



## Fifteen Complaints against the New Kinship Studies

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**Abstract.** – Fifteen complaints are lodged against the so-called “new kinship studies” inspired by David Schneider. The main argument of these studies, that they get at indigenous appreciations, as contrasted with pre-Schneiderian analyses, supposedly entrapped in a Eurocentric model, is shown to be without merit. On the contrary, these latter analyses, far from *assuming* a procreative base for kinship worldwide, regularly *discovered* it in the field. Schneiderian kinship studies are shown to be grossly deficient from a scholarly standpoint, and to aspire to hegemony in the academy. [*kinship, history of anthropology, the culture of academia, scholarly responsibility, “radical” feminism*]

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[A]ll systems of social relationships recognized by anthropologists cross-culturally as kin relationships are rooted in parturition (Goodenough 2001: 217).

[I]n many primitive tribes the terms used for the immediate members of the family are either distinguished from the same terms in the extended sense by the addition of some particle, or terms corresponding to “own” are used ... Family is family, whatever the system of relationship ... (Goldenweiser 1937: 301).

Although the studies which suggest that kinship in many cultures is defined not only by genealogy but also by a code of conduct, particularly conduct expressing sharing of food, land and services, may seem to challenge the anthropological con-

ceptualisation of kinship as a system of ties established through procreation, they are, in fact, parasitic upon it (Holy 1996: 167).

[T]he idea of Western bio-essentialized folk concepts of kinship being endlessly ethnocentrically project onto non-Western cultures by ethnographers and kinship theorists is itself a kind of anthropological myth (Wilson 2016: 573).

Creativity is predicated on a system of rules and forms, in part determined by intrinsic human capacities. Without such constraints, we have arbitrary and random behavior, not creative acts. ...

[I]t would be an error to think of human freedom solely in terms of absence and constraint (Chomsky 1975: 133).

The self-styled “new kinship studies” are expressly indebted to David Schneider’s writings – particularly his study of American kinship (1968), his Morgan Centennial essay (1972), and most of all his “Critique” (1984) of previous kinship scholarship. Although particular analyses do not all share the deficiencies enumerated below, this common indebtedness, it seems to be, is sufficient to regard these studies as constituting a “school of thought.” This appellation is quite deliberate: I shall argue that this “scholastic” character has a stark Medieval quality, specifically, that it ignores (or is ignorant of) an enormous quantity of pertinent evidence; that it also ignores elementary logical and semantic operations; that it denigrates its scholarly opponents as “Eurocentric,” at the same time claiming, utterly erroneously; that it presents “the natives’ point of view” (Geertz 1983: 55); that it substitutes for empirical and logically sound analysis a Manichean model

of humanity which is remarkably compatible with Biblical eschatology, including a messianic view of Schneider and a hopelessly obsolete biology/culture dichotomy; that it substitutes for the alleged authority of a scientific establishment a very real authoritarian plan of its own; that it violates principles of scholarly debate, either by ignoring its opponents or by responding *ad hominem*.

For present purposes I shall mostly ignore Schneider's scholarship itself: I intend to deal with this at length in a future project.<sup>1</sup> I shall focus instead on his by now considerable legion of admirers, who present themselves as advocates of a *performative* view of human kinship, where kin (or kinship-like) ties are formed by non-procreative criteria, such as commensality (Carsten 1995), name-sharing (Nuttall 1994), and consociation (Weston 1991). Although I have dealt with particular flaws of the performativist view elsewhere,<sup>2</sup> the present analysis is intended to be more comprehensive. I present it as a series of complaints, to wit:

(1) "There is minimal and/or sophomoric attention given to kinship terminologies." Despite the seemingly endless celebrations of a "return" to kinship studies inspired by Schneider, what has decidedly *not* returned is the study of kinship *terminologies*, the foundational subject of the entire discipline in the United States (Shapiro 2012: 394), thanks to Lewis Henry Morgan's herculean labors (Morgan 1871), and an important part of anthropology abroad.<sup>3</sup> This is much less true of scholars influenced by Schneider early on.<sup>4</sup> But subsequent studies of performed kinship give the analyses of systems of kin classification short shrift. Weston (1991: xiv) dismisses such studies as "arcane." Similarly, Carsten (2004: 16) voices what I take to be a generally held view among advocates of a performative approach when she writes that "studies of kin classification became a highly technical and specialized area, quite divorced from the ... everyday experience of kinship."

In other words, kinship terminologies are not what one might call, following Geertz (1983: 57f.), "experience near." But they come much closer if one considers certain details readily elicited from our

informants. Consider Carsten's own analysis of her Malay materials in the light of those of other scholars. She claims that the Malay village she studied has "undivided kinship" (1995: 115), but she provides little data on kin classification. She *does* tell us, however, that "villagers perceive a continuum of relatedness, from the 'close' ... to the 'distant' ..." (1990: 272). Other sources concur with this latter finding, indicating, in fact, that Malay kinship is decidedly "divided." Superficially, the kinship terminology conforms to the generational pattern of Murdock (1949) and others. But Banks makes the key point that "Malayan [kin] terms tend to be appended by ... marker-affixes indicating that the individual in question is 'like a ...' <sup>5</sup> but not a definitive representative of the Malayan category" (1974: 47; emphasis added). Thus, for example, only one's genitor is referred to by the lexically unmarked "father" term; other "fathers" are lexically marked according to their degree of collaterality. Thus, one's FB is, in Malay parlance, "father one degree removed," father's male first cousin, "father two degrees removed," etc. Malays, Banks (1974: 51) concludes, "thus distinguish degrees of cousinship in their own generation much as American English informants do," except that "the concept of collateral distance ... is applied ... in all generations and not simply in [one's] own." Moreover, actual parents and children are said to be the "real" members of their respective kin classes (Banks 1983: 59). This is a recurrent finding in systems of kin classification, as Goldenweiser (see my second epigraph) pointed out many years ago, and, as such, it largely demolishes the claims of the performativists.

To continue: Full siblings are similarly said to be the "real" members of their kin classes but half-siblings are lexically marked – i.e., even at this close a genealogical position secondary semantic status is in effect. Wilder (1982: 86) notes Malay expressions which he translates as "mother's side" and "father's side" and which are employed in kin classification. Banks (1974: 51; 1983: 57, 60) and McKinley (1983: 345) provide examples of kin reckoning, all of which involve parent/child and sibling/sibling links, both genealogical and terminological. Read (2018) has called attention to the cross-cultural occurrence of the latter, exemplified in English by the rule, "I call the wife of anyone I call 'uncle' 'aunt'." Note that such rules manifest *native* principles of kin class extension.<sup>6</sup>

1 I shall mention Marshall Sahlins' recent contribution to a general theory of kinship (Sahlins 2013) only occasionally. For a more comprehensive critique of his book, see Shapiro (2018).

2 See Shapiro (2008, 2009a, 2009b, 2010, 2011b, 2012, 2013, 2014, 2015a, 2015b, n. d.).

3 E.g. Kohler (1975 [1897]), Lubbock (1872), Starcke (2012 [1889]).

4 E.g. Basso (1973), Inden and Nicholas (1977), Silverman (1971).

5 Throughout this essay, I use single quotes to indicate *glosses*, i.e., exact or approximate translations of foreign terms.

