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On Studying Fertility at High Altitude: A Rejoinder to Hoff

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In this vein, it is important to note that a year before its publication, I was made aware of the contents of a draft of this review article. As a result, I called one of the coauthors to convey the major points raised in this reply. I also sent photocopies of portions of relevant papers and statistics that contradicted their statements. The coauthor agreed their review article was in error regarding their statements about the Andean studies and I received assurances appropriate corrections would be made. These changes were never made and the article was published in essentially its original form.

Finally, there are two other very important issues raised by the publication of this paper by Goldstein et al. (1983): the review policy of the *American Anthropologist* and the thoroughness with which reviews are made of papers containing substantial critiques and presentations of the research of others. Concerning the first point, it would seem appropriate (as is customary in other journals) to allow those whose work is being criticized the courtesy of examining and responding to the contents of such papers before their publication. This was not done in our case. In such situations, it also behooves the reviewers and editors to examine more closely the allegations being made and the validity of the evidence being presented to support such claims. In my opinion, if such procedures are adopted, the *American Anthropologist* will set a standard of scholarship that will enhance the reputation of anthropology among the other scientific disciplines (where I fear the general feeling is that anthropologists equate polemics with scholarship).

I would like to end with a relevant quote that succinctly expresses my feelings about this matter. "Anthropologists seem to take great delight in cutting one another off at the knees, even though it makes none of them taller. As we are close to being an endangered species anyway, I suggest we concentrate more on the accuracy of our [own] work and less on the destruction of [that of] other anthropologists" (Wright 1983:10).

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On Studying Fertility at High Altitude: A Rejoinder to Hoff

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Hoff's comments illustrate the methodological and conceptual problems that have permeated the literature dealing with the effects of high altitude hypoxia on human fecundity and fertility, the topic of our 1983 article (*AA* 85:28-49). That article emerged from our effort to reconcile the widely varying fertility levels reported in the literature on high-altitude Tibetan-speaking populations in the Himalayas. We eventually discovered that cultural factors were the principal cause of this variation. In particular, we demonstrated the necessity of controlling for variation in the exposure of women to the risk of sexual intercourse and conception. When this was done, the statistically significant differences in fertility initially found among Himalayan high-altitude populations and between these and low- and moderate-alti-

tude Himalayan populations vanished and there was no evidence supporting the hypothesis of a hypoxic depression of fertility in the Himalayas.

This finding prompted our examination of the Andean literature on this issue and we found similar wide variations in the completed fertility ratios (CFR) reported. For example, the CFR for women age 45+ in six high-altitude native populations in the Andes ranged from 5.8 to 8.5 births per woman (Goldstein et al. 1983:43) and compared with a range of 5.4 to 8.3 in CFRs of five low-altitude Andean populations.

Our review of the Andean literature revealed that the main case for the presence of hypoxic depression on fertility in the Andes derived from the widely cited comparative study of Hoff and Abelson on high- and low-altitude natives in Peru. This study, in turn, was based on the two separate studies of Hoff (1968) and Abelson (1972). As indicated in our original article, we found serious methodological and conceptual flaws in both these studies, as well as other information that led us to seriously question their conclusions.

Hoff's comments on our article question our understanding of his conclusions, our evaluation of his methods, and our academic integrity. Let us now respond to his points.

Hoff states that "Goldstein et al. (1983) claim that we are the main proponents of the hypothesis of significant hypoxic depression of Andean fertility, when in fact, we say repeatedly this is a highly fertile and fecund population." Here Hoff confuses two very different issues with respect to the fertility of high-altitude Andean populations: (1) whether high-altitude Andean populations are "highly fertile," as he puts it, in an absolute sense and (2) whether high-altitude hypoxia acts to reduce fertility from some level that could be attained if the population were not exposed to chronic hypoxic stress, regardless of whether the resultant fertility level is "high" or "low." Our concern was solely with the latter issue. This was stated clearly in the abstract to our article where we wrote: "it [the paper] argues that the claims for a statistically significant difference in the fertility *between* high, moderate, and low altitude Himalayan populations are groundless, and suggests that a parallel reevaluation of Andean findings is required" (Goldstein et al. 1983:28; emphasis added). The theoretically significant issue that we addressed was clearly not whether Andean or Himalayan natives are "highly fertile" but whether there is any scientifically valid evidence to support the

hypothesis that high-altitude hypoxia reduces the fertility of resident populations relative to moderate- and low-altitude populations, whatever the absolute level. We did not claim that Hoff or anyone else argued that the high-altitude native Andean populations have *low* fertility on some absolute scale due to hypoxia, for that is irrelevant to the theoretical and methodological issues we focused on. Indeed, Goldstein has written elsewhere (1981) on the relatively high fertility of Tibetan populations whose CFRs are equivalent to those Hoff reports for Nuñoa. Note that Hoff cites no quotation from our paper to document his charge that we accused him of articulating this view. The focus of our paper was clearly on the second issue, the existence of hypoxic depression of the fecundity/fertility of high-altitude natives relative to lowlanders.

