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Social versus reproductive success: The central theoretical problem of human sociobiology

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Children do not happen, not because children have become impossible, but principally because intelligence at the peak of intensity can no longer find any reason for their existence.
Oswald Spengler

In or about December 1910, human character changed.
Virginia Woolf

Abstract: The fundamental postulate of sociobiology is that individuals exploit favorable environments to increase their genetic representation in the next generation. The data on fertility differentials among contemporary humans are not consistent with this postulate. Given the importance of *Homo sapiens* as an animal species in the natural world today, these data constitute a particularly challenging and interesting problem for both human sociobiology and sociobiology as a whole.

The first part of this paper reviews the evidence showing an inverse relationship between reproductive fitness and "endowment" (i.e. wealth, success, and measured aptitudes) in contemporary, urbanized societies. It is shown that a positive relationship is observed only for those cohorts who bore their children during a unique period of rising fertility, 1935–1960, and that these cohorts are most often cited by sociobiologists as supporting the central postulate of sociobiology. Cohorts preceding and following these show the characteristic inverse relationship between endowment and fertility. The second section reviews the existing sociobiological models of this inverse relationship, namely, those of Barkow, Burley, and Irons, as well as more informal responses among sociobiologists to the persistent violation of sociobiology's central postulate, such as those of Alexander and Dawkins. The third section asks whether the goals of sociobiology, given the violation of its fundamental postulate by contemporary human societies, might not be better thought of as applied rather than descriptive, with respect to these societies. A proper answer to this question begins with the measurement of the pace and direction of natural selection within modern human populations, as compared to other sources of change. The vast preponderance of the shifts in human trait distributions, including the IQ distribution, appears to be due to environmental rather than genetic change. However, there remains the question of just how elastic these distributions are in the absence of reinforcing genetic change.

Keywords: adaptation; culture; eugenics; evolution; fertility; gene-culture coevolution; intelligence; IQ; nature/nurture controversy; sociobiology

1. Introduction

As a descriptive science of modern human populations, sociobiology confronts one particularly resistant piece of data: the inverse relationship between rank and reproductive fitness almost universally observed in these populations.[1] Sociobiology predicts that individuals will behave so as to increase their genetic representation in the next generation. Individuals, then, should exploit positions of power to increase their number of descendants relative to those below them. Rarely, however, do we observe such behavior in modern human populations. As Reed and Palm (1951, pp. 294–95) note, "The presence of an inverse correlation between the cultural and the biological contributions of the individuals of any particular generation has been demonstrated repeatedly." The one period and cohort for which we did observe a direct correlation coincided, unfortunately, with the explosion of quantitative research in the social and behavioral sciences after World War II and has caused a not inconsiderable confusion on this point.

Kant distinguished three passions – for possession, for power, and for honor – but pointedly not for offspring. That wealth, power, and rank are ceaselessly and obsessively striven for, as well as monitored in others, is obvious to all observers of human behavior, whether of the sociobiological persuasion or not. That modern humans exploit what they are able to obtain in the way of status and rank to produce, and to help relatives to produce, more offspring than those of lower rank and status seems clearly *not* to be the case. In fact, precisely the reverse appears to be true. The striving for, if not the

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actual possession of, status and power seems, on average, to deter rather than to stimulate reproductive effort among modern humans. This fact comes as close to being a universal regularity in contemporary societies as any other feature of such societies and has been pointed out on several occasions in the debate over sociobiology's relevance to the social sciences, but never insistently (Allen et al. 1975). My first purpose here is to introduce this fact frontally into the sociobiological literature.

My second purpose is to ask whether the tasks of human sociobiology, outside of its applications to anthropology and the study of premodern populations, do not go beyond the normal ones of descriptive science – because modern humans do not behave in accordance with its "central dogma," the maximization of reproductive fitness – to include those of applied science. Modern culture is therefore confronted by a possibly self-destructive condition caused by the violation of this "dogma" by the individuals living within it. The following question is posed. How long and under what conditions can a culture survive in which (1) a high relative frequency of certain traits is necessary to its survival, (2) these same traits are heritable across generations, and (3) those better endowed with these traits are demonstrating lower than average reproductive fitness? Many have expressed the fear (and some the hope) that human sociobiology is nothing but a reborn eugenics in disguise, and they will immediately recognize the question just posed as the central issue inspiring the now largely defunct field of eugenics. I shall argue that the interest of human sociobiology, as applied to contemporary societies, does, because of the syllogism just stated, overlap with that of eugenics, though this overlap is a logical one ("Eugenics," writes Collini [1984, p. 1436], is "regarded by some scholars as the only logical outcome of consistent Darwinism") and does not have its source in any hidden ideological agenda or class conspiracy, as has sometimes been suggested (see, for example, Lewontin, Rose & Kamin 1984).

This paper is divided into four parts. In the first, I review the published data showing an inverse relationship between rank and fitness in modern human populations. I also present some new data of my own in this section. In the second section, I review the existent sociobiological models of this phenomenon as well as more informal responses by sociobiologists to the prevalence of non-fitness-enhancing behaviors in modern societies. In the third section, I ask whether the goals of sociobiology, at least with respect to contemporary societies, might not be better thought of as applied than descriptive. A proper answer to this question lies in the measurement of the pace and direction of natural selection within modern human populations, as compared to other sources of change, the orders of magnitude of which I try to estimate. My conclusion summarizes the various arguments and themes of the paper.

2. Rank and fitness in contemporary societies

"To dominate," writes E. O. Wilson in his celebrated *Sociobiology*, "is to possess priority of access to the necessities of life and reproduction. This is not a circular definition; it is a statement of a strong correlation observed in nature. With rare exceptions, the aggressively superior animal displaces the subordinate from food, from mates, and from nest sites. It only remains to be established that this power actually raises the genetic fitness of the animals possessing it. On this point the evidence is completely clear" (Wilson 1975, p. 287). Wilson proceeds to show that, in a number of different species including *Homo sapiens*, dominance and status are strongly and positively correlated with reproductive success: The higher the rank, the greater the number of surviving offspring. In *Genes, Mind, and Culture*, Wilson and his colleague Charles Lumsden cite yet more evidence from human studies (see, in particular, the series of studies summarized in Neel, 1980) that the "'correct' choice of culturgens, leading to social and economic success in the opinion of the people employing these culturgens, results in more . . . mating and hence higher reproductive rates" (Lumsden & Wilson 1981a, p. 283). Lumsden and Wilson do acknowledge, however, as Wilson did not in *Sociobiology*, that this positive feedback from cultural to genetic success may only exist in "economically more primitive societies" (p. 283; see also Lumsden 1983, no. 109).[2] [See also *BBS* multiple book review of Lumsden & Wilson in *BBS* 5(1) 1982.]

Why is it so important to the sociobiological paradigm that status and rank, in a word, superior resources, confer upon their possessors greater reproductive success? There are two reasons. In the first place, the fundamental premise of sociobiology is that, in Betzig's words, "individuals have been naturally selected . . . to maximize 'inclusive fitness', essentially, to maximize their own reproduction and aid in the reproductive efforts of close genealogical kin" (Betzig 1982, p. 209). It follows from this premise that individuals will "exploit positions of strength, ultimately to the end of maximizing genetic representation in descendant generations" (p. 209).[3] Humans are observed to be obsessed with status and rank as with nothing else, after their basic subsistence needs have been met, and to strive for them ceaselessly within the limits of their various capacities and talents. That they could be so dominated by these goals and not translate whatever success they achieve with respect to them into enhanced fitness would mean, quite simply, that we are observing a direct violation of sociobiology's fundamental premise, that is, energy obsessively directed toward goals not associated with, indeed often negatively associated with, fitness. A systematic and widespread violation by humans of so central a premise of the sociobiological paradigm would signal to many the limitations of that paradigm for the social sciences, particularly sociology, which is the science of contemporary human societies.

The second reason that social and reproductive success must be positively correlated if sociobiology is to be successful as a general model of modern human populations is that this positive correlation is essential to one of the most ambitious attempts by sociobiologists to explain cultural evolution, which had heretofore remained outside the grasp of this paradigm and seemed to all to follow its own autonomous though unknown laws, namely, Lumdsen and Wilson's model of gene-culture coevolution (Lumsden 1983; Lumsden & Wilson 1981a; 1983). According to Lumsden and Wilson, individuals vary genetically in their predispositions, within a given environment, toward certain types of behavior and therefore in their capacities to thrive in the culture which has created that environment *and* to make that culture

thrive. As a consequence, genes and culture will coevolve, that is, reinforce eath other in a dynamic fashion, only if those who are more successful in a given culture leave more descendants than those who are less successful. On the other hand, if those who are less successful leave more descendants, as appears to be the case in contemporary society, then either the culture will cease to evolve in its accustomed direction, constrained as it would be by the failure of genetic evolution to accommodate it, or it will evolve in that direction independently of the parallel genetic evolution or lack thereof. In either case, gene-culture coevolution of the type Lumsden and Wilson consider central will have ceased, and their theory of gene-culture coevolution will be seen as irrelevant to the concerns of the social sciences, which are the sciences of cultural differences and historical sequences among modern human populations (K. Bock 1980). Let us now turn to the evidence for and against a positive relationship between cultural and reproductive success in modern human populations.

Wrong (1980) is perhaps the single best and most comprehensive work on historical fertility differentials in the West (see Wrong, 1958, for a shorter version). In this work, Wrong shows that the typical inverse relationship between economic and social class (i.e. resources, status, and rank), on the one hand, and birth rates, on the other, was a concomitant of the overall decline in Western birth rates; indeed, this inverse relationship may have predated that decline by one or two generations (Wrong 1980, p. 48). Before the demographic transition, on the other hand, reproductive fitness appears to have been positively related to social status, according to the few investigations of this question that I am aware of. According to Weiss (1980, pp. 147–48), for example, both differential fertility and differential mortality favored the upper strata in pretransitional rural Germany. Indeed, Stone (1977, p. 64) flatly states that "unlike today, the rich had more children than the poor" throughout pretransitional Europe, but his source (Shorter 1975, Appendix I) turns out to have nowhere near the generality needed to support such a broad claim. Nonetheless, what little evidence we have points to a direct relationship between fertility and endowment before fertility began to decline in Europe. The decline in birth rates began first in France, the United States, and Great Britain in the 18th and early 19th centuries and was present in most other Western countries by the end of the 19th century. An example of the kind of fertility differentials by socioeconomic class which emerged in the modern period is given in Table 1. Note that these data refer to surviving children rather than total number of offspring. The biological notion of reproductive fitness, of course, refers to the former rather than the latter. Unfortunately, apart from the data of Table 1, most of the available data are counts of total live births rather than children surviving to adulthood. In modern populations, however, variations in reproductive fitness do not seem to depart significantly from variations in fertility, as Cohen (1971) has shown for Bajema's data on IQ and fitness (Bajema 1963).