6 Such rules are unlike those associated especially with Harold Scheffler, which rely partly on genealogical positions that

Banks (1983: 80, note 2) reports an expression used “to refer to a group of all one’s blood kinsmen.” Wilder (1982: 97) notes another, which he translates as “near kin” and which, he notes elsewhere (1991: 132), distinguishes such people from “kinsmen a little distant” and “distant kinsmen.” Those outside this sphere are likely to be labeled by a term which Djamour (1959: 24), Massard (1991: 141), and McKinley (1983: 336) translate as “unrelated” or “stranger.” Massard (1991: 141) notes an expression pertaining to a whole village: she translates it as “the people here are all relatives.” But in the same paragraph she provides an expression by which neighbors who are not kin are rendered as “*like* brothers and sisters” (emphasis added).

Carsten makes much of the hearth used in common by the women of a household, but this Communitarianism is decidedly limited. Thus, in a polygynous marriage, according to Djamour (1959: 30), “[t]he children of the newer wife refer to the father’s older wife as ... [‘]stepmother[’], whereas those of the older wife refer to their father’s newer wife as ... [‘]young mother[’]. The wives themselves refer to each other’s children by their common husband as ... [‘]stepchildren[’] ...” There is a great deal more in Carsten’s corpus that contradicts her claim of “undivided kinship” among Malays: I shall have a bit more to say on this later. The interested reader should see Shapiro (2011b) for a fuller analysis.

Similarly, writing on the Reite of the Rai Coast of Papua New Guinea, and in a volume whose purpose is to “deconstruct” the idea of genealogy, James Leach (2009: 188) insists that “[t]o draw a line from father to son (in a kinship diagram) and say that this is a kinship connection is meaningless in Reite, for the role of the father is not to pass on some component of substance to the son ... but – through his work – to establish the conditions for the latter’s growth on the land.” But in his book-length statement on these matters, Leach tells us that such co-operative labor is “*modelled on procreation*” (Leach 2003: 29, emphasis added). Moreover, he renders the kinship terminology via conventional genealogical diagrams (2003: 57 ff.), though he presents no

information on lexical marking or other forms of subclassification, such as we have seen in the Malay case. But he calls attention to the teknonymous practice whereby “[s]iblings start to call their married female siblings ‘mother of,’ rather than ‘sibling’ ... *after they have borne their first child*” (2003: 66, emphasis added). It is hard to see this as anything but a practice based on procreative kinship. Moreover, this particular use of the “mother” term suggests that its primary meaning is based on actual maternity, rather than its “classificatory” counterpart – something which is true of all other teknonymous usages with which I am familiar.<sup>7</sup>

A third example is a recent analysis of Lakota Sioux kinship, wherein the author initially asserts, that “Sioux people continue to make relatives throughout their lives who are classed with, and *not differentiated from*, those they had at birth” (DeMallie 1994: 133; emphasis added). Then, two pages later, he mentions “linguistic forms that *differentiated between one’s biological parents and other mothers and fathers*” (135; emphasis added). But he provides nothing resembling a detailed semantic analysis of Lakota kinship terminology. Earlier scholars, however, were more thorough, presenting data that confirmed the latter statement but not the former. Thus, Walker (1914: 104) noted that one’s actual parents and siblings are distinguished from others in their respective kin classes by a modifier which he renders as “own,” which is logically comparable to Malay “real.” By contrast, these others are said to be members of their respective kin classes in a secondary sense: they are “considered as” mothers, fathers, etc. (Walker 1914: 96 f.). Close procreative kin are said to be “of blood,” but others are only “considered of blood” (97). A study of the neighboring Teton Lakota by Hassrick (1944) reveals *native* rules of kin class extension, e.g., let any woman married to a man I call “father” be called “mother.” Such rules, as we shall see, are common ethnographically.

(2) “Comparable pertinent data on systems of kin classification, which, if considered, would have obviated the performativist position, have been in the ethnographic record literally since Morgan’s day. But pace *supra*, performativist scholars seem to be unaware of it.” Thus Morgan noted that, among the Cree of the eastern Canadian Subarctic, the term for FB is *not* the “father” term in its unmarked form but rather that term with a lexical marker. Morgan translated this marker as “step-” and, consequently,

are likely foreign to native understandings. Schneider (1989) dismissed them as “virtuoso manipulations.” But there is more to the extensionist position than this: see esp. Scheffler (1972) for a response to such criticism. Throughout this essay my employment of such terms as “focal” and “focality” follows *native* distinctions – as indeed Scheffler did. Lounsbury, by contrast, simply *assumed* the logical priority of close procreative kin: thus his well-known analysis of Trobriand kin classification (Lounsbury 1965) takes no account of native distinctions noted by Malinowski and others (Shapiro n. d.).

7 E.g., Firth (1936: 130), Geertz and Geertz (1975: 85–94), Kroeber (1917: 70).

rendered the pertinent native term as “stepfather” (Morgan 1871: 208). In other words, in Cree the father is what semanticists call the *focal* or semantically central member of a larger “father” class, while the FB is a secondary member of that class. A less technical but perhaps more comprehensible way of putting it is to say that the FB is *fatherish*, or *like* a father, while the father is the Real McCoy. But Morgan was entirely unaware that such evidence, of which the Cree provide only one of his examples in “Systems,” utterly undermines his communitarian scheme of the progression of family forms more fully spelled out in “Ancient Society” (Morgan 1877). For these examples suggest a singling out of one’s father, mother, siblings, and children – in other words, one’s nuclear family – from other members of their respective kin classes. As we saw in the Malay case, English does the same – only more obviously, i.e., without resorting to lexical marking.

Speck (1918), basing his analysis on longer fieldwork and covering both the Cree and neighboring populations, confirmed the “stepfather” finding and added that the reciprocal terms for a man’s brother’s son and brother’s daughter are themselves lexically marked versions of the “son” and “daughter” terms, respectively. In his words they are “terms derived from those for son and daughter” (151). These are all terms of reference. As for “terms involving address within the immediate family,” these are “shortened forms of the non-vocatives denoting endearment” (153), comparable to English “mommy” and “daddy.” Moreover, in some of these populations the “mother” term is derived from terms meaning “to suckle” and “breast” (153; compare English “mama” and “mammary”), and, more directly related to procreation, it means literally “the one who bore me” (156, note 3). The corresponding “father” term is translatable as “my generator” (156, note 1).<sup>8</sup> Finally, in some of the groups adjoining the Cree, the “cousin” terms are lexically marked versions of the “sibling” terms, and the marker means “*not* by blood descent” (157, note 4; emphasis added). Again, as in the Malay case, local notions of kinship are not so very different from what we find in English.

