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enced the ravages of disease (Reff 1986:229–230).

Jesuit materials as well as other colonial sources suggest that it was not until the fourth quarter of the 16th century, following the rapid expansion of the Spanish mining frontier and the related development of an extensive trade network, that a suitable infrastructure existed that facilitated the regular northward movement of disease agents from southern Mexico into the Greater Southwest.¹ The founding of Franciscan and Jesuit missions further intensified contact within and without the northern frontier, paving the way for regular, widespread epidemics, including the epidemic of 1636–41 (Reff 1986).

In his commentary, Dobyns accused me of uncritically accepting several reports that the epidemic of 1636–41 involved smallpox.² In an apparent effort to demonstrate another pandemic, Dobyns argued that this epidemic actually involved scarlet fever, which was not distinguished at the time from smallpox. In support of this thesis, Dobyns noted that the epidemic affected Europeans as well as natives, and persisted in some locales for over a year. While it is true (as I noted in my original commentary) that Europeans often confused or failed to differentiate diseases, I am reluctant to dismiss the statements of at least three different observers that smallpox was involved in the epidemic of 1636–41. This is not to suggest that scarlet fever as well as other maladies did not contribute to the epidemic. Indeed as I noted on at least several occasions in my commentary, epidemics of a single malady were uncommon during the 16th and 17th centuries; circumstances such as malnutrition that encouraged smallpox also favored the spread of maladies such as pneumonia, influenza, typhus, and so on. The fact that most epidemics involved multiple disease agents explains in part why Europeans and natives frequently suffered during epidemics, although it is true that both incidence and mortality rates were invariably highest among native peoples. It should also be noted that it was quite common for diseases to ravage a community or area, subside, and then, many months or a year later, reappear (Reff 1986:237). The various areas (e.g. Santa Fe, Parral, Monterey, Zacatecas, Mexico) that reported smallpox or other diseases between 1636–41 were integrated by the *Camino Real* of the interior and other heavily traveled roads, which made it possible for a disease to move from one locale to another and back again. There is nothing problematic, therefore, about a community like Monterey or the Parral district suffering for a year or two from smallpox and other mal-

adies, or the whole northern region experiencing related outbreaks of disease over the course of five years.

Notes

¹Although most areas of the Greater Southwest do not appear to have been affected by Old World diseases prior to 1575, southern and central Sinaloa as well as the mountains to the east did witness tremendous population losses prior to this date (Reff 1986).

²It is unclear why Dobyns is willing to reject several different reports of smallpox in 1636–41, while readily accepting a passage from Obregon's *Chronicle* that mentions an officer who was suffering from "measles" in 1564. Significantly, the *maestro de campo* (Betanco) apparently was one of Ibarra's Basque followers. Since measles characteristically was a childhood disease among Europeans, Obregon, who wrote his *Chronicle* 20 years after the events in question, is likely to have erred in his recollection of what ailed Betanco.

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Questioning the Correlational Evidence for Kipsigis Wealth as a Cause of Reproductive Success Rather than Polygyny as a Cause of Both Extra Children and Extra Wealth

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I applaud Borgerhoff Mulder's (AA 89:617:634, 1987) effort to establish a causal link between cultural and reproductive success, and the pains she has taken to eliminate rival hypotheses. There is one point in her argument, however, that I find totally uncon-

vincing. After establishing the correlation between wealth and reproductive success in different Kipsigis age cohorts, she goes on to state that "objections can be made to the interpretation that wealth differences are the *cause* of reproductive differentials among males" and "wealth differences . . . may be a consequence, not a cause of family size" (p. 624). She then argues the following:

Were this argument valid, positive correlations between wealth and family size would not be expected in recent cohorts, where offspring are still too young to be productive. In fact, the association between wealth and family size is equally strong in the recent Korongoro cohort, where the mean age of offspring is only 2.9 years. This suggests that wealth differences are a cause, not a consequence, of difference in reproductive performance. [p. 624]

The argument for wealth as a cause of reproductive success, including offspring via polygyny, is flawed in that it ignores the role of additional wives in generating wealth among the Kipsigis. As the author states, "polygyny is the major cause of reproductive variance in Kipsigis men" (p. 626). This is likely to be true for the most recent cohort: since the average time to acquisition of the second wife is ten years (with variance), and the recent Korongoro cohort covers a 14-year age range, there are clearly polygynists in the youngest cohort. In this cohort it is likely that the number of offspring are correlated with the number of wives. The causal chain, then, may well be that extra wives generate extra wealth and extra children. This would account for the observed correlation between wealth and offspring even in the most recent cohort. The appropriate test of the author's hypothesis is thus whether there is a correlation between wealth and offspring for *monogamous* males in the youngest cohort. I should like to see a report of this correlation before judging the evidence for her hypothesis.