The decline in birth rates began at least a generation later in Japan than in the West (*circa* 1920, see Hashimoto 1974, p. 171; Saxonhouse 1974, p. 198). Prior to 1920, birth rates actually rose in Japan, and the available data suggest a direct relation between social success and

Table 1. *Surviving children per married couple, where wife's age exceeds 45 Years, classified by social status, 1911, England and Wales*

Social class	Surviving children per married couple
Professional and higher white collar	2.94
Lower white collar, commercial	3.38
Skilled manual	3.82
Semiskilled manual	3.79
Unskilled	3.88
Textiles	3.31
Coal mining	4.45
Agricultural laborers	4.57

Source: England and Wales, Registrar General, *Census of England and Wales: 1911*, Vol. 13, "Fertility of Marriage," Part 2 (London: HMSO, 1923), Table 24, pp. 19–20.

reproduction during this period (e.g. Hayami 1980). The decline that set in after 1920, however, although slow at first, seems to have been accompanied there too by lower than average birth rates among the economically and culturally successful, though published data on historical fertility differentials in Japan are rather scanty (see, however, Taeuber 1948, pp. 268–69). In Table 2 (following upon the work of Dudley Kirk, to be discussed below) I compare the fertility of ever-married men in the Japanese *Who's Who* of 1955 with that of ever-married women in Japan as a whole. The below-average reproduction of these men is clear.

It is apparent, however, from reading the works of human geneticists and sociobiologists, that the universality and constancy of this pattern is not appreciated in these disciplines (see, for example, Boyd & Richerson, 1981; Broadhurst, Fulker & Wilcock 1974; Daly & Wilson 1983, p. 333; Eckland 1967; Essock-Vitale 1984; Falconer 1981, p. 312; Gottesman 1968; I. Lerner 1968; Lewontin 1970; 1982, p. 171; McClearn 1970; Weinrich 1978a). The reason for this is that a substantial body of work appeared after World War II showing negligible fertility differentials by status and endowment in the postwar period. The conclusion drawn from this work was that fertility differentials are a temporary phenomenon of the demographic transition from a high-fertility, high-mortality regime to a low-fertility, low-mortality regime, that birth control spreads from the upper to the lower classes as this transition progresses, indeed, that this is the very way in which the transition, or at least the fertility component of the transition, proceeds. Once the transition is complete, fertility differentials by socioeconomic class disappear. Osborn (1940) went further and hypothesized that once the transition is over and birth control is available to all, those best fit for life in modern, industrial society will actually reproduce more than those less fit, a hypothesis strikingly similar to, but without the theoretical basis of, that of Lumsden and Wilson in their *Genes, Mind, and Culture*. Osborn called this hypothesis the "eugenic hypothesis" (Miller [1983, p. 1205] has recently revived this idea, apparently without being conscious of its antecedents) and, 30 years later, with Bajema (Osborn & Bajema 1972), concluded that the eugenic hypothesis had

Table 2. *Average number of children among ever-married men listed in Japanese "Who's Who," 1955, by age, with comparable data for Japanese ever-married women*

Ever-married men in "Who's Who"				Average no. of children for all ever-married Japanese women[b] (2)	Ratio of "Who's Who" to national birth rates (1)/(2)
Cohorts born	Age in 1955	No. of men	Average no. of children[a] (estimated s.e.) (1)		
Before 1896	over 60	638	3.23 (.11)	4.67	.69
1896–1900	55–59	419	3.25 (.09)	4.74	.68
1901–1905	50–54	509	3.21 (.07)	4.63	.69
1906–1910	45–49	394	2.78 (.08)	4.35	.64
1911–1915	40–44	156	2.47 (.10)	3.89	.63
1916–1920	35–39	41	2.10 (.19)	3.26	.64
After 1920	under 35	15	1.47 (.32)	1.75	.84
Total		2,172			

[a]Calculated from a sample of the first listings on each page describing an ever-married man. Derived from *Jinji Koshinroku* (Jinji Koshinroku Sha, Tokyo, Japan, 1956). Listings are short biographies of approximately 70,000 eminent individuals, including Diet members, cabinet ministers, city mayors, prefectural governors, high-level bureaucrats, executives in public enterprises, university administrators and professors, corporate executives, businessmen, publishers, journalists, lawyers, accountants, doctors, religious figures, novelists, painters, sculptors, musicians, actors, actresses, directors, dancers, entertainers, go and chess masters, athletes, philanthropists, union leaders, former courtiers, members of the royal family, etc.

Total births to the sample are 6,646, of which 3,446 are male, giving us a male sex ratio of .519. This is not significantly different from the overall Japanese sex ratio of .514 (Kaku 1972), indicating that there has been no undercount of female children in these data. Underreporting of female children is common in the available data on *samurai* fertility in the Tokugawa period (Yamamura 1974, p. 103) and might be expected in these data as well.

[b]Unweighted average of 1950 and 1960 birth rates. Derived from "Population Census of 1950, Volume 3, Results of Ten Percent Sample Tabulation, Part 1, Sex, Age, Marital Status, Citizenship, Education, Household, Housing, Fertility," Table 28, p. 169 (Bureau of Statistics, Office of the Prime Minister, 1952); "1960 Population Census of Japan, Volume 2, One Percent Sample Tabulation, Part 1, Age, Marital Status, Legal Nationality, Education and Fertility," Table 10, p. 362 (Bureau of Statistics, Office of the Prime Minister).

survived the tests of numerous postwar studies, which showed a positive relationship between IQ and fertility and much diminished, if not negligible, differentials by economic and social class.

Dudley Kirk, in a curiously neglected study of men in the 1956–57 *Who's Who*, was the first, to my knowledge, to actually document and emphasize the change in the pattern of fertility differentials after the war (Kirk 1957). His paper, in the main, reconfirmed the below-average fertility of the nation's most successful and esteemed men, a rate also found for earlier cohorts of such men by Huntington and Whitney (1927). But for the younger cohorts Kirk found higher than average fertility. I have extended his study to the 1980–81 *Who's Who* (see Table 3, Figure 1). Male cohorts in *Who's Who* born between 1905 and 1930 had fertility rates exceeding that of all white women (in Kirk's study, the reference group is actually all *native* white women) in the U.S. as a whole, a remarkable reversal of previous patterns going back many decades and cohorts into the past. Sly and Richards (1972) document a similarly above-average fertility in these same cohorts for a sample of men listed in both *Who's Who* and 1 of 11 cities' *Social Registers*.

The relationship between intelligence and reproductive fitness probably has an interest for sociobiologists equal to that for the relationship between rank and reproductive fitness. The human species is distinguished from other species, including the other primates, by a very large capacity for abstract thought and complex reasoning. Those individuals having large mental capacity were and are more likely to succeed in a given culture, to grasp its requirements, and to understand and adapt to the direction of its evolution. The emergence of *Homo sapiens* was marked by the rapid growth of the forebrain, the seat of the species' superior reasoning powers. To accomplish this growth, hominid culture must have promoted a strongly positive relationship between reproductive fitness and intelligence (Neel 1980). That is to say, among our fore-bears, those with larger forebrains, and therefore greater intelligence, must have left on a system-

Table 3. *Average number of children among men listed in U.S. "Who's Who," 1956–57 and 1980–81, by age, with comparable data for U.S. white women*

	Men in "Who's Who"						
	No. of men		Average no. of children per man		Cumulative birth rates to white women	Ratio of "Who's Who" to national birth rates	
Cohorts born	1956–57	1980–81	1956–57[a]	1980–81		1956–57	1980–81
1875–79	122		2.23		3.50	.64	
1880–84	166		1.86		3.31	.56	
1885–89	326		2.13		3.12	.68	
1890–94	530		1.94		2.94	.66	
1895–99	602		1.80		2.74	.66	
1900–04	586	131	1.82	2.01	2.48	.74	.81
1905–09	478	248	(2.10)	2.30	2.27	.93	1.01
1910–14	251	467	(2.43)	2.42	2.28	1.07	1.06
1915–19	117	544	(2.69)	2.55	2.48	1.08	1.03
1920–24		655		2.73	2.75		0.99
1925–29		624		2.95	2.95		1.00
1930–34		435		2.57	3.10		0.83
1935–39		275		2.30	2.92		0.79

[a]Figures in parentheses are estimates of future completed fertility as of 1955, by Kirk.
Source: Kirk (1957, p. 87); Vining (1982a, p. 257).

atic basis over many generations more offspring than those with smaller forebrains and less reasoning power (Davis 1976; Van Valen 1974).

Among modern peoples, the favored measure of intelligence is IQ. Though this measure clearly does not adequately capture the entire spectrum of mental abilities classified under the general rubric of intelligence, it does measure, as Anne Anastasi puts it, "a cluster of intellectual traits demanded in modern, technologically advanced societies. . . . This trait cluster is both developed by formal schooling and required for progress within the academic system. Hence it can be appropriately designated 'scholastic aptitude' or 'academic intelligence'" (Anastasi 1963, p. 182). That is to say, IQ measures a particular component of ingelligence ("scholastic aptitude") which is of particular, perhaps even unique, importance to modern, technologically advanced societies.[4] [See also Sternberg: "Toward a Triarchic The-

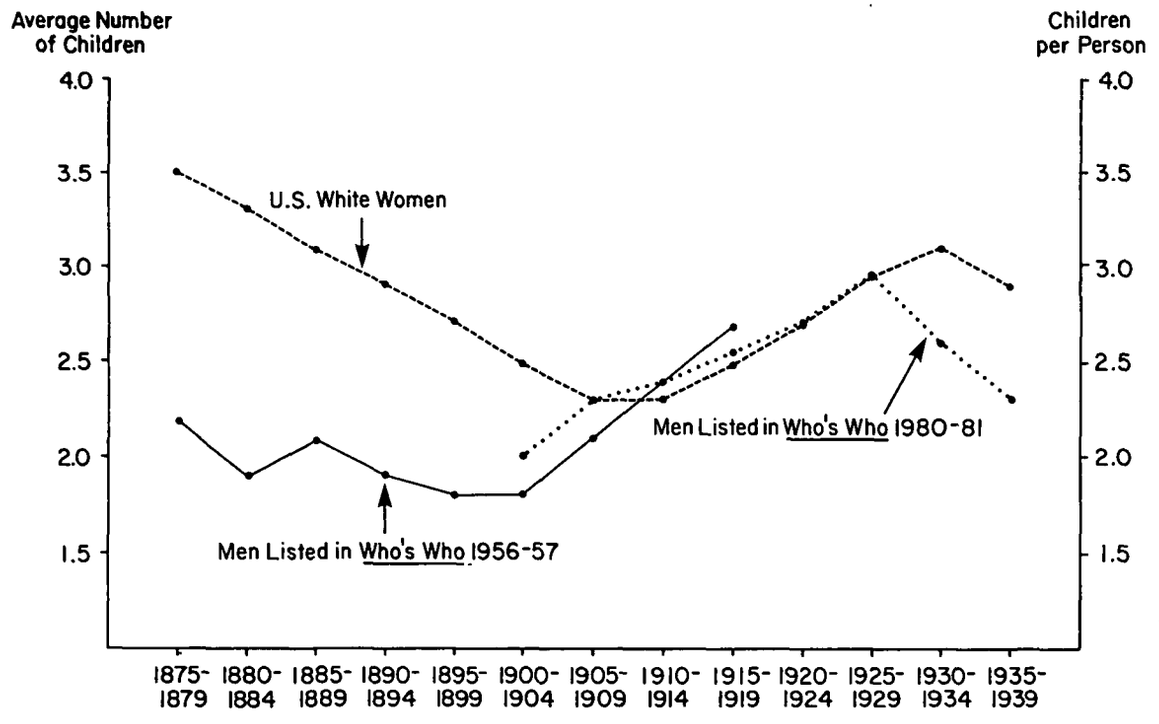

Figure 1. Average number of children reported by men in *Who's Who*, by year of birth, compared with completed fertility of United States white women of the same age; see Table 3.

ory of Human Intelligence" *BBS* 7(2) 1984 and Jensen: "The Nature of the Black-White Difference on Various Psychometric Tests" *BBS* 8(2) 1985.]