A further example pertains to some of the Native American peoples of the southern Great Plains and to adjoining populations of the Great Basin. Hoebel (1939: 447f.) notes that one’s parents’ same-sex siblings are subclassed *relative to the linking parent*, as “big” or “small” versions of the latter, de-

pending on relative age. This is to say that parent enjoys focal status in his/her class, other members being defined *relative to* him or her. This is a common feature in so-called “classificatory” terminologies: Wake (1889: 476–479), drawing on Morgan’s tabulations in “Systems,” noted it for Tamil-speakers in South India just after Morgan’s passing, and similar reports have come to us since.<sup>9</sup> Moreover, in Hoebel’s words, “The br[other] category receives several striking extensions” (1939: 448). “A wife-absconder and the aggrieved husband,” he tells us, “call each other ‘brother,’ though until a legal settlement has been made ... they are dangerous enemies.” “This terminological practice seems to stem from the notion that the males who share a married woman sexually should be brothers ...” (448; emphasis added). Hence the native extension rule, “Let any man cavorting with my wife be terminologically equated with my brother.” Moreover, “[t]he institution of formal friendship among men also entails the use of the brother terminology. The friend ... takes the status of his ‘brother’ in the relationship system of his comrade’s family ...” (448). Hence a further native extension rule, “Let my formal friend be terminologically equated with my brother.”

Speck’s findings on the Cree and their neighbors and Hoebel’s on the South Plains/Great Basin were by no means unusual for their time.<sup>10</sup> Indeed, they have continued to be reported, including in many of the classic ethnographic cases (Shapiro 2018).

(3) “Ideologies of substance-sharing, which, if considered, would have also obviated the performativist position, have been in the ethnographic record since Tylor’s day. Again, it goes almost unrecognized in the new kinship literature.” Although not nearly so abundantly analyzed as kinship terminologies, they point in the same direction. Over a hundred and fifty years ago Tylor (1865: 300–313) brought to the attention of social theory “the couvade,” a practice exemplified especially in Aboriginal Latin America in which a child is held, Tylor emphasized, to be in a post-partum bond of substance with its father. Subsequent research, especially in Amazonia but elsewhere as well,<sup>11</sup> has shown that the mother shares this tie too. Doja puts it cogently:

Most ethnographic accounts of couvade insist that both parents are protecting the infant’s vigor and assisting its fast growth through fasting [and other observances]. But

8 Westermarck (1894: 88f.), writing on other populations, was apparently the first to note the common use of procreative notions in the construction of kin terms.

9 E.g., Beck (1972: 216), Nayacakalou (1955: 48), Strathern (1980).

10 See, e.g., Freire-Marreco (1914), Harrington (1912), Junod (1912: 222–226), Kroeber (1917), Walker (1914).

11 E.g., DaMatta (1973: 279f.), Doja (2005), Valentine (2002).



it is important to stress that violations of the taboo not only harm the child but can also turn against the father or mother. ... Both father's and mother's fasting and inactivity strongly identify them with the newborn *with whom they form a community of substance* (2005: 930; emphasis added).

Doja (2005: 931) further points out that sometimes a wider sphere of kin is emphasized. But this is not true in all cases, and when it is, the injunctions to more distant kin are attenuated, on the grounds that *they are held to share less substance with the child* (see esp. Aijmer 1992: 8 and associated references). These points are entirely lost on Beckerman and Valentine (2002: 3), in their grand claim that Amazonian "partible paternity" – the idea that several acts of intercourse are deemed necessary to create a child, whose mother may have extra-marital lovers – challenges the "One Sperm, One Fertilization Doctrine" espoused by "Western" science, itself dependent on "our common Western view of [single] paternity as universal." As we shall see, such charges of ethnocentrism and the parochialism of science are common in the new kinship studies. The thing to note now is that Beckerman and Valentine utterly misrepresent what we know about Amazonian social theory, including the *couvade*. As I have shown (Shapiro 2009b: 41 f.), throughout the area *primary* *couvade* linkages to the child pertain to the mother and her husband, who enjoys the most sexual access to his wife and is, therefore, most likely to be the actual father of her child; her extra-marital lovers are deemed to be much less closely tied to the child. As one Amazonian ethnographer puts it, "the most direct bodily connections are those reinforced on a daily basis among parents and children who sleep and eat together" (Conklin 2001: 118).

Carsten's Malay analysis is relevant here as well. Her claim is that, in the village she studied, "ideas surrounding co-eating and sharing are as fundamental ... as are ideas about procreation" (1991: 425). This allegation rests on the idea that, just as blood is generated by procreation, so too is it held to come about through wet-nursing and commensality, because food is said to be transformed into blood within the body. This way of putting it suggests that the latter two sources of blood are *modelled* on the first – a conclusion consistent with Carsten's own statement that

[J]ust as relatedness [i.e., kinship] is thought of in terms of a continuum – one is more or less distantly related ... we find a parallel in terms of substance. ... Mothers and their offspring and full siblings are most closely related. ... More distant than full siblings but still close enough for marriage to be incestuous are those, such as foster sib-

lings, who have drunk the same milk. [Others] brought up in one house who have shared meals ... could technically marry. In fact they are very unlikely to, because to do so would carry connotations of incest (1992: 38).

And elsewhere Carsten (1990: 271) tells us that "[s]hared consumption is epitomized above all by siblings ..." The obvious conclusion, surely, is that the "fundamental" means of generating kin-ties among Malays is indeed procreation.<sup>12</sup> This is also the case if we move from the begging of life to its end. Thus, among the Kayapó of central Brazil, "those mourning the death of a member of their immediate family (for example, a spouse, sibling, or child) have their hair cut short" (Turner 1980: 117). This is apparently because

[p]arents are thought to be connected to their children, and siblings to one another, by a tie that goes deeper than a mere social or emotional bond. The tie is imagined as a sort of spiritual continuation of the common physical substance that they share though conception ... Although spouses lack the intrinsic biological link of blood relations, their sexual relationship constitutes a libidinal community that is its counterpart. In as much as both sorts of biological relationship are cut off by death, cutting off the hair, conceived as an extension of the biological ... self, is the symbolically appropriate response to the death of a spouse as well as a child (Turner 1980: 117).

(4) "Local notions of spiritual animation have been misinterpreted by performativists." Thus, Sahlin (2013: 4 f.), drawing on Godelier (2011: 229–280), writes of a "third party" in ideologies of conception – i.e., a spiritual one in addition to the mother and father. In this connection, it is surely no accident that a group of scholars intent on divorcing kinship from biology (see below) should seize upon some of the well-known claims of an alleged "ignorance of physiological paternity," as Sahlin (1976: 37–39), Carsten (2000: 8), and Franklin (1997: 33–43) have all done. But none of these claims has withstood further analysis. Thus, Malinowski (1916: 407) initially claimed that in the Trobriands "the state of knowledge ... is just at the point where there is a vague idea as to some *nexus* between sexual connection and pregnancy, whereas there is no idea whatever concerning the man's contribution towards the new life which is being formed in the mother's body." Conception is held to occur,

12 In her initial publications, Carsten was nearly silent on the role Malay fathers are held to play in procreation, but others have had more to say on the matter (see Shapiro 2011b: 144). Indeed, Laderman (1987: 75) tells us that the fetus is said to originate in the father's brain. Carsten (2004: 129) seems more recently to echo this.

he tells us, when a spirit-child enters a woman, her husband only “opening the way” through repeated copulations (412f.). But even in this classic article Malinowski unwittingly presents contrary evidence. Thus he describes a ritual bath undergone by a woman “four of five months after the first symptoms of pregnancy” (404). The ceremony, he tells us, “is connected with the incarnation of the spirit children” (405), and he further notes that “[t]he view taken by one of my informants was that during the first stage of pregnancy the ... [spirit-child] has not really entered the woman’s body. ... Then, during the ceremonial bathing, the spirit child enters the body of the woman” (405). In “The Sexual Life of Savages” these data are repeated (Malinowski 1929: 225), but we also learn that “[p]regnancy is first diagnosed by the swelling of the breasts and the darkening of the nipples. At *this* time a woman may dream that the spirit of one of her kinsmen brings her the child from the ... [spirit] world to be reincarnated” (211); emphasis added). In other words, the woman is *already* pregnant when entered by the spirit-child. This is made plainer by Read (1918). Writing only two years after Malinowski’s initial formulation, he states expressly that spirit-entry is held to occur at fetal quickening. Thus what we are dealing with is *not* a *conception* ideology at all: rather, *it is a doctrine about the generation of the spiritual aspect of the person*, and, as such, it is comparable to baptism in Christianity, as well as other metaphysical ideas which Aijmer (1992) has dubbed *animation*.