In 1976 Hoff and Abelson (1976:144) concluded: "Hypoxia, which acts to reduce fecundity, appears to be the major component of reduced fertility at high altitude." However, in his comment on our article Hoff writes: "In retrospect, the wording [of that conclusion] was unfortunate and, if one were to read only this paragraph, one might gain the mistaken impression that we felt there was a significant depression of fertility in this population." Hoff's statement again confuses the two distinct issues of absolute and relative fertility levels discussed above. We interpreted his original comment, as well as his overall article, to argue that (1) high-altitude native populations have reduced fertility in relation to comparable low-altitude native populations, and (2) this is caused primarily by hypoxia. That is what we feel he intended and this is clearly what we conveyed in our article and what has been widely cited in the past. Hoff himself continues to argue this in his comment on our paper, although he now does so with less assuredness: "we suspect the realization of the full reproductive potential may be below the theoretical maximum because of altitude stress." This is far less emphatic than the 1976 conclusion, but it illustrates that we have not erroneously attributed this view to him.

Our paper critiqued the sampling and methods, and therefore the findings, of Himalayan and Andean studies of fertility, including, of course, that of Hoff. Hoff's response to the critique does not address our basic points. For example, he presents a calculation based on a confidence interval of ± 1 birth to demonstrate that his sample of 31 women age 45+ approaches statistical adequacy. But statistical

calculations are meaningless if the samples to which they are applied are not representative. The probability of a nonrandomly chosen sample exhibiting bias increases as the sample size decreases. Hoff's sample of 31 women age 45+ is not a random sample. Indeed it is not strictly a sample of women; the information on women's reproductive histories was provided by their current husbands who were recruited for biological testing (Hoff 1968:101). In addition to the potential error inherent in a methodology that elicits women's reproductive histories from their husbands, the problem is exacerbated in Nuñoa because many of these husbands are the second or third spouse of a woman and would be unlikely to be very familiar with infant mortality or other details of a previous marriage.

There are other reasons why we questioned the representativeness of his sample. Hoff wrote (1968:100): "Due to the cultural [and apparently biological] homogeneity of the population it is felt that the sample is representative of the *Indígena* inhabitants of the district." The question of homogeneity or heterogeneity is critical because any attempt to test such a hypothesis must not only obtain research findings that are consistent with that hypothesis but must also rule out plausible alternative explanations (Brim and Spain 1974). One way of doing this is to establish that other factors cannot explain the findings, that is, that there are no confounding factors operating. For example, what are the fertility differences between women who remained married to the same man for the entire duration of their reproductive years and women who had two or more spouses? Hoff himself was aware of this problem because he wrote:

Marriages are particularly unstable among the Nuñoan population. . . . It was found that 29.4% of the sample had terminated the first union. Only 17.6% of this group remarried at a mean age of 24.9 (± 11.2 years). . . . Average time between unions was about 3 years. . . . It is not possible to state that this marital instability affects exposure time of the women and plays a role in lowering net fertility. . . . In summary, it is not yet possible to assess the effect of marital instability on net fertility. [1968:109]

However, since Hoff included such women in his sample, his CFR_{45+} may reflect lost reproductive time due to marital instability. Similarly, the effect of temporary separations of fertility is another potentially confounding factor not addressed by Hoff. What are the fertility dif-

ferences between women whose husbands continuously lived with them in Nuñoa and those whose husbands spent considerable periods of time somewhere else as migrants, a pattern common among Andean high-altitude residents (Thomas 1976)? This too could produce a lower CFR_{45+} . Without a careful demonstration that potentially confounding factors such as these do not influence fertility, Hoff's assumption of a homogeneous population is not sound. We are now aware that this is particularly important with regard to fertility because of our demonstration that in the Himalayas the large differences in fertility reported in the literature are to a large extent artifacts of differential exposure to the risk of intercourse among different populations, that is, social rather than biological factors. Fertility cannot be used as a reflection of fecundity unless all social and cultural factors that might reduce fertility are controlled for, and this was not done in Hoff's study.

Hoff also argues that his data are representative because,

Goldstein et al. (1983) report that the CFR means reported for other Andean populations and Nuñoa are statistically homogeneous. . . . Therefore, this further strengthens the contention that the Nuñoa sample is demographically representative of Andean populations.

A far better indication of the representativeness of his CFR findings would be comparison not with all Andean populations of high and low altitude, but rather with other studies in the same area (Nuñoa). It is significant, we think, that nowhere in his comments does he address the inconsistency between his and Way's (1972) findings for the same Nuñoan population even though that study appeared before the publication of the joint Hoff and Abelson (1976) article and even though we raised the issue in our paper. Way (1972) cites a figure of 9.1 pregnancies for high-altitude Nuñoan women age 45+, whereas Hoff reported only 6.7 births. Way (1972) also reported *fewer* pregnancies for low-altitude women age 45+. It is this kind of variability that led us to conclude that confounding factors have not been adequately controlled for in the studies of Hoff and others. Similarly, Hoff does not discuss our parallel criticisms of Abelson's data from a low-altitude Peruvian site, despite the fact that the validity of the high-low altitude comparison rests on the validity of the data of both populations.