The trait measured by IQ has high heritability among those human behavioral traits for which heritabilities have been estimated (Bouchard, 1982, pp. 77–78; Cattell 1981; Henderson 1982 [who cites somewhat lower heritabilities for cognitive than for personality traits]; Schilcher & Tennant 1984, p. 52; as these last authors note [pp. 52–53; see also Hartung 1984, p. 516], high heritability implies that the trait had low adaptive significance in the past and therefore that IQ may not be measuring the kind of intelligence that was presumably so strongly selected for among our prehistoric forebears – for some ways out of this impasse, insofar as it is one, see Schilcher & Tennant 1984, pp. 52–53). Thus, a particularly simple and direct test of the gene-culture evolution model as a description of contemporary human, particularly Western, populations is to ask whether those with greater intelligence, of the type required to accommodate the evident direction of cultural evolution, that is, toward a greater and greater reliance on complex technological control of the environment, have higher reproductive fitnesses than those with less intelligence. The consistent finding of those who have performed this test is that the relationship is in the predicted direction (see Bajema 1963; Higgins, Reed & Reed 1962; Olneck & Wolfe 1980; Spuhler 1962; Waller 1971; for reports on unpublished work, see Anastasi 1956; 1959; I. Lerner 1968). This work has been widely cited by sociobiologists and others interested in whether cultural and biological evolution reinforce each other in contemporary societies (see, again, Boyd & Richerson 1981; Broadhurst et al. 1974; Daly & Wilson 1983, p. 333; Eckland 1967; Essock-Vitale 1984; Falconer 1981, p. 312; Gottesman 1968; I. Lerner 1968; Lewontin 1970; 1982, p. 171; McClearn 1970; Weinrich 1978a). The late Frederick Osborn, a leading figure in and benefactor of the postwar American eugenics movement, was particularly vigorous in using this work to suggest that the cultural environment created by the modern human promotes biological evolution (i.e. changes in gene frequencies) in the direction required for the maintenance of that environment and consonant with its direction of change and therefore that direct state intervention to promote such fertility patterns is unnecessary (Osborn 1968; 1973; Osborn & Bajema 1972).

The many papers demonstrating a slight but significant positive relationship between intelligence, as measured by IQ, and fertility parallel the studies of men in *Who's*

Table 4. *Average number of children among 400 wealthiest persons in U.S. ("Forbes 400") by age, with comparable data for U.S. women*

	Persons in "Forbes 400"				
Cohorts born	Age in 1980	No. of Persons[a]	Average no. of children[b] (1)	Cumulative birth rates to white women as of Jan. 1, 1981[c] (2)	Ratio of "Forbes 400" to national birth rates (1)/(2)
before 1901	80 and over	25	2.52		
1901–1905	75–79	26	2.27		
1906–1910	70–74	45	2.53	2.25	1.12
1911–1915	65–69	50	3.12	2.31	1.35
1916–1920	60–64	54	3.24	2.53	1.28
1921–1925	55–59	50	3.22	2.79	1.15
1926–1930	50–54	45	3.56	2.97	1.20
1931–1935	45–49	34	2.89	3.10	0.93
1936–1940	40–44	30	3.13	2.85	1.10
after 1940	under 40	27	1.41		
Total and averages		386	2.90	2.69[d]	1.16[d]

[a]Men and women combined.

[b]Derived from biographical sketches given in "The *Forbes* Four Hundred," *Forbes*, Vol. 130, No. 6 (Sept. 13, 1982), pp. 99–186. All children are counted, including those deceased. Persons for whom biographical information is not given or for whom only surviving children are mentioned are excluded. Persons for whom number of children is not listed are counted as having no children. Males and females are combined. Husbands and wives are counted only once.

[c]*Source*: For cohorts born between 1906 and 1930, "Fertility Tables for Birth Cohorts by Color, 1917–1973," DHEW Publication No. (HRA) 76-1152, United States Department of Health, Education and Welfare, Public Health Service, Health Resource Administration, National Center for Health Statistics, Rockville, Maryland, April 1976, pp. 131–33. For cohorts born between 1931 and 1940, "Vital Statistics of the United States 1980, Vol. I-Natality," National Center for Health Statistics, U.S. Department of Health and Human Services, pp. 1–34.

[d]Unweighted average.

Table 5. *Average number of children reported by Terman's high-IQ sample, 1972*

Cohorts born	No. of men	Average no. of children (1)	No. of women	Average no. of children (2)	Birth rates to all white women (3)	Ratio of birth rates of Terman sample to national birth rates (1)/(3)	(2)/(3)
1900–04	42	1.81	12	2.00	2.48	.74	.82
1905–09	147	2.18	124	1.55	2.27	.96	.68
1910–14	215	2.52	207	1.78	2.28	1.11	.78
1915–19	37	2.57	45	2.51	2.48	1.04	1.01
Total	441	2.27[a]	388	1.96[a]	2.37[a]	.96	.83

[a]Unweighted average.
Source: Terman, Sears, Cronbach, and Sears (1983); Table 3 above.

Who by Kirk (1957) and in *Who's Who* and the *Social Register* by Sly and Richards (1972). As yet another example of the tendency of the better endowed to have higher fitness (as indexed by fertility) than average, I compare in Table 4 the fertility of the 400-odd wealthiest persons in the U.S. with that of their equivalent age cohorts in the nation as a whole. Essock-Vitale (1984), incidentally, provides a much more exhaustive and rigorous analysis of these data. In particular, she corrects for under-reporting of number of children by *Forbes* (by cross-checking their biographies with those of *Who's Who*) and finds an even higher average number of children of 3.1 for the *Forbes* population, as compared to my own finding of 2.9. By either analysis, however, it is clear that there is higher than average fitness in this manifestly highly endowed group. Of course, this population represents the very extreme end of the wealth distribution and its reproductive behavior may not be typical of the upper tail of that distribution in general. In Table 5, I compare the fertility of Terman's high-IQ group, a sample of persons with IQs $\geq \bar{X} + 2.5$ SD in California, with that of the equivalent cohorts comprising all white women in the nation. Though the pattern is less clear here, inferior fitness certainly is not observed for the younger cohorts of this highly able group, particularly for the males. Thus, the available quantitative evidence would appear to show higher fitness among the nation's economic, political, social, and technocratic elite (Kirk 1957; Sly & Richards 1972; Table 3) and its wealthiest citizens (Table 4), as well as a generally positive or at least nonnegative relationship between IQ ("academic intelligence") and fertility (see the references above and Table 5). In all respects, it would seem, those with a higher than average measure of traits necessary to succeed in modern culture – as indexed by either the actual social and economic positions of the possessors of these traits or by a direct measure of one trait, intelligence, thought to be a necessary condition for such success – have higher than average reproductive fitnesses. The gene-culture coevolution model seems confirmed for contemporary, economically advanced society as it has generally been for premodern, economically primitive societies (Chagnon 1980; Daly & Wilson 1983, pp. 332–333; Hill 1984b; Irons 1979; Neel 1980; Ruse 1982; Beall and Goldstein, 1981, p. 10, however, present some suggestive disconfirmatory evidence for a premodern Tibetan population, the only such instance that I am aware of). The contradictory evidence found in the prewar data on fertility differentials by socioeconomic class was apparently only a transitory phase of modern culture whereby fertility rates dropped most rapidly among the elite groups. With the end of this transition to a regime of low fertility, the usual pattern of higher reproductive fitness among those more fit, in the common-language sense of that word, reasserted itself.[5]

The flaw in this interpretation of the postwar data is that it fails to take into account the fact that fertility had not just ceased to fall when the cohorts studied in the postwar period entered their child-bearing years but actually had begun to rise. The years 1935–1960 were characterized by rising birth rates, both in the U.S. and in Europe (Kiser 1959; Westoff 1974). That is to say, the many studies demonstrating a positive relationship between IQ and fertility and insignificant differentials by socioeconomic class were of cohorts whose fertility was unique in the modern period: They exhibited significantly higher fertility than their predecessor cohorts rather than the more usual pattern of lower fertility. There is reason to believe that the pattern of fertility differentials in these cohorts, born between 1905 and 1930, was also unique, that while upper-class fertility is lower than average in periods of falling or permanently low overall fertility, it converges toward, or may even come to equal or exceed, the average during periods of rising overall fertility (O'Connell 1981, pp. 11–12).

To test this hypothesis, I have recently studied the relationship between IQ and fertility in a respresentative sample of the immediate *postwar* birth cohorts – those born in the period 1942–1953. This cohort, unlike that of their parents, is reproducing in the post-1960 period of falling and low birth rates. Fertility completed to date within this cohort shows a clear inverse relationship with IQ (Table 6). As of 1978, however, this cohort had not yet reached the end of its child-bearing years. One estimate of completed fertility for a still fertile cohort is expected lifetime fertility (as reported by the respondents in the survey), which also shows a negative, though more moderate, relationship with IQ (Table 7). At the same time, expected lifetime fertility is apt to overestimate the actual lifetime fertility of the more intelligent and underestimate that of the less intelligent, because women with

Table 6. *Mean cumulative fertility rate according to measured intelligence of parent generation, probability sample of U.S. population, aged 25–34, 1976 (white men) and 1978 (women)*

	IQ range						
Subgroup	≤70	71–85	86–100	101–15	116–30	>130	All
White women	1.59	1.68	1.76	1.44	1.15	.92	1.46
(Estimated s.e)	(.32)	(.11)	(.06)	(.04)	(.06)	(.17)	(.03)
(Sample size)	(17)	(122)	(522)	(907)	(438)	(60)	(2,066)
Black women	2.60	2.12	1.79	1.63	1.20	.00	1.94
(Estimated s.e.)	(.22)	(.13)	(.11)	(.14)	(.53)	(—)	(.07)
(Sample size)	(50)	(165)	(159)	(88)	(10)	(1)	(473)
White men	1.17	1.30	1.29	1.19	.84	.45	1.14
(Estimated s.e.)	(.25)	(.11)	(.05)	(.04)	(.06)	(.11)	(.03)
(Sample size)	(24)	(142)	(565)	(825)	(377)	(60)	(1,993)

Note: Data on black male fertility unreliable and therfore not reported here.
Source: Vining (1982a, p. 247).

below-average fertility (in this case, high-IQ women) tend to overestimate their future fertility whereas women with above-average fertility (in this case, low-IQ women) tend to underestimate their future fertility (Freedman, Freedman & Thornton 1980). One would also expect that the less intelligent would have more children than they expect because of a greater failure rate in contraception (Cliquet & Balcaen 1979; Udry 1978), whereas the more intelligent would have fewer children than they expect due to lower fecundity in the later child-bearing years as well as to the unexpected demands of outside-the-home careers in which the more intelligent would be disproportionately represented (Bajema 1978).