It is, therefore, unsurprising that spirit-entry is held to be *antithetical* to physical generation. Thus Glass, drawing on more recent research in the Trobriands, tells us that “[t]he Trobrianders were very guarded about articulating their knowledge of [physiological] paternity *for fear of offending ‘the ears of the spirits’*” (1986: 60, emphasis added).<sup>13</sup>

Another example of spiritual animation cited by Sahllins (2013: 82–84) more recently pertains to the Mae-Enga of the Papua New Guinea Highlands. Here Meggitt (1965a: 163) tells us that the father’s contribution to the fetus is openly acknowledged but what is emphasized instead is entry into the mother’s body by the spirit of a patri-clan ancestor. As with the Trobriands, this entry is held to occur not at conception but a fetal quickening (Meggitt 1965a: 163). Moreover, such ancestors are placated in male secret/sacred ritual, held in a men’s house from which women are barred (Meggitt 1965b:

115). Further, it is considered offensive to the ancestors if a married couple copulates in the vicinity of the men’s house and/or just after they – the ancestors – have been propitiated, for to do so would “antagonize” them (115, 118). There is thus much the same spiritual/carnal opposition as we found in the Trobriand case.

Sahllins (2013: 9) observes that “the Eskimo-speaking peoples must be the world champions of postnatal kinship”, and it is clear that recent ethnographic studies of these peoples have played a large role in making a case for the performativist position.<sup>14</sup> Nuttall’s materials on an Inuit population in Greenland (1992, 1994, 2000) are especially pertinent, partly because they supply a great deal of information on Inuit naming practices and their connection to kin classification. Indeed, Nuttall (1994: 133) insists that “name beliefs [among Greenland Inuit] raise questions ... for kinship theory in general.”

Naming here is a form of ensoulment: an individual is said to be a reincarnation of a deceased relative whose name he or she is given (Nuttall 1992: 60, 67–69, 89; 1994: 127). This is so much the case that names influence the employment of kin terms: thus, for example, a boy who is given the name of his maternal grandfather may call his own mother “daughter” (Nuttall 1992: 68, 131). Moreover, he may assume the kin terminological position of someone else who has this name: he may call that person’s wife “wife,” etc. So we have another native extension rule, viz. Let anyone my name-giver or name-sharer called (or calls) X be called X by me. But this is less impressive than it seems. Here are Nuttall’s words:

This does not mean that actual kinship, that is, kinship based on putative biology is entirely forgotten about. Non-biological ties ... are ultimately recognized as fictive when sexual relationships and inheritance are taken into consideration. The equivalence of [kin] terminology does not mean the equivalence of blood, so there are no incest taboos that apply between an individual and the actual kin of his/her name-sharer ... Furthermore, inheritance also recognizes the importance of genealogical connection. A son ... inherits ... from his father. ... [B]ut (those connected through naming) have no claim ... to property (Nuttall 1992: 68, 131; see also Nuttall 1992: 92; 2000: 44, 46).

In short, among Greenland Inuit kin ties through naming are *modeled on* those based on procreation. This being so, they do *not* have the significance for kinship theory that Nuttall claims.

<sup>13</sup> This is a highly truncated version of a more comprehensive study of Trobriand kinship currently in preparation. See Shapiro (1996) for remarkable parallels in Aboriginal Australia.

<sup>14</sup> E.g. Bodenhorn (2006), Fienup-Riordan (2005: 219–221), Nuttall (1992).

(5) “Elementary semantic operations are unrecognized, with the result that analysis is deeply flawed by sloppy conceptualization.” Thus, Chock (1974: 33), in her study of Greek-American spiritual kinship, insists that “[s]piritual kinship in Greek-American culture is of *equal symbolic status* as [sic] blood ... kinship” (emphasis added). I suspect she means that both forms of kinship are symbolic, that “blood kinship” for these people is a “construct” and not an unmediated reading of nature. This bit of triteness is a regular – and uncontested – claim in the “new kinship studies,”<sup>15</sup> and it is entirely beside the point. The *real* empirical issue is whether, for Greek-Americans, one of these two forms of kinship provides a *model* for the other, and it is quite clear from the data Chock presents that “blood kinship” does indeed serve as a model for spiritual kinship. Here are some of the parallels she adduces in tabular form, supplemented by my own clarifications:

physical birth	spiritual rebirth
womb	baptismal basin
blood (of parturition)	water (of baptism)
umbilicus cut	hair cut

Chock (1974: 44) explicates by telling us that

informants ... remarked on the birth symbols of baptism, including the similarity between the shape and function of the baptismal basin and the womb. The child is spiritually reborn from its water [basin] ... [B]its of [its] hair are cut off. These symbols thus contrast with the symbols of physical birth.

It seems fairly clear from this that her informants *modeled* spiritual rebirth on physical birth, that they *likened* the former to the latter. This interpretation is supported by the way these informants depicted godparents, with whom a relationship is established at baptism:

People say that godparents are “like spiritual parents,” ... A godparent is a “spiritual mother” or a “spiritual father.” Furthermore, children who have been baptized by the same person are believed to be related as “brothers and sisters of the oil” ... This expression refers to the fact that their godparent anointed them with olive oil at their baptisms. They cannot marry. ... In the same way, one cannot marry the child of one’s godparent ... The prohibition is sometimes extended to a sibling’s godparent’s child ... [One informant] said that she had once dated the son of her brother’s godmother and “there was a big commotion ... because it was like dating your brother” (Chock 1974: 38 f.).

Note the *likenings* – “like a spiritual parent,” “like dating your brother”: in each case the *focus* is a biological relative. Note also the use of lexical markers (“brothers and sisters of the oil,” “spiritual mother,” “spiritual father” – indicating *specialization* and *derivation* –, and again the focus is biological relationship. Finally, note the *extension* of marital prohibitions, almost certainly based on comparable *extension* of notions of relationship. The conclusion seems to me inescapable that, if we are to use the expression “equal symbolic status” with any rigor, Chock’s thesis is entirely false, a distortion of the way Greek-Americans represent the world.

