

ries. Here attention is necessarily paid to mating effort and therefore to the frequency-dependent payoffs that follow from male competition for mates (see, e.g., reviews in Roff 1992:273–82; Charnov 1993: chap. 3). Since females are the key resource for males, they cannot be ignored entirely, but detailed reference to female strategies appears only when female choice for male traits is the focus of attention. This fundamental asymmetry means that the study of male strategies cannot ignore females as completely as most life-history work ignores males. Still, the life-history strategies of each sex often show patterns that seem to be generally explicable without much reference to the other.

Putting human patterns in the wider context of evolutionary biology can completely refashion the questions that the human patterns pose. Hrdy's (1981:chap. 1) brief review of classic explanations of male dominance is a fine example. As the classic explanations all invoke causes unique to humans, recognition that the pattern is *not* unique to us but generally characteristic of primates shows the need for a more general explanation. Of course it could be that different reasons apply for each species, but the greater the coincidence of patterns the more improbable the independence of the causes. Chisholm's equation of earlier maturity and shorter life spans with high mating effort does not put human patterns in their wider primate or mammalian context. Instead, it obscures the wider patterns, deflecting attention from the trade-offs that have been the focus of study in recent life-history work (see Hill's 1993a review for anthropologists) and from the pervasive differences in reproductive strategies between males and females. As a consequence, it gives no hint of the real story brewing in the evolution of life histories (especially for those of us trying to figure out what *men* are doing). Could it be that when we want to understand the fertility patterns catalogued by demographers—which depend on *female* life histories—the strategies of males play little role? From the perspective of life-history theory, each sex faces different trade-offs in allocating reproductive effort. Obscuring these fundamental differences does not serve the goal Chisholm himself specifies, that of introducing social scientists to this rich body of work.

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Chisholm presents an integrative environmentalist perspective on human life histories, consciously avoiding "genetic determinism and insensitivity to historical contingencies—especially those affecting inequalities due to race, class, and gender" (p. 1). Unfortunately, political purity has caused him to miss the already large literature on human life histories that cannot be accounted for by his theory. He also overlooks the many behavior-genetic studies showing the substantial heritability of individual and racial variation in the traits described.

A formidable challenge for Chisholm's account is the inverse relation empirically to be observed between brain size and gamete production across human racial groups and their association with numerous other biobehavioral variables. No environmental factor is known to account for the trade-off between brain size, speed of maturation, and reproductive potency or to cause so many diverse variables to correlate in so comprehensive a fashion. There is, however, a genetic factor: evolution.

The extensive array of evidence for race differences from around the world that I have assembled (Rushton 1988, 1992, n.d.) shows that for more than 60 variables people of East Asian ancestry (Mongoloids, Orientals) and people of African ancestry (Negroids, blacks) define opposite ends of the spectrum, with people of European ancestry (Caucasoids, whites) falling in between and much variability within each broad grouping. This racial matrix occurs in brain size (average cranial capacity 1,360, 1,350, and 1,290 cm³), intelligence (IQ 107, 100, 85), maturation rate (age at walking, age at first intercourse, age at death), temperament (activity, cautiousness, impulsivity), social organization (marital stability, mental health, rule following), reproductive effort (gamete production, intercourse frequency), and hormones (testosterone, gonadotrophins).

For brain weight at autopsy, Ho et al. (1980) summarize data for 1,261 American subjects aged 25–80 years after excluding obviously damaged brains. They report a significant sex-combined difference between 811 whites, with a mean of 1,323 g (S.D. = 146), and 450 blacks, with a mean of 1,223 g (S.D. = 144), a difference that persists after controlling for age, stature, body weight, and total body surface area. For endocranial volume, Beals, Smith, and Dodd (1984:307, table 5), having computerized the world database of up to 20,000 crania, report sex-combined differences by continental area. Excluding nonfrost areas of Asia and frost areas of Africa, 19 Asian populations averaged 1,415 cm³ (S.D. = 51), 10 European groups averaged 1,362 cm³ (S.D. = 35), and 9 African groups averaged 1,268 cm³ (S.D. = 85). For external head measurements, I have examined data from a stratified random sample of 6,325 U.S. military personnel and found that, after adjusting for the effects of stature, weight, sex and rank, the cranial capacity of Asian-Americans averaged 1,416 cm³, that of European-Americans 1,380 cm³, and that of African-Americans 1,359 cm³.

Around the world, the rate of dizygotic twinning (caused by double ovulation) is less than 4 per 1,000 births among Mongoloids, 8 per 1,000 among Caucasoids, and 16 or more per 1,000 among Negroids (Bulmer 1970). A similar racial ranking is found with measures of triplets and quadruplets (Allen 1988). Studies of Mongoloid-Caucasoid crosses in Hawaii and Caucasoid-Negroid crosses in Brazil indicate that multiple birthing is inherited largely through the mother (Bulmer 1970).