Hence, the ultimate slope of the relationship between IQ and fertility for this cohort will probably fall somewhere in between that found for IQ and fertility to date and that found for IQ and expected lifetime fertility. The relationship is, in any event, unambiguously negative, for both black and white females. The selection differential (i.e. the change in mean IQ if heritability is equal to 1 and there is no regression to the mean, see Falconer 1966; Plomin, DeFries & McClearn 1980, pp. 260–61) for whites is −2 IQ points (SD = 16) if differentials in fertility to date by IQ are used in its calculation, and −0.7 IQ points if differentials in expected lifetime fertility are used. These selection differentials are rather larger, in absolute value, than those calculated from Bajema's data (Cohen 1971; Falconer 1966; Van Valen 1974). Note again that I ignore here the other variables which contribute to relative fitness, that is, mortality and generation length differentials (Bajema 1963). However, as Cohen (1971) has shown with Bajema's data, the differentials in fertility in these data account for almost all of the differentials in fitness.

A second set of observations consistent with those just cited comes from the American Mensa, an organization for persons with IQs $\geq \bar{X} + 2$ SD. The fertility of Mensa members is probably not typical of all high-IQ persons, since Mensa tends to attract a disproportionate number of unmarried and divorced persons without family responsibilities (Vining 1984). However, there is no reason to believe that the variation in fertility *across* cohorts within Mensa is not typical of the variation in fertility across cohorts within the high-IQ population as a whole. And this cross-cohort variation in Mensa relative to cross-cohort variation in the nation as a whole confirms my conjecture that the absence of the inverse relationship between IQ and fertility found in many postwar studies is an artifact of their confining their attention to cohorts reproducing in a unique period of rising fertility. Figure 2 and Table 8 show cumulative fertility by age in the Mensa membership and among white women nationwide. Note

Table 7. *Mean expected lifetime fertility, according to measured intelligence of parent generation, probability sample of U.S. women, aged 25–34, 1978*

	IQ range						
Subgroup	≤70	71–85	86–100	101–15	116–30	>130	All
White women	2.31	2.16	2.30	2.14	2.03	1.93	2.15
(Estimated s.e.)	(.38)	(.11)	(.06)	(.04)	(.06)	(.18)	(.03)
(Sample size)	(16)	(122)	(517)	(893)	(432)	(59)	(2,039)
Black women	3.20	2.75	2.36	2.25	2.30	2.00	2.56
(Estimated s.e.)	(.26)	(.13)	(.11)	(.15)	(.47)	(—)	(.07)
(Sample size)	(50)	(161)	(155)	(88)	(10)	(1)	(465)

Note: Data on expected lifetime fertility unavailable for men.
Source: Vining (1982a, p. 252).

Table 8. *Average number of children among members of American Mensa, 1980, by age, with comparable data for U.S. white women*

		Mensa members				Cumulative birth rates to white women as of Jan. 1, 1981[a]	Ratio of Mensa to national birth rates	
Cohorts born	Age in 1980	No. of women	Average no. of children (1)	No. of men	Average no. of children (2)	(3)	(1)/(3)	(2)/(3)
1906–1910	70–74	99	1.53	250	1.71	2.25	.68	.76
1911–1915	65–69	202	1.63	550	2.18	2.31	.71	.94
1916–1920	60–64	364	1.91	1,081	2.43	2.53	.75	.96
1921–1925	55–59	655	2.31	1,569	2.57	2.79	.83	.92
1926–1930	50–54	1,033	2.28	2,261	2.49	2.97	.77	.84
1931–1935	45–49	1,132	2.31	2,430	2.18	3.10	.75	.70
1936–1940	40–44	1,422	1.79	2,999	1.80	2.85	.63	.63
1941–1945	35–39	1,963	1.20	4,149	1.22	2.33	.52	.52
Total		6,870		15,289				

[a]*Source*: Vining (1984, p. 727).

the convergence in the fertility rates of the 1905–1930 birth cohorts in Mensa to those of the equivalent national (white) cohorts and then the divergence for the post-1930 cohorts. Note also that Mensa cohort fertility rises earlier than that of the nation and also falls earlier, suggesting that the fertility of high-IQ persons is a leading indicator of national trends. But this is a minor theme in what is the important regularity here, namely, the tendency of high-IQ persons to raise their fertility up to or near the national level during a period of rising birth rates and to lower their fertility to levels well below the national levels during periods of falling fertility. The overall level of Mensa fertility may not be an unbiased estimator of that of the population of which it is a sample, but the fluctuations in this level from year to year relative to that of the nation probably accurately reflect similar converging and diverging tendencies in the equivalent cohorts of the high-IQ population as a whole, though possibly at different overall levels.

In Tables 9 and 10, I show fertility differentials by years of education for a slightly older cohort than the one studied in my IQ/fertility study (Vining 1982a). Nonethe-

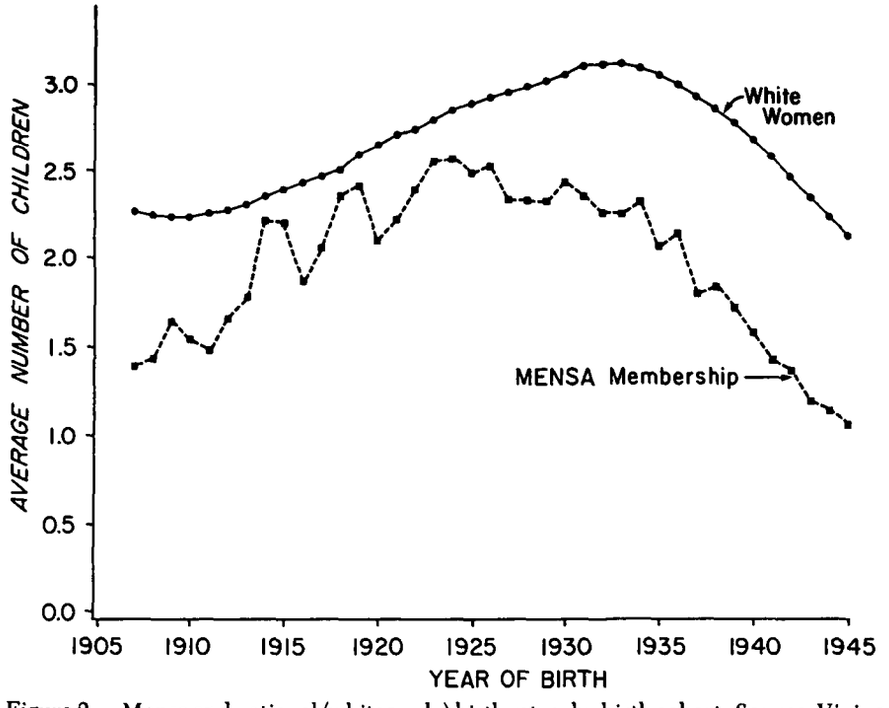

Figure 2. Mensa and national (whites only) birth rates, by birth cohort. *Source:* Vining (1984, p. 727).

Table 9. *Children ever born by years of school completed, probability sample of black and white U.S. women aged 35–44, June 1980*

Years of school completed		Whites		Blacks	
		No. of women	Children ever born per woman	No. of women	Children ever born per woman
Elementary:	0 to 7 years	505	3.44	167	4.00
	8 years	365	3.16		
High school:	1 to 3 years	1,458	3.08	422	4.38
	4 years	5,271	2.63	561	3.29
College:	1 to 3 years	1,895	2.47		
	4 years	1,127	2.16	353	2.37
	5 or more	718	1.64		
All women:		11,340	2.60	1,501	3.47

Source: U.S. Bureau of the Census, Current Population Reports, Series P-20, No. 375, *Fertility of American Women: June 1980*, U.S. Government Printing Office, Washington D.C., 1982, pp. 49 and 51.

less, this is a cohort which bore the majority of its children after the fall in birth rates had begun (*circa* 1960), and it shows the predicted pattern – a strong inverse relationship between educational level and fertility – and corroborates my own IQ/fertility study. It most emphatically contradicts Coale's prediction in 1964 that "there will be a positive association between socioeconomic class and natality within another decade or so" (Coale 1965, p. 58; see also the somewhat more cautious prediction by Carter 1962, p. 149), if we can use, as we surely can, years of schooling as an indicator of socioeconomic class. *Who's Who* unfortunately does not yet include sufficient numbers of the post-1935 birth cohorts to reveal whether a similar kind of "reversal of a reversal" in the relationship between social esteem and fertility has taken place. An interesting further test of the hypothesis put forward here will be whether men in *Who's Who* born after 1935 exhibit a lower than average fertility, in contrast to the prewar cohort.

Van Court and Bean (1985) have presented yet another demonstration of the tendency of fertility differentials by IQ class to vary with the cohort chosen for study. Table 11, an adaptation of Table 2 in their paper, gives the coefficient of correlation between IQ and fertility for a nationally representative sample of white Americans broken down into 5-year birth cohorts. The data are drawn from the 1974, 1976, 1978, and 1982 General Social Surveys of the National Opinion Research Center in Chicago. The measure of intelligence employed in these surveys was a steeply graded national vocabulary test. Vocabulary tests are said to perform quite well as measures of general intelligence (see, for example, Table 3 on p. 431 in Jensen and Reynolds, 1982). The pattern of correlation coefficients shown in Table 11 is consistent with the thesis being put forward in this section, namely that cohorts with rising fertility rates (i.e. cohorts 4–8) tend to show a less negative relationship between intelligence and fertility (average $r = -0.07$) than cohorts with

Table 10. *Children ever born by years of school completed, probability sample of U.S. women aged 35–44, June 1980*

Years of school completed		No. of women	Children ever born per woman
Elementary:	0 to 7 years	628	3.55
	8 years	447	3.27
High school:	1 to 3 years	1,915	3.36
	4 years	5,925	2.69
College:	1 to 3 years	2,139	2.48
	4 years	1,278	2.14
	5 or more	794	1.66
All women:		13,127	2.70

Source: U.S. Bureau of the Census, Current Population Reports, Series P-20, No. 375, *Fertility of American Women: June 1980*, U.S. Government Printing Office, Washington, D.C., 1982, p. 47.

Table 11. *Zero-order correlations between vocabulary test score and number of offspring for whites, by cohort*

Cohort	Date of birth	r	N	Mean Score	Mean Children
1	1895–1899	−.17*	120	5.6	2.6
2	1900–1904	−.23***	195	5.8	2.5
3	1905–1909	−.17***	273	5.6	2.4
4	1910–1914	−.08	307	6.3	2.3
5	1915–1919	−.13**	363	6.4	2.6
6	1920–1924	−.12**	424	6.2	2.8
7	1925–1929	.00	364	6.4	2.9
8	1930–1934	−.03	358	6.4	3.2
9	1935–1939	−.16***	429	6.4	2.8

Note: All significance levels are for one-tailed tests. *significant at $p \leq .05$. **significant at $p \leq .01$. ***significant at $p \leq .001$.
Source: Van Court and Bean (1985).

falling fertility rates (i.e. cohorts 1–3 and 9, average $r = -0.18$). Van Court and Bean's data show, in general, a strong negative relationship between fertility and intelligence but also show, for cohorts 4–8, a reduction in the strength of this relationship. It is plausible that for a different sample this inverted-U relationship between correlation coefficient and cohort birth year would be shifted upward so that its peak would be above zero, as was found in the more limited data of such investigators as Bajema, the Reeds, and Waller.