Weston’s analyses of gay/lesbian families in the San Francisco Bay Area turns Chock’s claim, so to say, upside down, but effectively says the same thing, and is equally mistaken. Weston (1995: 99) claims that gay families are “just as real” as heterosexual families. The expression, like Chock’s “equal symbolic status,” is used rhetorically, without rigorous meaning, and is, therefore, beyond contestation. The real issue is the semantic status of such a family: as I have pointed out elsewhere (Shapiro 2010) it is quite plainly a *derivative* one, and its focus is heterosexual families (see also Peletz 1995: 348, 364). Why else is it labelled a *family*? There are other examples of semantic modeling in Weston’s corpus; for details again see Shapiro (2010). There is as well as solid evidence of some of her informants’ grappling with what they rightly construe to be a heterosexual model. Here is Weston’s summary of their position:

Why speak of lovers, friends, or even children as kin? “We” ... should develop “our” own terminology to describe “our” experiences, rather than adopting “their” (heterosexual) language (1992: 122 f.).

This would seem to suggest a recognition by these informants that modeling is *in status nascendi*, and that its base is heterosexual kinship: there is considerable evidence for this in Weston’s corpus (again see Shapiro 2010). It also suggests an apparently minority opinion that gay people should stake out a path of their own, rather than imitate an established kinship model – which, I submit, is precisely what they do.

A third and final example is provided by an updating of Ward Goodenough’s Truk analysis by Marshall (1976), who provides a description of various forms of performed “siblingship” on Truk without any recognition that all of them, as Goodenough’s classic analysis makes plain (1951: 99 f.), are semantically derived from procreative siblingship, either by lexical marking or some other process (but see Marshall 1983: 206). Here is what is apparently Goodenough’s last statement on the matter:

15 See, e.g., Carsten (2000: 10 f.), Franklin (2001), Yanagisako and Delaney (1995: 13–15).



When people enter into nonkin relationships to which they give the rights and duties of a kin relationship, they may analogically extend kin terms to these relationships. Thus Americans may say, "We are like brothers." In ... [Truk], people who are close friends will say publicly "We are in a ... ["sibling"] relationship" ... In America and ... [Truk] alike, people recognize that these relationships are modeled on genealogically based relationships and that they are not "true" in this latter sense (Goode-nough 2001: 210; see also Scheffler 2001: 166f.).

Marshall, for his part, enters the comparative arena as well, claiming that various communities studied by other scholars "are all reported to define kinsmen primarily on the basis of performance" (Marshall 1977: 651). But the words "define" and "primarily" are used without rigor, probably meaning "posit" in the first instance and, in the second, something like "has a lot of." At any rate, I know of no ethnographic case in which local *definitions* of kinship do *not* assign *semantic primacy* to procreative notions, as these are locally posited: most of this essay is a partial demonstration of this proposition.

(6) "Related, calling attention to the fabricated or nonfocal nature of performative kinship is seen by performative scholars not as a matter of semantic analysis but as one of denigration and ethnocentrism, and it therefore stimulates, utterly unnecessarily, adversarial rhetoric." Hence Weston's remark, noted above, about gay kinship being "just as real" as its heterosexual analogue. Hence, too, Schneider's charging me with ethnocentrism for using the rubric "pseudo-procreation" (Schneider 1989; Shapiro 1988), as if the Aboriginal Australian rites noted by Hiatt (1971), wherein elements of the male and female contributions to reproduction are mimicked, were *not* derived from these elements, or as if, *pace supra*, Greek-Americans did *not* model their ideas about spiritual kinship on their ideas about physical kinship. A handy summary of this mistaken attitude is provided by Marshall (1977: 644), who writes as follows: "Kinship ties that are neither consanguineal nor affinal have been called many things in the anthropological literature (for example, 'fictive,' 'pseudo,' 'ritual,' 'artificial'), all of which imply that these relationships are somehow not 'real'." But there is nothing wrong with these labels insofar as they imply native *modeling*, which they do, and I know of no anthropologist who uses them in a pejorative way.

(7) "Having thus created a non-existent gulf between procreative and performative kinship, more recent performative scholars mistakenly marry this distinction to a West/Rest one, and, even worse, to

a Manichean one, thus creating what D'Andrade (1995) has called 'a moral model'." The structure of this model is as follows:

*performative kinship* : *procreative kinship* :: *Rest* : *West* ::  
*Good* : *Evil*

To which should be added

*communal kinship* :: *individual kinship*

Thus McKinnon (2005a: 59) laments a Lost Paradise characterized by "an expansive understanding of kinship," which, she argues, survives in the Third World of Latin American peasantry in the form of *compadrazgo*, which is of course historically derived from the westernmost part of Western Europe. Elsewhere (McKinnon 2005b) she instances "classificatory" kinship terminologies, which, she contests, provide for "a multiplicity of mothers" (2005b: 112), as opposed to maternal singularity in the West; but she is utterly oblivious to the immense evidence for lexical marking in such systems, as well as such English expressions as *godmother*, *step-mother*, and *Mother Superior* (see Shapiro 2008 for a more detailed critique). Zimmer-Tamakoshi (2001: 192) writes of "a relatively rigid, Western biological view of kinship." And Weston (1991: 196) complains of "the genealogical logic of scarcity and uniqueness." All this is preposterous: it is as if performative scholars have never heard of such Western notions, whereby members of a trade union are "brothers and sisters"; whereby all Christians, especially those "born again" as adults, are "brothers and sisters in Christ"; or as if all the evidence cited in this essay on the centrality of procreative kinship in the non-Western World did not exist.

(8) Related, the very positing of such a non-existent gulf indicates that, far from representing non-Western ethnography in 'indigenous terms' (Carsten 1997: 292) – the grandest of the grand claims of performative scholarship –, "the more recent performative scholars misrepresent this ethnography."

I have already documented this in the present essay, as well as in several other places (see above). I would add here only that, for performativists, the time-honored tasks of serious inquiry into human variation and the unity of humankind are abandoned in favor of the promulgation of a moral model.

(9) "Related, subscription to this paradigm excuses performativists of the scholarly responsibility of commanding any of the kinship literature published between Engels and Schneider, so it is hardly surprising that the latter is viewed in virtually messianic terms." Thus Faubion (2001: 5) suggests that "the



anthropological study of kinship might be divided into ‘pre-Schneiderian’ and ‘post-Schneiderian’ periods.” Other commentators<sup>16</sup> are only slightly less enthralled. Many of the key figures in the history of kinship studies – Westermarck, Kroeber, Lowie, Murdock, Goodenough, Lounsbury, and Scheffler, for example – are effectively banished from the curriculum of the “new kinship studies.” All the egregious flaws in Schneider’s scholarship<sup>17</sup> are ignored. Knight (2008: 69–70) and Weismantel (1995) represent the logical outcome of these tendencies, evoking the image of a vast right-wing conspiracy bent on hiding from the anthropological world the insights of Marx and Engels. Some idea of how absurd this argument is can be gleaned from Shapiro (2009a).

(10) “Related, there is recurrent appeal in the performativist literature to some of the unsustainable claims of so-called ‘radical feminism’.”<sup>18</sup> In particular, Friedrich Engels’ “The Origin of the Family, Private Property, and the State” (1972 [1884]) is accorded a venerated place in the new kinship studies. Engels (1972: 113) imagined that at an earlier “stage” in human “prehistory” “the position of women [was] not only free, but honorable,” and he linked this high ranking with the absence of the nuclear family – the *bête noir* of “radical” feminism – and “mother right.” So – as if to instantiate his words – Carsten emphasizes that her Malays live in houses composed partly of matrilineally-related women who, she claims, are “central to the political process” (1997: 18). She ignores completely the compartmentalization of these houses into nuclear families (Shapiro 2011b: 143 f.). As for the “politically central” claim, it is gainsaid by a mass of data, including one of the photographs in her *magnum opus*: its caption is “Men vote at a village meeting” (Carsten 1997: 141), and, accurately enough, it contains not a single woman.