The fact that the first wife sometimes instigated the taking of a second (p. 620) would seem to indicate that polygyny is an investment in the future productivity of the family, and that additional wives generate additional wealth. This would be consistent with the fact that parents are not averse to polygynous marriages for their daughter, and that her husband's having a first wife may indeed be viewed as an advantage (Borgerhoff Mulder 1985:36).

If, under my alternative hypothesis, additional wives augment both wealth and offspring, there is an additional argument that

must be pursued. The fact that wives are acquired through payment of bridewealth (p. 627) does not mean that polygyny, in turn, is caused by additional wealth. The wealth that is utilized in bridewealth payments may be generated for the first wife (financed by the father) by the polygynous wives of the father, and for subsequent wives, by wives of the husband. That is, even if it is only men from wealthier families who can marry second wives due to high bridewealth requirements, the bridewealth raised by the father for his son's first marriage may be the result of wealth produced by his polygynous marriages.

This critique is not necessarily devastating to the argument that wealth causes reproductive success among the Kipsigis, depending on the result of the appropriate statistical test, but it very well could be devastating. It is consistent, however, with the high standards for the assessment of hypotheses that Borgerhoff Mulder has used in her analysis, and with her critique of studies in a variety of societies (Hill 1984; Irons 1979; Chagnon 1979; Faux and Miller 1984; Kaplan and Hill 1985; Flinn 1986), many of which are also polygynous, in which the authors try to establish a link and causal direction from wealth to reproductive success. My alternate interpretation is also more consistent with the author's own findings reported elsewhere (Borgerhoff Mulder 1985:34):

Preliminary results show that monogamously and polygynously married females do not differ markedly in fertility-per-year-married. . . . This study also suggests that polygyny, far from constituting a relationship thwarted with tension [taken to be an indicator of conflict over resources], may actually offer subtle benefits to Kipsigi women. . . . Indeed Kipsigi women appreciate the economic costs and benefits of marriage. They view marriage as a necessary means of acquiring rights to land and cattle for themselves and their children. [p. 37]

There are, of course, major implications for Darwinian evolutionary theory, depending on the outcome of attempts to invalidate either or both types of arguments proposed above. Cross-cultural corroboration of the hypothesis that wealth (or, more generally, cultural success) causes reproductive success (and polygyny as one means of such success) would have the effect, for example, of placing many instances of human polygyny in the sociobiologist's category of "resource-defense," an argument that was proposed unsuccessfully by Hartung (1982). It is quite possible, however

(see Gray 1985), that human polygyny fits neither the sociobiological model of resource defense or that of harem-defense (which seems, at least, more appropriate for Betzig's [1986] despotic polygyny). Human beings are wealth-augmenting for one another, via production and food sharing, in a way that differs, at least in detail, from many other animal species. The manner in which this wealth-augmenting potential among humans is arranged within a society is a basic and variable feature of social structure and culture and consequently a critical variable to take into account in the comparative and Darwinian evolutionary study of societies. It certainly bears further investigation as to whether multiple wives are not wealth-augmenting under certain ecological conditions (White 1988; White and Burton 1988), thus inverting the sociobiological emphasis on males "producing" wealth, attracting mates, and producing more offspring because of wealth that is assumed to be controlled by males. Would it not, for example, also make sense to look at the production and manipulation of wealth from the female perspective, as an aspect of their reproductive strategies?

Male-generated wealth may well increase male reproductive success in many instances (see, e.g., Turke and Betzig [1985] for Ifaluk, where monogamy is the contemporary norm and salaried males may have higher reproductive success; the impact of women's contributions to the family, however, have not been analyzed). What I am arguing is that there are also many instances where females generate wealth in addition to males, and that this is often the case in polygynous societies if there is ample access to land and other resources (White and Burton 1988).