Several other investigators (among them Cliquet & Balcaen 1979; Retherford & Sewell 1985; Udry 1978; Yogev & Vierra 1983: for a brief sociobiological interpretation of the last, see Draper & Harpending 1982) have also found either an inverse relationship between IQ and fertility or a lower than average fertility among the ostensibly very intelligent (e.g. women university faculty) for the cohorts reproducing during the time of fertility decline following the well-known "baby boom" of the early postwar period in the U.S. and Europe. Retherford and Sewell's (1985) analysis is particularly interesting because it is of a random sample of approximately 10,000 seniors from public and private high schools in Wisconsin who graduated in 1957. Eleventh-grade IQ scores and number of children as of 1975 are available and have a negative relationship. This study can be viewed as a kind of replication of Bajema's, Waller's, Olneck and Wolfe's, and Higgins, Reed, and Reed's studies of samples of persons in the neighboring and ethnographically similar states of Michigan and Minnesota but born 20–30 years earlier.

So far, I have contrasted the pattern of fertility differentials in periods of rising and falling fertility. But we now seem to be entering a period of permanently low fertility (in many Western countries below-replacement) with only minor waves of rising and falling fertility (though we cannot wholly discount the possibility of a major rise in fertility in the future to well above replacement levels, as demographic forecasts are notoriously unreliable, see Keyfitz 1982). This is unknown territory, a period of permanently low fertility not having been experienced before. But one might expect that periods of permanently low fertility will exhibit a pattern similar to that of falling fertility, that in such periods as well the more intelligent will be more capable of and more inclined toward fertility suppression (Hardin 1972), and that the inverse relationship between endowment and reproduction observed for periods of falling fertility will thus extend into a period of permanently low fertility.

In short, until evidence is presented to the contrary, I think we can take it as one of the universals characterizing modern culture that social and reproductive success are inversely related. This pattern was apparently broken or certainly attenuated during a unique period of rising fertility in the middle of this century, which coincided with an (unfortunately unrepresentative) explosion of empirical research on this question.[6] As Easterlin (1980a, p. 227) has candidly admitted, demographers understand very little about this period. Nor, it must be granted, do demographers understand why upper-class fertility rates converged toward those of the nation during this period. Here it is enough to know that it was exceptional. In general, then, insofar as the traits possessed by those of higher status and higher intelligence are heritable, gene-culture evolution is divergent rather than self-reinforcing in the modern industrial and postindustrial culture. There is no evidence of which I am aware demonstrating the absence of this inverse relationship in any modern human breeding population with their characteristically low or falling fertility, with the possible exceptions of the Japanese today[7] (Vining 1982b) (though not a generation ago in a period of only moderate fertility decline, see above) and certain quasi-isolated religious groups in the West, such as the Mormons (the only modern population, to my knowledge, in which a nonnegative relationship between family size and IQ, as well as socio-economic status, has been documented, see Galbraith 1982, p. 169; Heaton 1984; Zajonc 1983, pp. 467–470). I invite commentators and readers to present counterevidence to the thesis presented here that reproductive and social success are negatively correlated in modern societies except possibly in the rare periods of rising fertility; but in the absence of such evidence so far I will proceed in the next section to review the various explanations to be found in the sociobiological literature of the classic inverse relationship between reproductive fitness and social success in the modern setting.

3. The sociobiology of non-fitness-maximizing behavior

"What sociobiologists deny," write Richerson and Boyd, "is that culture is under the influence of a mechanism that can cause cultural behaviors to depart significantly and *systematically* from those that would maximize genetic fitness" (Richerson & Boyd 1981, p. 240). As a description of intellectual history, this statement is, strictly speaking, untrue. Sociobiologists have not infrequently excluded the populations of modern societies from their general prediction that, in Daly and Wilson's words, "reproductive success is likely to be positively correlated with whatever variable is considered to be a measure of success within the culture under study" (Daly & Wilson 1978, p. 290). That is, nearly all sociobiologists *do* concede that in modern culture, cultural behaviors *do* "depart significantly and systematically from those that would maximize genetic fitness." What this systematic violation of sociobiology's central premise implies for the revelance of sociobiology as a science of modern humans is less clear from their writings. I shall take up the latter issue in my next section. Here I wish to review some of the models of the prevalence of non-fitness-maximizing behaviors in modern (that is, in sociobiologists' own) culture, as well as some of the more informal responses in the sociobiological literature to these behaviors.

Sociobiologists in fact confront two contradictions to their theory in the modern world, one at the group level and one at the individual level. In the first place, the wealthier the culture, the lower the overall fertility rate (and the rate of natural increase) of that culture (see, for example, Population Reference Bureau 1983). In the second place, *within* the wealthier cultures at least, those of higher status under-reproduce relative to those of lower status – that is, the better endowed do not translate their superior status into superior relative fitness within these cultures. Sociobiologists who have taken an interest in the problem posed to their discipline by these behav-

iors have for the most part taken both the cross- and within-culture relationships as manifestations of one and the same mechanism at work. To avoid confusion, I shall concentrate here on their treatment of the *within*-culture relationship between reproductive fitness and status.

For an explanation of this relationship, sociobiologists appeal to what might be most appropriately called the "novel environments" hypothesis. "Human nature," write Daly and Wilson (1978, p. 287) in their textbook on sociobiology, "may be adapted to maximize reproduction in the circumstances under which it evolved, but we no longer live in those circumstances." Of course, to say that the modern environment is not the same as that under which human nature originally evolved and therefore may promote non-fitness-maximizing behaviors does little more than to push the mystery of non-fitness-maximizing behavior around. The scientific question is why the release from premodern circumstances causes non-fitness-maximizing behavior. What is the precise agent in the modern environment which causes its better-endowed inhabitants to suppress reproduction relative to those less endowed, to violate in so fundamental a way the central behavioral premise of the Darwinian theory?

The most exhaustive and imaginative attack on this problem is that of Barkow and Burley (1980) and Burley (1979). Barkow and Burley begin by restating the common argument that intelligence conferred tremendous selective advantages upon its possessors. In no other way are we able to explain the explosive growth of the hominid brain over the past million years (Godfrey & Jacobs 1981). "No organ in the history of life has grown faster" (E. O. Wilson 1978, p. 87). At the same time, Barkow and Burley argue, this enhancement of intelligence increased the female's appreciation and foresight with respect to the dangers, pains, and inconveniences of childbirth and child-rearing and thereby her will as well as her ability to control her fertility, thus threatening her fitness (i.e. genetic representation in the next generation). As a consequence, according to Burley and Barkow's model, other traits, both cultural and innate, evolved to counter the one great selective *dis*advantage of intelligence, namely, that it would cause its possessors, particularly females, to under-reproduce. Examples of such traits are concealed ovulation (unqiue to the human), continuous sexual receptivity and strong sexual desire ("human beings are unique among the primates in the intensity and variety of their sexual activity" [E. O. Wilson 1978, p. 140]), male dominance (a trait having both physiological and cultural elements), and pro-natalist dogmas and ideologies universally found in premodern societies. "With the growth of intelligence," write Barkow and Burley, "early hominid females eventually understood the relationship between ovulation, copulation, and fertilization. They used this new knowledge to control their fertility, reducing it to the point of eliminating their genes from the gene-pool. Since intelligence itself was of high adaptive value, selection reduced not female intelligence but awareness of ovulation" (Barkow & Burley 1980, p. 172). The universality of pronatalist dogmas likewise suggests a certain hesitation on the part of the females to bear children. "Why should so many societies both pressure and reward women for childbearing, if women were not reluctant to have children?" Barkow and Burley (1980, p. 174) ask rhetorically. According to their model, then, a whole complex of traits, which they describe, evolved to prevent women from using their intelligence to suppress their reproduction.

Modern culture, however, typically provides women with both the autonomy (e.g. freedom from male dominance, equal opportunity for employment in the money economy) and the means (e.g. efficient, safe, and convenient methods of contraception) to thwart the various devices which had evolved, in turn, to thwart the human female's predisposition to under-reproduce. More important for the discussion here, the greater the modern female's intelligence and social status, the greater her access to situations in the modern economy which allow her the autonomy she requires to be able to suppress reproduction as well as the greater her ability to foresee the pain and inconvenience of childbirth and child-rearing and hence the greater her will to use the contraceptive devices which the modern economy provides her to avoid reproduction. Thus freed from its procreative function, sex can be enjoyed as untrammeled pleasure, by the intelligent most of all since it is they who are most efficient in contraception (although it is interesting that Kinsey reported the more educated to be less sexually active as well as less procreative, at least in the American version of modern culture; see Haldane, 1956, and the more recent data in Tanfer and Horn, 1985).

In short, despite its adaptive value for humans in the past, intelligence leads in modern culture to maladaptive behavior by the inhabitants of that culture. Moreover, *within* the modern culture, Barkow and Burley's model predicts precisely the inverse relationship between fertility and IQ observed today in the U.S. as well as the fertility differentials by economic and social class in both the U.S. and Europe observed since the demographic transition began, with the already noted exception of a 20- to 30-year period of rising birth rates in the middle of the 20th century. In fact, it may not be so much modern culture as "high" culture which creates the conditions under which high-status females suppress their reproduction relative to low-status women. Hopkins (1983), for example, provides a particularly detailed account of the below-replacement-level fertility which prevailed among upper-class Romans in the late Republic and the Principate.

Another sociobiologist who has attempted to account, in an explicit manner and within the sociobiological paradigm, for the non-fitness-maximizing behavior of modern humans is Irons (1977; 1983). He explicitly rejects Barkow and Burley's explanation on the simple grounds that it is highly unlikely that natural selection would have produced an organism which under any circumstances actually preferred to under-reproduce, given the opportunity to do so, or not to reproduce at all. The desire for children is common, according to Irons, among human females in particular, though perhaps not universal; and there is no reason to believe that the strength of this desire is negatively correlated with intelligence, ability, and wealth. It is important to note here in defense of Barkow and Burley that they do entertain the notion that selection has countered "our tendency towards fertility control by strengthening our desire for and liking of children" (p. 173). They note Lorenz's discussion of the "releasers of 'attractiveness' in the young, includ-

ing a disproportionately large head and eyes, and a wobbly gait" and speculate that "the rise of intelligence and fertility control resulted not only in the elimination of external and internal cues to ovulation, but a countervailing strengthening of the attractiveness of young children, so that viewing them releases not just protectiveness but a desire to produce them" (p. 173). As they say, there "seems little doubt that the sight of an infant elicits in many people a strong desire to have one." But they go on to argue that one, or perhaps two, children are apparently enough to satisfy this desire – offspring are just one among many "consumption" goods whose sight elicits in people a strong desire to have one.[8] And the "marginal consumption utility" derived from children is probably a strongly negative function of their number (see the discussion in Retherford, in press), as with other such "consumption" goods.[9]

Irons himself attributes under-reproduction in the modern culture to an innate psychological mechanism which in the premodern culture also reacted to abundance and security by limiting offspring but did so because, as I read Irons, the offspring of the offspring would thereby be maximized, through the maximization of *surviving* F_1 offspring. According to Irons, this mechanism does not have this effort in modern culture because mortality differentials are slight and therefore survival rates in poor and rich environments do not differ in any significant way. That is, in rich environments in premodern cultures, according to Irons, "quality" production of children promotes fitness, whereas in poor environments it does not. Hence, the selection for a psychological mechanism whereby humans reduce fertility in rich environments in order to produce more surviving (higher "quality") offspring but maintain high fertility in poor environments because fertility reduction in such environments does not have this effect. In the modern setting, however, since survival rates are roughly the same across environments, this psychological mechanism becomes maladaptive for those living in the richer environments. Reducing *gross* fertility in rich environments no longer promotes higher *net* fertility and therefore higher relative fitness. On the contrary, it promotes lower net fertility and lower relative fitness.