16 E.g., Bamford and Leach (2009: 9), Brettell (2001: 48), Terrell and Modell (1994: 158).

17 E.g., Kuper (1999: 133 ff.), Scheffler (1976), Wallace (1969).

18 The temporal and geographical proximity to Morgan – he lived in Rochester, New York – is remarkable, especially in the light of Morgan’s incipient feminism. Thus he writes as follows: “It ... remains an enigma that a race, with endowments great enough to impress their mental life upon the world, should have remained essentially barbarian in their treatment of the female sex ... Women were not treated with cruelty ... within the range of the privileges allowed them; but their education was superficial ... and their inferiority was inculcated as a principle ... The wife was not the companion and the equal of her husband ... The wife is necessarily the equal of her husband in dignity, in personal rights and in social position” (Morgan 1877: 474 f.).

But I am unaware of any scholarly treatment of the matter.

Her misanalysis is by no means unique. Thus an alleged “new anthropological view” of the near-universality of the nuclear family by Collier and her associates (Collier et al. 1992) maintains that the Mundurucú of central Brazil, studied by Robert and Yolanda Murphy (Murphy 1960; Murphy and Murphy 1974), lack the nuclear family because village men reside largely in a special “men’s house,” with women resident, apparently collectively, in separate dwellings. This recalls the Mae-Enga case, noted above. But this “collective” residence is decidedly compromised by several factors. First, a man regularly visits the women’s quarters, and when he does he associates *exclusively* with his own wife and their dependent children, providing them with meat from the hunt and fish from the catch. Second, he leaves his personal possessions near his wife’s hammock, not in the men’s house. Third, when ill, he leaves the men’s house and resides with his wife, so that she can minister to him. Finally, there are various symbolic expressions of the unity of husband and wife, along with their common children. So, as with Carsten’s Malays, the all-female collective is an illusion. As for the men’s house it-self, it is not just a place where men sleep and spend many of their waking hours. *It is a locale from which women are absolutely barred*, on penalty of gang rape, and within it are held rituals of a very high order of misogyny. That the traditional Mundurucú should be held up as exemplifying a feminist paradise seems therefore nothing short of delusional.

Even more revealing is what has happened to those Mundurucú who have become involved in the Brazilian rubber trade. This involvement has led to the disappearance of both the men’s house and the allegedly collective female residences, and *it has been instigated by the women*, who plainly prefer to reside permanently with their husbands and children. The men, for their part, tend to wax nostalgic about the former regime. So the suggestion, made by Robert Murphy (1959) himself, is that the relatively isolated nuclear family encourages the diminishment of sexism, and that a regime of “extended family” households is what encourages the mistreatment of women (Shapiro 2011a).

All this, of course, is *directly* contrary to Engels. These two cases show with remarkable clarity that the conjoined fantasies of the all-female collective and the absence of the nuclear family readily trump anything resembling accurate rendering or reading of ethnographic materials. What’s more, Murphy seems to have been on the right track. To see this, all one has to do is to consider where – and under what conditions – the modern feminist movement emerged. The place, apparently by common agree-

ment, was Seneca Falls, New York:<sup>19</sup> It was not Moscow or Beijing or a village in Amazonia or the Papua New Guinea Highlands. It occurred more or less simultaneously with the European political revolutions of 1848 – more than three decades before Engels wrote, nearly seven decades before Lenin replaced the tyranny of the czar with the Tyranny of the People, but only fifteen years before the abolition of slavery in the United States. Among its more or less immediate predecessors were changes in British law which allowed women to inherit their fathers' private property (Tobias 1997: 15–17). All this seems lost on “radical” feminists – including their representatives in the new kinship studies – who, like other utopian intellectuals, want the world made perfect the day before yesterday.

An especially outrageous example of radical feminism's influence on the “new kinship studies” is provided by one of the most regularly cited articles in the performativist corpus, i.e., Weismantel's essay on adoption in a village in the Ecuadorian highlands, which claims to find a “masculinist bias” in pre-Schneiderian kinship studies (1995: 692). She expands, in what I regard as the single most irresponsible statement in all the performative literature:

[O]ne aspect of this masculinist heritage that has remained largely unexamined is the emphasis upon sexual intercourse as the single moment in which paternity becomes embodied. The authors of classical kinship thereby universalized a heterosexual masculine perspective, derived from traditional bourgeois life, in which men defined their role in the family primarily in terms of sexual access to the wife and a distanced authority over the children. Extended nonsexual physical contact, especially with children ..., or nurturance for other family members, was defined as feminine and demeaning ... (Weismantel 1995: 697).

Weismantel (1995) claims that all this “is alien to the experience” of the people among whom she worked. Let me suggest that it is also alien to the experience of a very large number of Westerners, “bourgeois” or otherwise. I cannot pronounce upon the numbers involved, just as Weismantel has no idea as to how many “bourgeois” families fit her stereotype: apparently, she sees no need to cite any historical or ethnographic research on such families. Her article consists almost entirely of actual instances showing that people get attached to those who adopt them in early childhood, which is hardly news, and recurrent bashes at “bourgeois” understandings of sociality. She claims that, in the com-

munity in which she carried out fieldwork, “there is absolutely no privileging of the relationship a child has with the genitor and genitrix over others who are called parents” (1995: 691), and that “[o]ften ... the reverse is the case ...” (691), but we are given no idea as to *how* often, and under what conditions, and there is absolutely nothing on kin classification.

In any case, her book on this community (Weismantel 1988) is not at all consistent with these claims. Here she notes that “[t]he nuclear family is the conceptual basis for the ... household” (169), and that there are “extended and fictive kin” (171). Moreover, she tells us that “[a] man or woman refers to his/her mother-in-law or father-in-law as ‘mother’ and ‘father’,” respectively, but the reciprocal terms are *not* the unmarked “child” terms, or any lexically marked version thereof. Rather, *they are purely affinal terms* (171): hence it is *not* true that all others called by parental terms are undistinguished from actual parents. These affinal “parents” are said to be “not really kin” (171), nor are they behaviorally like real parents; on the contrary, they are far less nurturant than exploitative: Weismantel (1988: 171–174) spends some time instancing examples of callous treatment of sons- and daughters-in-law by the parents of their spouses. Moreover, there is considerable stigma – very “bourgeois,” this – attached to producing a child out of wedlock, even if the couple marries before the child is born (170).

(11) “Related, performative scholars claim, quite mistakenly, that certain ethnographic findings demonstrate the unimportance of procreative kinship in at least some non-Western communities.” The claim by Storrie (2003: 408) that the Hoti of the Venezuelan rainforests “do ... not recognize any idea of genealogical connection between persons” is surely the most astonishing example; but it is gainsaid by his employment of native theories of parent/child linkage in questioning informants (410f.); and then by his list of Hoti kin terms, including the information that “parent” terms, when used in the possessive form, isolate the “biological father” and “the biological mother” (412). Also pertinent is the proposition, fairly widely found in the performative literature, that, just as performative ties can be maximized in practice, so procreative ones can be minimized or neglected altogether. This is, supposedly, especially true with regard to adoption, wherein the seeking out of birthparents by adoptees is put down by performativists to a Western obsession with biological connection.<sup>20</sup> But as I have shown elsewhere (Sha-

19 “I cannot bring myself to call these women ‘radical’ feminists, as they are usually labeled, because I do not believe they go to the root of anything” (Patai 1998: 136).

