fusion in itself, as it is clear that kinship is affected by gender, as well as enough evidence by now that women often view their kinship systems differently from men (a good example is Josephides 1995, on the Kewa of Papua New Guinea). For that reason, I am not in principle opposed to culture as a perspective, nor do I have a reverse preference for seeing structure as the answer to everything. That said, I would certainly agree that there is a place for kin terms as a form of classification as well as for kinship as practical action, since without the former to set the conditions for the latter, the latter would only descend into chaos. I would agree with Read and el Guindi, though, that leaving it at that would mean omitting many important topics from the study of kinship, and I suspect in gender as well. One can readily think of examples in the models of kinship systems and constructions of actual kin-

ship systems and terminologies in themselves, which many of us have sought to contribute to, including Read and El Guindi.

Regarding the 'cultural turn' in kinship, however, in one respect my scepticism has steadily increased over the years over one issue in particular: the alleged absence of biology/genealogy from our informants' thinking about kinship. Ironically it is something that both David Schneider and Rodney Needham tried to dismiss, despite their opposition in other matters, the former less consistently than the latter if Read and El Guindi's demonstration in their comment is anything to go by. For the Needhamites it was more a matter of their emphasis on category at the expense of genealogy in their studies of kin terms, not Schneiderian culturalism. Read and El Guindi's reference to Ellen Lewin's study of how gay couples seek out biological kin in preference to biological non-kin in seeking support in caring for their children resonates with a previous paper by Christoph Brumann (2000). Working on ideologically driven collectives, from hippy communes to the kibbutz, the latter found that those ventures of this sort that allowed some space to the 'natural' nuclear family alongside an ideological commitment to share everything, including parenthood, tended to survive better (the evolution of the kibbutz in this regard is salutary). Another example is Rita Astuti's work among the Vezo of Madagascar, who ostensibly see their kinship in terms of idealized bilateral circles uniting as many alters as possible without there necessarily being a biological connection with them, which in their explicit statements they ignore as irrelevant. However, on Astuti pressing them, they were eventually prepared to acknowledge the existence of biological ties between some egos and alters (Astuti 2009). Contrast this with the perspective of Bamford herself in an earlier edited volume: in her work among the Kamea of Papua New Guinea, she goes to great lengths to deny that links between fathers and sons have anything to do with genealogical thinking, despite saying that, in intercourse, the father contributes 'bone and internal organs', the mother 'skin and surface blood vessels' to the fetus (2009: 162; cf. Parkin 2013).

Of course, anthropologists are used to the fact that folk models of biology are generally not fully in line with the perspectives of the geneticist or human biologist, and might be at complete variance with them. This is nothing new, as it affected now ancient debates in anthropology about the failure to recognize certain kin ties among, e.g., the Todas or Trobrianders. Such recognition might well be lacking in some societies, but equally it is firmly present in others, which, I would argue, are probably in the majority worldwide. Sometimes, too, it might depend on context: thus the famous Trobriand denial of paternity might have more to do with protecting matrilineal rights than with everyday relations of kinship, as Edmund Leach recognized (1967).

Two final points: it was a structuralist, Rodney Needham, as much as the culturalist Schneider, who spread the idea that there is no such thing as kinship (cf. Needham 1971: 5). Secondly, there is an irony in the fact that the Schneiderian dismissal of kinship as biological was balanced by an

emphasis, innovative in itself, on the body as a key anthropological topic. In fact, of course, the contrast is in effect one between structure and culture.

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Allison Jablonko

2 days ago

Reading Dwight Read's and Fadwa El Guindi's Critique on the first day after it appeared here, my interest in kinship was reawakened. I had not given the subject much thought after using kinship charts in the 1960s for the very utilitarian purpose of "sketching out" the lineages and sub-lineages of the Maring speaking sub-clan where I was doing fieldwork. Read's and Guindi's considerations pointed to layers of theoretical thinking I had been unaware of. It struck me that their critique could well be published as a sort of introduction to the Cambridge Handbook - just as it has appeared here.

The next day, comments began appearing, each adding to a multi-dimensional and gratifyingly collegial discussion.

Now, on this last day before the discussion closes, I want to thank Fadwa and Dwight for starting the process and all the commenters for filling it out. What I see here is the articulation of basic material - theoretical and ethnographic - that has been developed over many remote and recent decades witnessing to the richness of anthropological discussion. Scholars interested in how intellectual history unfolds will be rewarded by close attention to this particular instance. Students may get a taste of the universe of discourse they are entering. I look forward to reading both the Critique and the Comments again as a mental pick-me-up.

Michael Fischer
8 hrs ago

Read's and El Guindi's contribution has stirred up a good discussion. The range of response reminds us, as G. P. Murdock argued in his 1971 Huxley Lecture, Anthropology's Mythology, that neither culture nor social structure can be used as a 'cause' for anything as either is simply a label anthropologists apply to the outcomes of human thought and effort and its organisation. The same stricture applies to Kinship itself; this can never be more than a collection of observations and considerations of phenomena that anthropologists have observed.

But because an after-the-facts emergent category cannot be used causally does not mean that the constituents cannot. Some are and some aren't. It might be tempting to argue that because kinship is rooted in relationships, then relationships must be the seminal principle. And ultimately, this may be the case. But a seminal principle cannot account for the rich details, textures, and possibilities inherent in more specific instantiations arising. We can attest to the processual continuity, and indeed seek the underlying causes, of any of the instantiations that collectively account for the observational category 'relationship' or 'relatedness'. We cannot account for the set of instantiations from some seminal relationship alone.

Read and El Guindi demonstrate that kinship represents activities and organisation that are unique relative to other relationship phenomena. Perhaps the most significant argument that they

minimised in their discussion are some implications of the algebras that Read has identified as probably ordering all human kinship terminologies. One, which made a big impression on me when I originally criticised some of his work over 40 years ago, was in his simple rejoinder ... the fact that the relationship between terms can be expressed as an algebra at all is remarkable, as there are many many less restrictive forms these relationships could take; why would humans, apparently universally, adopt this way of structuring these relationships. A little over 20 years ago I had the privilege to work with Dwight for a few years to modernise his software intended to be an expert system to help other anthropologists to identify these algebraic structures in terminologies of interest. The project went astray when we discovered that an expert system was not required; the pathway to identifying and specifying a specific algebra for a specific terminology was more or less algorithmic.

Further we found that it was not just a descriptive abstraction, but a predictive abstraction; from knowledge of just a few terms and their relationship to each other based entirely on human symbolic judgements, applying no genealogical knowledge whatsoever, that the number of and positions of the remainder of the terminology would be 'predicted' by the resultant algebra. To my knowledge, this is the only example of a complete and working predictive model of human judgements, in Anthropology, or any area of human sciences, based entirely on a closed system of symbols and human judgements of a subset of the relationships to each other. While there are fewer algebras than there are terminologies, there appear to be enough to accommodate the range of human approaches to reckoning kinship relations.

The point of this longer-than-I-intended comment is that over the years of Anthropology's formation and development exploiting the region we call kinship has often led the discipline in new directions for the understanding of humans and their creations. Directions that often lead elsewhere. Whether or not kinship is also a part of the region we call relationships, it is clear that the study of relatedness alone would not be sufficient, and should not be given as the only solution.

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