The precise mathematics of these differential responses is not presented, and so it is as yet difficult to judge the plausibility of Irons's model, but the logic, on the face of it, seems less than compelling. It remains unclear, after several readings of Irons's argument, why survival rates should not be higher, in richer environments, at all parities, and why, therefore, quantity should ever have to be sacrificed for quality in such environments. There is no evidence, in fact, that gross as well as net fertility are not higher among upper-status individuals in the premodern populations where the psychological response to rich environments of fertility suppression is said to have evolved (see, for example, Weiss 1980). It is also apparent that, as Barkow and Burley point out, such a model cannot explain deliberate childlessness, which appears to be growing, particularly among the better educated and endowed (Bloom & Pebley 1982; Bloom & Trussel 1984).

Given the brevity of Irons's argument to date, perhaps all that can be said if it is that he has restated the general proposition that novel environments may produce maladaptive behaviors and that in the case of modern culture, it certainly has. A precise delineation of the psychological and physiological mechanisms and the relative fitness calculations involved, however, remains largely obscure. Barkow and Burley alone have treated the subject in the detail it deserves. They hypothesize that modern culture enables women to circumvent the various devices (concealed ovulation, male dominance, strong sexual desire) by which they were prevented in the premodern setting from avoiding the travail of childbearing and rearing. In the modern culture, human intelligence is, in effect, freed to function autonomously and to respond in full measure to the various "proximate" goals programmed into it, for example, status, power, pleasurable activities such as sex, travel, sports, and so on (Barkow 1984). Irons and others might wonder why "having many offspring" was not programmed into it likewise; and the fact that it was not, whereas compulsive seeking after status was, is a continuing mystery. Perhaps the "program" was beyond natural selection's capabilities to produce (Darlington 1983) or was simply redundant and therefore not needed among premodern populations in which higher status was both a necessary and a sufficient condition for higher fertility (Daly & Wilson 1983, p. 335). As E. O. Wilson (1975, p. 548) notes, "Our civilizations were jerrybuilt around the human biogram." Nothing so demonstrates this fact as the rapid abandonment of fitness-maximizing behaviors by modern peoples.[10]

Apart from Barkow, Burley, and Irons, few sociobiologists have taken the problem modern human fertility patterns pose to their discipline to the point of actually attempting to explain them within the sociobiological framework itself. However, most sociobiologists acknowledge these patterns in the world's most successful species to be contradictory to the sociobiological model. Richard Alexander, for example, one of sociobiology's most prolific contributors, does so in a quite forthright manner, surprisingly so in light of the vigor with which he has promoted sociobiological theory as a framework for understanding human affairs as well as animal behavior:

> I have not suggested that culture precisely tracks the interests of the genes – obviously this is not true – but that, in historical terms, it does so much more closely than we might have imagined, and that for the future, our enlightenment on this point is almost certain to reduce the extent to which culture follows the interests of the genes and to increase the extent to which it tracks, instead, our phenotypic interests as individual and social collectives. At least for humans unaware of their reproductive history, then, culture will remain paradoxically both the handmaiden of the genes and the obligate, and not always optimal, environment of their reproduction. In my opinion, for humans who understand their history, reproduction is likely to be bypassed deliberately, and often with relief; in its place we are likely to see the substitution of a combination of reduced reproduction and increased attention to more direct phenotypic satisfactions. I see no possibility that the genetic change selection will inevitably engender against such tendencies can even remotely approach in pace the acceleration of technological changes abetting them. (Alexander 1980, pp. 142–43)

An equally prominent sociobiologist, Richard Dawkins, has likewise expressly rejected the Darwinian para-

digm as providing an accurate model of contemporary society and for the same reasons adduced by Alexander and described in our last section: the systematic violation by the populations of these societies of sociobiology's central tenet, which is that individuals behave so as to maximize reproductive fitness. "Lay critics," writes Dawkins,

> frequently bring up some maladaptive feature of modern human behavior – adoption, say, or contraception – and fling down a challenge to "explain that if you can with your selfish genes". Obviously, as Lewontin, Gould and others have rightly stressed, it would be possible, depending on one's ingenuity, to pull a "sociobiological" explanation out of a hat, a "just-so-story", but I agree with them . . . that the answering of such challenges is a trivial exercise; indeed it is likely to be positively harmful. Adoption and contraception, like reading, mathematics, and stress-induced illness, are products of an animal that is living in an environment radically different from the one in which its genes were naturally selected. The question, about the adaptive significance of behaviour in an artificial world, should never have been put. . . . (Dawkins 1982, p. 36)

Thus, Dawkins, too, accepts the "novel environments" hypothesis, though he shows little interest in working out the mechanism by which the novel environment of modern culture promotes nonadaptive behavior. For Dawkins, as for Alexander, it is enough to know that human populations in the setting of modern culture are not properly understood within the Darwinian framework, indeed, that these populations and their culture follow their own autonomous laws, though these may mimic the laws of natural selection. After defining a "meme" – a "unit of cultural inheritance, hypothesized as analogous to the particulate gene, and as naturally selected by virtue of its 'phenotypic" consequences on its own survival and replication in the cultural environment" (Dawkins 1982, p. 290) – Dawkins writes, "Any new meme's replication success will be influenced by its compatability with [the] existing background. Positive feedbacks will provide a momentum which can carry meme-based evolution in directions unconnected with, or even contradictory to, the directions that would be favored by gene-based evolution" (Dawkins 1982, p. 111).[11]

Note that Dawkins, unlike Alexander, does not acknowledge the possibility that the genetic selection inevitably induced by that of his "memes" could not only be "contradictory" to the "meme" evolution but actually defeat it, through a change in the relative frequences of the various capacities necessary to carry the meme evolution forward. Moreover, whereas Dawkins only notes the *possibility* of cultural evolution developing "a momentum all its own" (Pulliam & Dunford 1980), Alexander expressly predicts that it will and tells us why: Modern culture allows persons, for the first time, to respond solely to their "phenotypic interests" and to ignore their "genotypic interests" (precisely how this link is broken is not described, but see Barkow and Burley, 1980, as summarized above). Most important, Alexander explains how it is possible for these two to become unlinked and for the society to remain viable: The rate of change in man's environmentally induced capacities exceeds by several orders of magnitude the slow pace of genetic selection which would, by theory, eliminate non-fitness-maximizing behaviors and, therefore, the culture which engendered such behaviors. I shall present in the next section some empirical evidence on the observed (phenotypic) distribution of IQ in the American population which tends to support Alexander's thesis.[12]

4. Sociobiology as applied science

If it is not a good descriptive model of the evolution of modern human populations, then does sociobiology have a role as an applied science? I will argue here that because modern human populations do not behave in a manner consistent with its most basic premise, a possible role left to sociobiology is to advise us as to what the implications are for these populations of departures from this premise in the observed directions. In my first section I showed that modern human populations exhibit a systematically inverse relationship between rank and fitness or, to put it another way, between social and reproductive success. If modern culture depends upon the prevalence of traits highly concentrated in those of high rank (e.g. intelligence) and if those traits are highly heritable, then this inverse relationship could undermine the very culture which engendered it by causing a decline in the relative frequency of those genotypes necessary for its existence. Of course, systematic study may show that the impacts of the kinds of selective forces engendered by modern culture upon that culture are not significant in any event, that the speed and consequences of the genetic selection which modern culture puts in motion are trivially small relative to other sources of change. I also consider this possibility here. I do not take up here but leave to a subsequent paper the sensitive moral question of whether sociobiology *should* become an applied science in the sense discussed here. In this section, I confine my attention to whether there are "facts on the ground" sufficient to force the question in the first place.

It is interesting that Lumsden and Wilson (1983) present a rather different vision of applied sociobiology. They argue that the important lessons of sociobiology for modern humans are in showing us how the environment might be altered to cause, in interaction with the underlying (presumably stable) genotypic distribution, certain desired changes in behavior. They use the example of a society which seeks to promote brother–sister incest (to create, say, a more homozygous or less genetically "wild" population, for whatever reason). To accomplish this end, siblings are separated at birth and raised apart. The sociobiological "theory" used here is that humans are genetically programmed to avoid sex with those with whom they have been raised as children, whether related or not. [See van den Berghe: "Human Inbreeding Avoidance" *BBS* 6(1) 1983.] Such a behavioral propensity has been selected for in all human populations according to the theory: Those not exhibiting the propensity leave fewer surviving offpsring because of the deleterious effects on offspring of incestuous matings, gradually breeding themselves out of the population. Sociobiology, according to Lumsden and Wilson, provides a unique insight into how certain strong genetic propensities can be overcome, propensities which may not be consonant with modern culture. The task of an applied sociobiology

is to show, as in this example, precisely how environments can be changed in order to modify certain behavioral patterns genetically programmed to unfold in the unaltered environments.

As can be seen from the curiously academic example they employ, Lumsden and Wilson's idea of an applied sociobiology is not yet a compelling one. For to change the environment in as radical a way as their example demands is to alter human culture beyond recognition, whereas it is its preservation which is presumably the desideratum. Surprisingly, they choose to ignore the other route such a discipline might take, which is the study of how the distribution of genotypes, rather than environments, might be altered in order to promote certain types of collective behavior or to increase the prevalence of certain traits, either to produce a better fit to a constant environment or to accommodate environmental change in a certain direction. They also ignore the possibility of the environment itself causing a certain pattern of reproductive behavior which has feedback effects on that environment, say through an untoward change in the frequency distribution of mental ability necessary to the maintenance of that environment, and the necessity, therefore, of policy measures to reverse or suppress such feedback effects. This is not to say that induced, "local" environmental change cannot have important impacts, only that the insights afforded by sociobiology into such changes and impacts, as Lumsden and Wilson's odd choice of illustrative examples shows, are so far either uninteresting or nonunique to that discipline.

The possibility here raised of a culture inducing its own demise through the promotion of deleterious fertility patterns, we might call, for brevity's sake, the "eugenic problem," as eugenicists were the first to systematically call attention to it. Few sociobiologists have shown an interest in this problem, though the social sciences have dealt with the more general model – certainly not fashionable at present (e.g. Hirschman 1982) – whereby a culture or economy promotes collective behavior and habits which undermine the viability of that same culture or economy. One possible reason for sociobiologists ignoring the implications of gene-culture "devolution," as we might call it, is that the empirical evidence for its being of much importance is, surprisingly, rather weak.