20 E.g. Bowie (2004), Terrell and Modell (1994), Weismantel (1995).

piro 2015a), adoption in most of the world involves lexical marking – i.e., it is generally regarded as a *secondary* form of kinship, birthparents are frequently reluctant to give up their children and virtually always retain ties to them, including the ability to reclaim them (see also Silk 1987).

The flexibility of non-Western kinship is a recurrent theme in the “new kinship” literature.<sup>21</sup> There is indeed such flexibility, in both parts of the world in fact, but it has its limits: relatively *distant* procreative ties can be ignored or even cut, but usually not close ones.<sup>22</sup> The plain fact is that most human beings do *not* just forget about their parents, siblings, or children.

(12) “Performative scholars subscribe to a ‘culture’/‘biology’ dichotomy which is utterly obsolete.” Thus, Sahlins (1976: 22), in his supposed critique of sociobiology, distinguishes between “a naturally given set of ‘blood relationships’” and “a culturally variable system of meaningful categories”; and in his recent (and also supposed) synthesis on kinship (2013), he declares flatly that human kinship is “culture,” not “biology” – as if these expressions pointed to anything more than an Everyperson’s innate/learned dichotomy, abandoned in the hard sciences for at least four decades (Lehrman 1970). McKinnon (2005b: 127) claims that evolutionary psychology insists upon “universal forms of behavior,” in apparent ignorance of the reliance on probability calculi in virtually all research in this field. And she adds (117), in fine “deconstructionist” style, that it exemplifies a “restrictive understanding of kinship ... that is a reflection of Western upper-class concerns.” She fails utterly to see its indebtedness to the idea on brain modularity of Noam Chomsky, not a man generally associated with “reactionary” political views. Cassidy (2009) marries this antiquated view of “biology” with the pre-Schneiderian concern with “the genealogical model,” about which she says the following:

It reduces individuals and events to the playing out of ... inevitable properties ... It presents the contingent as necessary, and in doing so provides evidence supporting the most conservative of interpretations of the present. Much of its utility depends on an ability to separate the model from society, to allocate it a place within “nature” ... (2009: 24 f.).

No one is spared the all-seeing eye of “deconstruction”: even Darwin is exposed as An Enemy of

the People and gets his comeuppance in the volume in which Cassidy’s essay appears.<sup>23</sup> More, the following excerpt from one of Marx’s letters to Engels appears at least twice in the performativist literature (Sahlins 1976: 101 f.; Yanagisako and Delaney 1995: 5): “It is remarkable how Darwin recognizes among beasts and plants his English society with its division of labour [read, diversification], competition, opening of new markets [niches], ‘inventions’ [variations], and the Malthusian ‘struggle for existence.’”

So Darwin’s thought occurred in a particular social and economic environment. It’s unclear how this distinguishes him from anyone else, including Marx and the new kinship scholars. Thus, Wilson (2016: 573) turns the tables on the performativists by arguing that

a conception of kinship indeed has been projected from “the West” to “the Rest” in [performative] kinship present ... This conception of kinship reflects the shift in kinship structures in the West in the 1960s and 1970s ... [This shift was exemplified by] working mothers, the spread of contraception, skyrocketing divorce rates, Brady Bunch families, communal living and free love [and] sexual liberation ... Combined with developing technologies of reproduction, those in the West had new ways to live ... That [new] conception of kinship was then projected onto societies subject to past colonial and imperial influence ... .

But of course the purpose of the quotation from Marx is not illumination but denigration. Let me suggest instead that Darwin is a rather bad choice for an argument that people (except, of course, “deconstructionists”) are just parrots for an established regime, as well as, *pace* Cassidy, the contention that his sense of genealogy “reduces individuals and events to the playing out of inevitable properties.”

Performativists need to recall the title of Darwin’s most famous book “On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life” (1859), as well as the importance of differential reproduction and speciation in his overall theory. This is the very opposite of Cassidy’s implication that his model of evolution “presents the contingent as necessary.” Such a preformationist model (Richards 1992) is the one proffered by Morgan and other “classical evolutionists” (Carneiro 2003). Although Darwin could never fully rid himself of it,<sup>24</sup> this is not why he is generally considered the greatest fig-

21 E.g., Bodenhorn (2000), Carsten (1995), DeMallie (1994).

22 See, e.g., Beattie (1971), Marshall (1977: 659), Weston (1991: 43–75).

23 Bamford and Leach (2009: 6), Cunningham (2009: 112), Ingold (2009: 208).

24 E.g., Bowler (2009: 163 f.), Gruber (1981: 181–184, 197–199), Richards (1988: 137).



ure in the history of biology. The radical distinction between the two senses of “evolution” is widely appreciated by historians of social theory.<sup>25</sup> Moreover, far from echoing his *Zeitgeist*, Darwin was quite aware that his formulations, especially on the mutability of species, would offend extant religious sensibilities (Gruber 1981: 202–204, 209–213). Indeed, as Bowler (1986: 41) has argued, his “making natural development an essentially haphazard ... process” “challenged the most fundamental values of the Victorian era.”

The shoe, really, is mostly on the other foot: it is *performativists* like Sahlins and McKinnon who subscribe to a performance model (to which they add certain Manichean quality). For them “culture” is imagined as replacing “biology,” and as antithetical to it. At the same time, “biology” is construed as something apart from “learning” or “socialization,” somehow “inherent” in individuals, and “its” consequences, unless countered by “culture,” are seen as inevitable. This has absolutely nothing to do with current conceptualizations of the organism/environment interface in biology;<sup>26</sup> it is, again, nothing more than an Everyperson’s ideology.

With this antediluvian view of biology, performativists claim that those who stress procreative kinship ideas are advocates of a “reductive” (or “essentialist” or “biologized”) view of the human situation, views which, we are to believe, held in unison by the Western scientific “establishment” and Western folk theory.<sup>27</sup> It is, of course, nothing if not vogueish these days to “deconstruct” various scientific claims, or even the whole scientific/medical enterprise (Gross and Levitt 1994). Such “deconstruction,” it bears noting, should not be confused with earnest history-of-ideas scholarship, which seriously examines theories in their social and intellectual contexts. Its *only* purposes, as noted, are denigration and rhetorical one-upmanship, and it carries the implication that those who “deconstruct” are part of an elite class who have somehow managed to surpass the biographical and social constraints that entrap the rest of us and see into Ultimate Reality. A corollary of all this, widely maintained, as we have seen, in the performative literature, is that biology too is “culture,” implying in turn that there is no reason to grant it privileged status. Thus, not only can the old kinship studies be ignored, “reductive” as they are, but so too can a very considerable lit-

erature on native science apart from the reproductive process.<sup>28</sup> Non-Western people are viewed as corseted in “cultures,” devoid of any scientific traditions of their own, and as having no interest in Western science; in truth, this is not very far from the worst Victorian and pre-Victorian images of “savagery.” All knowledge, after all, is held to be “constructed” – except the knowledge of “deconstructionists,” which, to them at least, is quite real. Consider the sheer pretentiousness of the following remarks:

[T]he presumption of the structuring importance of the “facts of life” as they are defined by Western biological science [has] enormous theoretical consequences ... The production of sexual differences, the maintenance of heterosexuality, the operation of an *a priori* domain of “natural fact,” [and] the presumption of a reproductive telos at the base of social organization all ... evade critical *recognition* ... as a result of being so taken-for-granted [that] they remain invisible. ... [T]hese features ... of a century of anthropological debate [on kinship] can be directly traced to the post-Darwinian worldview of Euro-American anthropology (Franklin 1997: 49, emphasis in original).