If we are to apply Darwinian evolutionary theory to human society, an application I support in principle if not in certain research practices or assumptions of human social biology, should we not investigate more carefully whether the appropriate concept has been applied? Harem-defense or territorial-defense are not sufficient evolutionary models of human polygyny. We need to recognize the prevalence of a type of polygyny where co-wives are producers and potentially wealth producing for their families. It is the warrior sons in such systems, at least in precolonial times, who are mobilized for defense and warfare. Here, it is common both for sons to defend their fathers' polygyny and to recruit wives of their own from the capture of women. These features of polygyny are not captured by either the concept of harem-defense or that of territorial- or resource-defense.

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Polygyny and the Extent of Women's Contributions to Subsistence: A Reply to White

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White's pursuit of high standards in the study of polygyny raises a crucial issue: the relationship between a woman's marital status and her productivity. Here I present further evidence, including a new analysis proposed by White, in support of my argument that, among Kipsigis, the wealth a man uses to marry additional wives is obtained by means largely *independent* of the contributions of co-wives.

1. To acquire a wife Kipsigis men must make a substantial payment of livestock and, since 1960, cash (Borgerhoff Mulder 1988a). A girl's parents' choice of son-in-law is influenced both by his bridewealth offer and by the size of his land plot. These conclusions are supported by my interviews with parents (Borgerhoff Mulder 1988b), as well as by an analysis of the marriage sequence of Kipsigis men who settled a pioneer community in the 1930s and 1940s (Borgerhoff Mulder 1989a). Land, livestock, and cash are therefore the principal resources required for marriage and polygyny. How are each attained?

Land. Since restriction of Kipsigis to Native Reserves in the Colonial period very little land has become available locally for expansion (Manners 1967); only a few Kipsigis families emigrate to new settlement areas such as the Mau Forest, and must compete there (generally unsuccessfully) with Kikuyu over land. With rapid rates of population growth land is becoming an increasingly limited resource, such that no amount of female productivity can significantly enhance a man's access to new land.

Livestock. Labor is often viewed as a critical limiting factor in pastoral production systems (Dahl and Hjort 1976), because only with sufficient labor can optimal herd management practices be implemented, leading to improved cow/calf ratios, milk yields, calf survivorship, and growth rates. Kipsigis women play a major role in the husbandry of cattle

and small stock, through milking, herding, watering, and general inspection; indeed, men view wives as economic assets (Peristiany 1939:73-75). However, three sources of evidence suggest that polygyny in Kipsigis men is not contingent on the enhanced productivity of their herds: (a) strict cultural rules prevent a man from using the products of his wives' labor, or the bridewealth stock paid for his daughters, to procure subsequent wives for himself (Peristiany [1939:205, 211] discusses the economic independence of co-wives' households; see also Borgerhoff Mulder [1987a]); (b) there is a range of alternate means of obtaining labor, such as cooperation among age set and clan members, the *kokwet* (communal neighborhood work teams) and *kimanagan* (stock-loaning partnerships), which raises the question whether polygynous men effectively enjoy greater access to labor than monogamous men; (c) the number of a man's wives is correlated with the number of cattle he inherits (Borgerhoff Mulder 1989b), suggesting that cattle-wealth generated independently of a man's wives' productivity plays some role in determining polygyny (without excluding the potential role of polygyny-generated wealth).

Cash. Some men sell stock to raise cash for bridewealth, but if these stock are all allocated to a particular wife the cash proceeds are technically hers (see above), although violations occur; generally, however, a husband is unwilling to use this cash to acquire a second wife, since this can precipitate his first wife's desertion. Other men raise cash through their own efforts in cultivating maize for sale (Peristiany 1939:130, 140); others trade in livestock (between Kisii and Masai); yet others conduct petty trade (e.g., in tobacco) or sell their labor. While wives' labor inputs could to some extent facilitate a man's ability to raise cash by giving him time to pursue these off-farm activities, neither the conduct of trade nor regular labor employment was positively associated with the number of a man's wives ($X^2 = 3.88$ and 2.18 respectively, $n = 619$ men, $df = 2$, n.s.; no effects found within cohorts, so data are lumped).

Finally, in some instances White implies that greater productivity of polygynous households is due to cooperation among co-wives, presumably through economies of scale (page 176). In line with this suggestion, polygynously married Kipsigis women do experience some benefits in the provision of infant care, primarily due to the greater availability of caretakers (Borgerhoff Mulder and Milton 1985). As regards productive work, however, my suspicions, as well as those of the original