There is considerable consensus among behavioral geneticists and psychologists that the quantitative trait, mental ability, as measured by such psychological tests as the IQ test and a trait of obvious importance to the maintenance and development of a highly technical society, has substantial heritability (see Ehrman & Parsons 1981; Henderson 1982; Plomin et al. 1980). There is also overwhelming evidence, which I outlined above, that the trait, as indexed by years of education or, more rarely, by IQ itself, has a negative association with reproductive fitness. Here, then, seems to be a textbook example of gene-culture "devolution," of a culture promoting the decline in the relative frequency of a trait essential to the viability of that same culture. But like many superficially clearcut syllogisms in human affairs, this one turns out to be less convincing than it appears to be on first sight. Richard Alexander, in the passage quote from his *Darwinism and Human Affairs* above, provides a hint as to why this is so: to wit, that the environmental changes induced by modern culture can cause a shift in the distribution of phenotypes at such a rate that the opposing genotypic changes are overwhelmed. That is to say, the distribution of environments may be changing, due to a dynamic internal to modern culture, in such a way that the actual human phenotypes produced in the culture are superior (in the common-language sense of that word) to those in previous generations, though their phenotypes within the original distribution of environments would have been inferior.

The possibility that the overall level of a trait distribution is highly responsive to environmental change, though the individual variation around this mean is still largely accounted for by genetic variation, is well known to behavioral geneticists, but it is rare to find discussion of this possibility in their texts. Darrell Bock, however, in his review of Arthur Jensen's *Educability and Group Differences*, gives the following brief but lucid account:

> In the Fisher-Wright model for the components of trait variation due to heritability, allele interation, environment, gene X environment interaction, measurement error, etc., only individual differences (i.e., the deviation of the trait value of individuals expressed as a deviation from the population mean) are explicitly treated. The data analysis based on the model deliberately excludes the population mean, the level of which is highly sensitive to the prevailing environment in which the trait – even one that is entirely heritable within the population – is expressed. . . . In the U.S. and European populations, stature has about the same heritability as general verbal ability, and yet the mean stature of these populations has increased nearly 1 standard deviation in two generations. . . . Careful studies of this secular increase . . . have shown that nonenvironmental sources, such as reduced inbreeding or selective survival, are inadequate to explain an effect of this magnitude. (Bock 1974, p. 595)[13]

Indeed, it is a reasonable hypothesis that among modern human populations most of the observed short-term or historical changes in the central tendency of various trait distributions are due to changes in the environment, not in fertility patterns, whereas a significant portion of the variation *around* the central tendency is due to genetic variation.[14] This, of course, is a hypothesis about how things actually work and does not deny that large shifts in the central tendencies of trait distributions could also be accomplished through changes in fertility patterns, as breeders have repeatedly demonstrated for plants and animals (Fuller 1983, p. 463; Medawar 1984).

Though Bock uses stature as his example of a highly heritable trait whose mean value is nonetheless empirically observed to shift in response to changes in the environment rather than in fertility patterns, he might have more appositely used the example of IQ if he had been writing his review today. IQ, like stature, has high heritability but has at the same time undergone changes in its mean which are of a magnitude comparable to and in the same direction as those characterizing stature and which likewise cannot be attributed to changes in fertility patterns. To my knowledge, Richard Lynn's study of the mean IQ of the Japanese population first revealed how large such positive shifts in mean IQ can be (Lynn 1982; previous studies, such as those reviewed by Duncan, 1952, indicated much smaller positive shifts), and James

Flynn's work on the mean IQ of the white American population, which followed almost immediately that of Lynn, revealed similarly large positive shifts for this population, shifts which in fact exceed the half standard deviation per generation recorded for stature (Flynn 1982; 1984a). I have shown in my work on IQ and fertility that the negative change in mean IQ within American ethnic groups implied by current fertility differentials across persons of different IQs, if we assume a heritability in IQ of 0.5, cannot exceed one IQ point (SD = 16) or 0.0625 SD per generation (Vining 1982a). This change is opposite in direction to the observed change in the mean of phenotypic intelligence but, in any event, is only $\frac{1}{16}$ its magnitude in absolute value. Clearly, changes in the mean of the U.S. IQ distribution due to environmental sources are occurring at such a rate that the negative shifts in the mean of the genotype distribution are simply overwhelmed. This is despite the fact that fertility differentials by IQ class are currently not small. A person of IQ 70 has, on average, 0.6 to 1.2 more children than a person of IQ 130[15] (the first figure is for the white population and the second for the black).

Jensen (1981, p. 448) has written that "both the 'environmentalist' and the 'hereditarian' agree that the average IQ of the next generation could be most surely altered by selecting the parents," but this assertion does not seem consistent with the empirical evidence so far available. What are rather large differentials in parental fertility by IQ class do not shift the mean of the IQ distribution at a rate even approaching that actually recorded over the last two generations by Flynn (1982; 1984a). In fact, it is not clear that *any* "selection of parents" could bring about a change in mean IQ of the magnitude Flynn reports, a change which, as we have seen, must be due to environmental causes (if they are not a result of measurement errors). So the current evidence points to a conclusion which is the opposite of that of Jensen – the average IQ of the next generation, like that of stature, could be most surely altered not by the selection of parents but rather by overall changes in the environment. Indeed, I would hazard the guess that it would be impossible to duplicate the kind of shift in mean IQ observed (i.e. one SD over 40 years) if selective breeding were the only tool available to us *and* if we operated under the two additional constraints that the overall size of the population must be maintained from generation to generation and that the institution of monogamous mating is preserved.

Another example of a large change in the distribution of a phenotypic trait is the rapid fall over the last 20 years in adolescent intelligence, as measured by SAT (Scholastic Aptitude Test) scores among U.S. high school students (B. Lerner 1983; Zajonc & Bargh 1980). The mean of the SAT distribution has fallen by about $\frac{1}{2}$ SD since 1963. The preponderance of this decline cannot be explained by the substantial changes, which also took place over this same period, in the racial, sexual, and socioeconomic composition of those taking the tests (B. Lerner 1983; Levine 1983; Zajonc & Bargh 1980). The magnitude of the decline is also far too large to be accounted for by the fertility patterns of the previous generation, contrary to an assertion by Karlsson (1978, p. 187). In fact, as was discussed above, these fertility patterns may have been uniquely neutral with respect to IQ, as they occured during a unique period of rising fertility. Thus, the observed decline in adolescent intelligence in the U.S. seems to be yet another example of the great sensitivity to pervasive environmental change of the "central location" of trait distributions among modern human populations – in this case, the change that has occurred is most probably a decline in educational efficiency, both in the school and in the home, during the adolescent years, since the SAT assumes considerable substantive knowledge of both mathematics and the English language. As Shuttleworth noted some 50 years ago, "Even if environmental differences accounted for zero percent, and heritability differences accounted for 100 percent of the individual differences in intelligence, it would still be true that the general level of the environment would be a most important factor determining the general level of intelligence" (Shuttleworth 1935, quoted in Scarr 1981, p. 72). The available evidence suggests, in fact, that it is the most important factor. Insofar as the IQ and SAT tests measure at all accurately what a complex, technical society requires in the way of cognitive ability, it appears that changes in the general level of such ability are determined overwhelmingly by environmental changes (as yet poorly identified changes but ones clearly substantial force) induced by that society itself. The tendency of the society to promote, at the same time, fertility patterns whose aggregate effect is to reduce the relative frequency of high-IQ genotypes and raise the relative frequency of low-IQ genotypes has much less impact and is of much smaller force.

What has been presented so far clearly argues against the relevance of human sociobiology even as an applied science. There is no reason to believe that the environmental changes which caused the 1 SD increase in mean white IQ over a 40-year period in the U.S. reported by Flynn (1982; 1984a) and the $\frac{1}{2}$ SD decline in mean SAT scores over a 20-year period are best studied within the sociobiological paradigm, unless the kind of environmental analysis outlined by Lumsden and Wilson (1983), an example of which was given above, could be broadened to address changes in societally important trait distributions such as those which have been reviewed here. However, I do not wish to end on this note. There is reason to believe that the positive phenotypic changes in mental ability, and perhaps other traits as yet unmeasured at the population level, such as cooperativeness or altruism (Darwin's "sympathy"), cannot be maintained indefinitely and that further increases – which many would argue (e.g. Cattell 1982) are necessary if the modern economy is to continue to shift in the direction of increased technical complexity, interpopulational dependencies, and environmental control – can only be brought about by changes in the frequency distribution of the genotypes themselves.

The reason for thinking so is that the reaction curve giving the genotypic response to different environments is, if the experience of animal and plant breeders has any relevance at all to human evolution, only linear over a certain range (Bouchard 1976, p. 178). Eventually, this curve flattens out; improvements in environmental conditions cannot increase phenotypic performance indefinitely (Richards 1984).[16] Thus, one would expect that even in the richest imaginable environments, not all persons could be trained to understand certain difficult technical subjects (requiring, say, high spatial ability),

the understanding of which on a large scale, however, may be essential to the continuing growth of the economy along its current trajectory (Cattell 1982). As Richards (1984, p. 77) writes, "*If* genetic determinants operate, they must do so by setting an upper limit on attainable intelligence, a ceiling, above which the individual [and by extrapolation, the nation] cannot go." In this case, it might be necessary to induce gene-culture coevolution, as described in the texts of sociobiology, unless, of course, the evolution of artificial intelligence could release us from this constraint as well (Feigenbaum & McCorduck 1983).

In addition, a poor country (e.g. China) without large surpluses with which to improve the environment may likewise find eugenic measures to be a necessary and unavoidable component of any program to maintain and upgrade its human resources. There are strong hints in the Chinese demographic literature that some policy-makers and population planners in China have come to this conclusion (Dong-Sheng 1981; Ruoyun 1983; Tien 1981). After stressing the poverty of China, Dong-Sheng (pp. 181–82, English translation) goes on to argue, "Only by promoting the births of better offspring can we improve the genetic quality of our population, reduce or eliminate a variety of genetic diseases, and thereby lessen the burdens imposed on both family and nation. Therefore, to promote eugenics is to secure immeasurable advantages with no harmful consequences. Such a course of action would carry much significance for the speed at which socialist modernization can proceed." The recently announced Singaporean experiment in positive eugenics (Baum 1984; Editorial 1984; Gould 1984) suggests that some middle-income countries may attempt mutually reinforcing eugenic and environmental policies to improve their human capital, in order to redouble the effects of already large environmental improvements. Indeed, the emergence of such policies in East Asia, insofar as they are successful, may eventually force the question of eugenics back into the open in the West, as the appearance of Admiral Perry's black boats in Tokyo Bay in 1853 forced the Japanese to reconsider their policy of total isolation and economic autarky.[17] These would be "facts on the ground" alluded to at the beginning of this section, such as do not yet exist within the Western countries, as I have shown elsewhere in this section. How such facts should be responded to is a question that would take us beyond the space of this paper and is best left to a full discussion in another place.