That Hoff still tries to defend this method-

ology as perfectly adequate and casts aspersions on the reviewers of the *American Anthropologist* for allowing publication of our criticisms is illustrative of the need we felt to air a whole series of problems in the literature on high-altitude hypoxia and fertility/fecundity.

It is also interesting to note, with regard to the variation in fertility reported among high-altitude Andean populations, new findings from another high-altitude demographic study that support our original observation of a wide, but equivalent, range of CFRs at both high and low altitude. Godoy (1983) studied high-altitude Bolivian natives located in four highland villages in Jukumani (3,500m). Fertility data were collected by interviewing all available women in the households present during the time of his investigation. He reports that the CFR_{45+} for 68 highland women was 8.05 (S.D. 2.86). The difference between this CFR and that reported by Hoff (6.7) was statistically significant at the 0.05 level.

Hoff also charges us with incorrectly stating that he did not present age-specific fertility data in his 1968 study and refers us to pages 117-118 of that study where he says it appears. Hoff is in error on this issue, apparently because he does not understand what age-specific fertility rates are. On those pages he discusses what he calls the "child-woman mean," which he calculates as the cumulative number of births to women at various ages. Thus, for example, for women age 31-35 he cites a child-woman mean of 4.32, or, on the average, each woman in his sample in the age range 31-34 experienced 4.32 live births. This, however, is not the age-specific fertility rate, which is defined with regard to a specific duration of time, usually one year, as:

$$\frac{\text{Number of Births to Women of an Age Group}}{\text{Total Number of Women in that Age Group}}$$

Age-specific fertility (or birth) rates tell us the number of births to women of a certain age experienced during a particular period of time (Barclay 1958:48-50). When one sums these age-specific fertilities one achieves a total fertility rate. This is completely different from Hoff's "child-woman mean." We strongly agree with Hoff's suggestion that readers verify this by checking his original study themselves. However, it is interesting to note that Abelson, Hoff's coauthor on the later comparative article, agrees with our interpretation (1972:41). In a similar vein, Hoff states that a regression equation of completed fertility of women under

45 years of age produced a predicted CFR value for women age 45+ very close to his observed mean and that this "strongly suggests the observed CFR is accurate for the Nuñoa population." We strongly disagree. Whatever cultural factors may be acting to reduce fertility or to bias the sample in the 45+ age cohort would likely be operating among the younger women also. Thus, extrapolations from the reproductive data on younger women cannot be used as independent validations of the data from the older women.

Hoff accuses us of unethical behavior by claiming that a coauthor of our study "agreed their review was in error regarding their statements about the Andean studies" and that he "received assurances that appropriate corrections would be made" which were not. This is preposterous. The coauthor in question merely agreed to reexamine Hoff's writings taking into consideration his comments. We did not accept them then and do not now.

We also want to state that Hoff's comment that we criticized Dutt for advancing a hypothesis of hypoxic depression of fertility is another misreading of our article, where we wrote (1983:42): "Furthermore, Dutt (1976, 1980) reported *no* statistically significant difference in the fertility of the high, middle and low altitude Bolivian samples he studied."

Finally, we disagree completely with Wright's (1983:10) scientifically naive statement about scholarly debate and conflict, which Hoff cites as expressive of his own feelings. All scientific knowledge is subject to critical investigation and revision; this is the heart of the development of new ideas and the solving of old problems. We are shocked to realize that Hoff has taken our critiques of the existing studies as an attempt to destroy other anthropologists. We are shocked because our commitment has been, and will continue to be, the advancement of our understanding of human social and biological adaptation, in this case to the stress of high-altitude hypoxia.

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Anthropological Realities

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Greet Kershaw's "A Breath of Fresh Air" (*AA* 85:119-120, 1983), together with her rejoinder (*AA* 85:122-124, 1983) to Susan Kent's critique (*AA* 85:120-122, 1983) of this short article, reflects a perspective on the nature of anthropology—its intellectual foundations, scope, and application—that I find very disturbing.

I have absolutely no objection to Kershaw's advocacy of training for the kind of work and the types of jobs for which she believes there is an important future, and I heartily agree with her that there is no incompatibility between idealism and employment. Similarly, who can be against the teaching of courses that enhance the skill and knowledge of those likely to make their living in a variety of applied fields? What does concern me is the projection of a particular image of the discipline—what it has been, what it is, and what it should become—and the argument that is made in favor of a set of anthropological priorities. That such matters are indeed being addressed is clear from the final paragraph of the first paper:

Departments, programs, and faculty need to have the courage to ask themselves whether they should change fundamentally and not just cosmetically. Anthropology in the 20th and 21st centuries is not an updated version of the ideas of the 19th century, but a whole new world in which we and our students work. [Kershaw 1983a:120]¹

Kershaw, I suggest, comes close to portraying anthropology as a simple polarity between the useless and the useful. On one side of the conceptual divide is the stuffy world of "academia" becalmed in "the doldrums"; on the other, "the real world of work and exciting knowledge." This real world is presented as *modern* ("where are the courses which deal with the modern world?"; "speak about this century"), *proximate* ("right here in our own back yard"), and *engagé*, in the sense that the thrust of research and application is in the direction of understanding and resolving such problems as racism