(13) “Performative scholars participate in what might be dubbed as ‘feudal’ academic community, entirely at odds with the universalistic values that academic institutions are supposed to live by.” They repeatedly cite one another – and in often laudatory terms, as if each one had made a serious contribution to knowledge, one which is beyond question. Thus, Carsten (2013: 245), self-involved in the sort of literary flourish common among performativists, judges Sahlins’ recent synthesis (2013) to be “wonderful” and “intuitively graspable – not as an analytic abstraction, as many definitions of kinship seem to be, but in a way that palpably makes sense of a whole range of human experience.” Schneider’s analysis of Yapese kinship is described as “brilliant” (Terrell and Modell 1994: 158), despite the fact that it has been called into question by several scholars.<sup>29</sup> On the other side of the emotional spectrum, Weismantel’s vitriolic contribution, as I have already noted, is regularly cited, but hers is only the starkest expression of an overall hostility to contrary views. The most common response to such views is to ignore them. Scholarly debate, when it occurs, is almost invariably *ad hominem*, either blatantly or by assigning to adversaries unfashionable political views (“arch-conservative”), social

25 E.g., Carneiro (2003: 171 f.), Nisbet (1969: 159–208), Richards (1992).

26 E.g., Barkow (2006), Oyama (1985), Pinker (2002: 100–102).

27 E.g., Franklin (2001), Franklin and McKinnon (2001), McKinnon (2005a, 2005b).

28 E.g., Brown (1984), Goodenough (1996), Sanga and Ortalli (2004).

29 E.g., Labby (1976), Lingenfelter (1975), Shimizu (1991).

class positions (“bourgeois”), or gender attributes (“masculinist”).<sup>30</sup>

(14) “Since ‘biology’ implies inevitability, ‘culture,’ its Manichean counterpart, implies untrammelled freedom.” In practice this amounts to the untrammelled freedom of alleged authorities to dictate people’s lives. This utterly disrespects the complexity of the human nervous system as well as human creativity and initiative (see my last epigraph), including the ability to question authority. As noted, performative scholars see themselves as part of a larger “deconstructionist” project, allegedly collaborating with other “revolutionary” movements to free the world from the constraints of capitalism, the nuclear family, and the erotic and power lusts of White heterosexual males. But here, as elsewhere, their “ethnography” is deeply flawed: by my lights the greatest power on a university campus these days lies with the local Sexual Harassment Office, seconded by the political intolerance of both faculty and students, programs peddling “diversity” (but insisting on conformity), and the almost complete “adaptability” of administrators (since the late 1960s, in fact) to extant fads.

(15) Last, and worst of all, “all this bodes badly for the future of kinship studies – indeed, for education at large, which in any case has been on a downhill course since the late 1960s.” This is because performative scholars have come to constitute part of an Establishment on university campuses. Even with the best of intentions, university administrators, not being (except in rare cases) kinship experts, will believe that those who claim to be so actually *are* – all the more so when they are willing – *more* than willing to marry their “expertise” with current trends in social thought, particularly “postmodernism” and “radical” feminism, which appeal to students and potential students and, thus, help such institutions to meet their bottom lines. Much the same can be said for funding agencies, whose grants depend largely on “expert” advice. In this way, such an Establishment becomes self-perpetuating: it is looked to for advice of this sort, including what to do about hiring, promotion, and student admission. Finally, and also self-perpetuating, a professoriate of under-edu-

cated pseudo-scholars will have in their classrooms far more devotees than students with a right to question; the latter, if tolerated at all, will likely be sent for “sensitivity training.”

## Concluding Remarks

In one of the very many self-congratulatory “syntheses” attempted by performative scholars, Schweitzer (2000: 214) writes of “the ‘outdated’ nature of pre-Schneiderian kinship studies.” But such a triumphant tone, as I believe I have shown, is warranted only if one conflates scholarly progress with intellectual sloppiness and academic faddishness. The performativists’ position seems reasonable to some, because they ignore systems of kin classification or have sophomoric views of their semantic structure, and because they have no idea of the overwhelming evidence for the extensionist position in the early literature and beyond – indeed, even in much of their own published work. The “cutting edge” they have generated aligns what was once anthropology’s gem with Afrocentrism, Goddess and matriarchy theory, the idea that gender is merely a “social construct,” and other hokums that have been developing since the 1960s.

All this is bad enough, but in addition, performativists have married it to utterly discredited collectivist theories of human sociality and the unsustainable West/Rest dichotomy that drives those theories. They are, I think it fair to say, part of a Brave New Academic World in which professorships – even the status of “public intellectual” – are granted, based less on scholarly achievement and more on pretentious writing, *faux* commitment to hopelessly failed social and political ideas, gender (female, transgender), sexual preference (gay/lesbian), racial category (non-White, in the process of becoming, at least in the United States, non-Asian as well), misrepresenting The Other (as Primitive and/or Matriarchal Communists), and, of course, the ability to “deconstruct” what one knows next to nothing about.

Finally, it seems to me long overdue that we abandon the absurd argument that procreative kinship is a Western perversion of the “essentially” collective nature of human sociality, allegedly to be found in one part or another of the non-Western world. Focality and extension in kin classification are not fabrications of a vast right-wing conspiracy bent on hiding from us the “insights” of Friedrich Engels, or David Schneider, or Marshall Sahlins: *they have been in the ethnographic record for over a century*. We find them whenever one of our informants tells us that his or her genetrix is the

30 The one exception that comes to mind is a debate between Linda Watts and myself (subsequent to Shapiro 2009a), which, though heated, was nonetheless scholarly, if I may say so. It is, I think, significant that Watts is one of the few performative scholars who takes systems of kin classification seriously, and who is familiar with the older literature. The “arch-conservative” charge was leveled against me by Sahlins (2012).

“true” member of her kin class, and whenever he or she notes that he/she calls a particular man “father” because his/her “true” father called him “brother.” This being so, the suggestion is that the nuclear family is something to which human beings are inclined by their species heritage and not the disposable product of a particular “stage of society” or a particular economic regime or “arch-conservative” social views. The number of exceptions to its near-universality in the ethnographic record on the Third World can be counted on the fingers of one hand. More urgently, there are exceptions all around us in the shape of “alternative family forms.” But even this expression suggests *modeling*, and it should be clear by now what this model is. The *real* achievement of the performativists, though I doubt that they would ever admit it, is *not* that they show that the nuclear family is absent or unimportant in some communities but, quite to the contrary, that it is *so* important nearly everywhere that people *model* other social ties on it.

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