Although Chisholm mentions *r/K* theory, he is not enthusiastic. However, he provides little justification for his avoidance. The racial pattern corresponds well to the *r/K* scale. At one end, *r*-strategists emphasize high

reproductive rates; at the other, *K*-strategists emphasize parental investment. Although the scale is generally used to compare the life histories of disparate species, I have used it to describe the very much smaller variations among human subspecies. To stress that all humans are *K*-selected relative to other animals I have referred to this proposal as "differential *K* theory" (Rushton 1988, n.d.). Mongoloids are more *K*-selected than Caucasoids, who are more *K*-selected than Ne-*g*roids.

The *r/K* model makes unique predictions applicable to human races. Across 21 primate species, Smith (1989) found age of first molar eruption, a developmental variable under strong genetic control, correlated (0.98) with brain size; that is, the more delayed the rate of physical maturation, the larger the brain. Smith interpreted her data in terms of the *r/K* life-history model. Among humans, Africans have an earlier age of first molar eruption than Europeans or Asians as well as being faster on numerous other indices of physical maturation (Rushton n.d.).

The *r/K* scale of reproductive strategy can be mapped onto human evolution. Genetic-distance measures indicate that archaic versions of the three races emerged from the ancestral hominid line in the following order: Africans about 200,000 years ago, an African–non-African split about 110,000 years ago, and a Caucasoid–Mongoloid split about 41,000 years ago (Stringer and Andrews 1988). African populations, the earliest to emerge, are the least *K*-selected and Mongoloids, emerging latest, the most. Such an ordering fits with and explains how and why the variables cluster.

The question for Chisholm and life-history theoreticians is how to explain the statistical matrix of data on race differences. Similarly challenging is the huge body of knowledge being built up by behavioral geneticists from both twin and adoption studies showing substantial heritabilities for all the within-race variables of interest (Bouchard et al. 1990).

Chisholm refers to my work only to dismiss it as an example of deterministic bias, but this charge is untrue. Building on the work of the behavioral geneticists I have proposed that a 50% genetic plus 50% environmental model fits the data better than the (in effect) 100% extremist alternatives discussed by Chisholm. Although genes provide the initial set point, environmental factors move individuals up or down the continuum of reproductive strategies. The genetic leash may, of course, be a very long one (Lumsden and Wilson 1981).

The humanitarian desire not to cause harm and to promote well-being in disadvantaged groups has had the dark side of seriously restricting the Darwinian perspective in the human sciences. Degler (1991) provides a historical account. Perhaps, as Jensen (1984) once suggested, human sociobiology will begin to make real progress only after the race issue has been met head-on. No claim is made, however, that races are discrete groups. Black officers in the U.S. Army data average as large a cranial capacity (1,369 cm³) as white enlisted personnel (1,366 cm³).

Reply

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Hawkes believes that I erred in equating "fast" life-history strategies with high mating effort and "slow" life-history strategies with high parenting effort—that is, that I incorrectly equated the trade-off between current and future reproduction with the trade-off between mating and parenting effort. The reason this is wrong, in Hawkes's view, is that whereas I used the trade-off between current and future reproduction (i.e., the short-term vs. the long-term strategy) to explore differences *within* the sexes, the trade-off "between mating and parenting underlies differences *between* male and female reproductive strategies" (my emphasis). While I accept that I perhaps did not make sufficiently clear the distinction between the two trade-offs and agree that they are not invariably the same, I disagree with Hawkes's assertion that the trade-off between mating and parenting effort cannot be used to analyze within-sex differences.

The trade-off between current and future reproduction is not limited to "allocating between growth and maintenance, on the one hand, and current reproduction, on the other," as Hawkes seems to imply, because current growth and maintenance may enable greater parental investment later. Hawkes, of course, recognizes this, because where she outlines Charnov's theory of life-history evolution she says of a hypothetical mammalian female, "The later her maturity, the larger she will be, and so the more can go to offspring." In other words, by foregoing current reproduction—which may include delaying the expenditure of mating effort—and allocating resources instead to growth and maintenance, an individual may increase its own competitiveness and thereby the reproductive value of existing and/or future offspring (parenting effort). I know of no reason this individual cannot be either male or female. I understand, to be sure, that there is indeed a powerful theoretical basis for expecting differences between the sexes in the extent to which they possess adaptations (at least obligate ones?) for allocating mating and parenting effort, but I am unaware of any reason that, in principle, it is incorrect to analyze individual differences among males and females according to the trade-offs between mating and parenting effort.

The reason, perhaps, that Hawkes and I disagree about the validity of using the mating-parenting trade-off to analyze both within- and between-sex differences is that we have different perspectives on the nature or degree of sex differences. Hawkes believes that there are "pervasive differences in reproductive strategies between males and females" and that "each sex faces different trade-offs." She also states that the aim of her comment is "to emphasize the breadth of reasons from evolutionary ecology to expect differences in the reproductive