5. Some concluding remarks on the self-dissolving tendencies of modern culture and the relevance of sociobiology to their study

In this paper I have argued that the gene-culture coevolution model, in which genetic evolution tracks that of culture through the link between the superior resource acquisition of those more successful in the culture and the superior reproductive effort enabled by such superior resource acquisition, fails as a descriptive model of modern human populations. All evidence points to a state of gene-culture "devolution" in these populations, whereby those with heritable traits in high demand by that culture systematically leave fewer descendants in the next generation. At the same time, nothing so far indicates that environmental changes induced by modern culture cannot alter the distribution of phenotypes at a rate sufficient to meet the requirements for sustaining that culture. Our best quantitative examples of such environmentally induced shifts in a phenotypic distribution are those reported by Lynn and Flynn for the IQ distributions of the Japanese and American populations, respectively (other, though less striking, recent examples of such shifts may be found in Baltes and Schaie, 1976; Schaie and Labouvie-Vief, 1974; and Schiff, Duyme, Dumaret, and Tomkiewicz, 1982). The other side of this great sensitivity of the central tendency of phenotypic distributions among human populations to environmental change is that large discrepancies between the supply and demand of phenotypes can also occur because of untoward environmental change. Our best example of a measured shift in a phenotypic distribution of the latter type is the rapid decline in mean SAT scores ("late adolescent academic intelligence") among American high school students. This decline, which has been too rapid to have been caused by genetic changes caused by American fertility patterns a generation earlier, comes at a time when, ostensibly, the American economy is demanding more and more of the kinds of mental skills and abilities measured, albeit imperfectly, by the SAT (B. Lerner 1983).

In short, it has been my contention that the gene-culture coevolution model is not relevant as a descriptive model of the movement in the frequency distribution of various culturally important, highly heritable traits, as these movements are autonomous and do not mirror changes in the distribution of genotypes. On the other hand, the model may help us (1) qua Lumsden and Wilson (1983), to diagnose the causes of adverse shifts, by enabling us to identify more precisely the changes in the environment which, in interaction with a more or less constant distribution of genotypes, promote certain collective behaviors that degrade the very capacities necessary to sustain the culture or to accommodate its continuing evolution along its current path, e.g. toward increased complexity and technological control of the physical environment, and (2), perhaps more controversially, to identify those changes in fertility patterns that might lead, through a change in the frequency distribution of genotypes, to a human nature more accommodative to modern culture and its current path of evolution.

The tendency of modern culture to flounder on the shoals of a human nature not expressly adapted to it is, of course, an old theme and model in the social sciences. Thorstein Veblen, for example, observed that the mass culture created by the modern economy "unavoidably has an industrially untoward effect on the temper of the population, bends them with an habitual bias in the direction of trivial emulative exploits and away from the ready discrimination in matters of fact that constitutes the spiritual ground of modern technological proficiency" (Veblen 1915, p. 148–49). In modern terms, Veblen's basic model might be recast thus: Modern culture creates an environment which, in interaction with the genotypes of the population of that culture, causes a shift in the parameters of various trait distributions such that the culture itself may no longer be viable.

Schumpeter, like Veblen (though, curiously, never citing his predecessor in this respect), traced modern culture's "self-dissolving tendencies" to the destruction of certain patterns of behavior and human "types" inherited from premodern culture but essential to the modern economy. "The capitalist order not only rests on props made of extra-capitalist material but also derives its energy from extra-capitalist patterns of behavior which at the same time it is bound to destroy" (Schumpeter 1950, p. 162). Schumpeter's "mode of reasoning" is thus seen to be similar to that of Veblen: Certain patterns of behavior and trait complexes necessary to modern culture, whatever their origin, cannot be sustained in the environment of that culture.

Ortega y Gasset had as his central "model," if you will, of modern culture a similar kind of endogenous dynamic. Unlike Veblen and Schumpeter, however, he did not posit a degradation in the "temper" necessary for modern industrial activity but rather a mental inadequacy in the average human that increasingly constrains the advance of modern culture:

> Civilization becomes more complex and difficult in proportion as it advances. The problems which it sets before us today are of the most intricate. The number of people whose minds are equal to those problems becomes increasingly smaller. . . . This disproportion between the complex subtlety of the problems and the minds that should study them will become greater if a remedy be not found, and it constitutes the basic tragedy of our civilization. By reason of the very fertility and certainty of its formative principles, its production increases in quantity and in subtlety, so as to exceed the receptive powers of normal man. I do not think that this has happened in the past. All previous civilizations have died through the insufficiency of their underlying principles. That of Europe is beginning to succumb for the opposite reason. In Greece and Rome it was not man that failed, but principles. . . . But today it is man who is the failure, because he is unable to keep pace with the progress of his own civilization. (Ortega y Gasset 1932, pp. 90–91)

Or, as Vickers (1983, p. 177) recently put it: "Human systems have become very difficult for human beings to maintain. They demand from whole populations levels of understanding and tolerance seldom before found even among the few."

In response to such ideas, sociobiologists might argue that such "self-dissolving tendencies" are only necessary if the distribution of genotypes remains constant, that is, if what is demanded by the culture in the way of traits and patterns of behavior cannot be supplied by changes in the distribution of genotypes, whose current interaction, collectively, with the various novel environments created by modern culture is destructive, rather than sustaining, of that culture. They would point out that such constancy in the genotypic distribution is neither necessary nor probable and that the following mechanism exists for shifting the distribution of genotypes in a direction favorable to the culture: namely, that those who possess a higher frequency of traits fitting to the culture (e.g. with Veblen in mind, a "temper" more resistant to the appeal of "trivial emulative exploits" and less ready to abandon the "ready discrimination in matters of fact that constitutes the spritual ground of modern technological proficiency"; in Ortega y Gasset's case, a generally more self-conscious and discerning, in a word, more intelligent, population[18]) leave more offspring, thereby increasing the frequency of such traits and the overall "fit" between population and culture.[19]

Sociobiologist D. S. Wilson gives a limited but still illuminating example of how such a process might work, in a culture which requires the ability to operate and understand computers:

> The developers of the computer introduced a cultural innovation that is fast becoming a ubiquitous part of the human environment. Those individuals especially proficient at working with computers have a resource acquisition advantage (e.g. jobs) that did not exist a century ago. If a) a proficiency with computers has a genetic component, and b) a relationship exists between resource acquisition and reproductive success (now or in the future), then the developers of the computer [increased] the prevalence of their genotype far more by their cultural than by their reproductive activities. (D. Wilson 1978, p. 236)

The flaw in this reasoning lies in assumption (b). The positive relationship between "resource acquisition and reproductive success," as far as I can tell, does not exist in modern culture, nor can it be expected to. The only time that it might have existed was in a period of exceptional economic buoyancy after World War II, as documented above. The link between the two has been broken in modern human populations, for reasons also speculated upon above. So far, then, we do not observe in modern culture any signs of genetic evolution reinforcing that of culture. When and if modern peoples finally recoil from the various difficulties and intractabilities of trying to get "culture to go it alone," the gene-culture coevolution model may become the operative planning model and diagnostic tool (though the reader must be reminded here of the hoary problem, never addressed by eugenicists to my knowledge, of the slow turnover and long generation lengths in human populations and the difficulty this poses to those who would use eugenic measures to solve current problems). Until this time, I predict, human sociobiology, as both a descriptive and applied science of contemporary society, will confine itself to the study of those constants of human nature that act as important constraints on and inhibitors of the evolution of modern societies. The systematization of our knowledge of these human universals (or what sociobiologists calle the "human biogram") and of their mode of interaction with modern culture will constitute, in the immediate future, sociobiology's principal contribution to sociology – and will mark the extent to which the latter is "biologized."

NOTES

1. I use the word "modern" here as it is customarily used in the social sciences, namely, to apply to peoples belonging to urban or urbanizing societies of the present or recent past.

2. Indeed, the comprehensive review by Dewsbury (1982) suggests that the evidence is less than "completely clear" that this feedback is positive among a number of other social species as well; see also Chapais (1983). As far as I know, sociobiologists specializing in nonhuman social animals have yet to confront the data of Dewsbury and Chapais.

3. Or, as a popular account explains it, "so far from being shameful or unnatural, the drive for status is one of the principal tools of evolution and natural selection not only in man but in a

wide range of other species, and in particular those species which go around in groups; indeed, it is a question whether there is any animal society that does not have a status-hierarchy. Baboons, jackdaws, monkeys, hyenas, antelopes, rats, hens, swordtail fish – the list goes on and on, and man has his place in it as clearly as any of the other animals. *The function of status is to select for survival the best individuals in the group – the best both individually and for the group*" (emphasis added, Jay 1971, p. 161).

4. Or, as Flynn (1980, p. 9) quotes one particularly forthright (though anonymous) defender of the IQ test: "Jensen's critics have a great deal to say about IQ tests being culturally biased, namely, that they test only for a kind of abstract thinking valued within Western civilization and which is not valued within non-industrialized or simpler or more bucolic cultures. So be it! They do not seem to realize that they have conceded exactly what we have always contended. . . . Our culture may be unusual in that it sets a value on physicists and engineers and surveyors, a pity that and a dreadful cultural bias on our part, but there it is: we want to comprehend the heavens, build highways and bridges, and so forth."

5. Thus Coale (1965, p. 58) writes, "During the long period when natality was falling in the United States . . . for most of that time or a good bit of it there was a difference by socioeconomic class in family size. People with higher education, incomes, and so on had smaller families. During these 130 years the trend looked like a statistical law, but now that's no longer true. During the past 20 years or so, this situation has been in the process of disappearing. Our best estimate is that, quite likely, there will be a positive association between socioeconomic class and natality within another decade or so. This is so partly because everybody has access now to means of controlling family size, and even an unskilled, uneducated person will be able to control it very well. So that one could have come to possibly genetically significant conclusions after possibly a century or more of experience, which would be by now wholly out of date." Kirk (1969, pp. 81–82) comes to a similar conclusion: "Whatever the *historical* importance of fertility differentials in the United States they have been reduced since World War II with the spread of birth control to most segments of the population . . . by the 1960s there was not much difference in completed family size by education and hence little opportunity for dysgenic effect from this differential." The late British eugenicist Cecil Carter also noted the "encouraging evidence with western European culture that within those sections of the population who plan their families, the trend is for average family size to become positively correlated with the intelligence and educational achievement of the parents" (Carter 1961, pp. 199–200). Sweet and Rindfuss (1983), on the other hand, deny that a convergence in fertility differentials ever occurred in the postwar period in the United States. Thus, the demographic literature is not without contradictions on the nature of posttransitional fertility differentials.

Mayr (1976, p. 318) gives a useful discussion of the distinction between, and the frequent noncongruence of, "reproductive fitness" and "adaptive value" in the human as well as in other species.

6. Curt Stern states, "The facts of differential reproduction of groups within a population are, of course, independent of the absolute birth rate of the population as a whole" (Stern 1973, p. 763). Stern is here denying a necessary *logical* connection between differential and aggregate fertility, but there still may be a strong *statistical* association. For an example having to do with regional fertility differentials, see O'Connell (1981).

7. Evidence from medical genetics likewise suggests the possibility of a uniquely eugenic pattern of births in Japan. In all Western populations studied, members carrying the dominant gene for Huntington's chorea have reproductive fitnesses not greatly different from noncarriers (for a review, see Walker, Harper, Newcombe & Davies 1983). The Japanese population is the only one so far found in which the reproductive fitness of carriers is well below average. Walker et al. (1983) offer this fact as an explanation of the relatively low incidence of Huntington's chorea in Japan. It is interesting to note with respect to this fact that Japan alone among the developed countries has an explicitly eugenic law at the national level (Vining 1982b; Whelpton 1949).

8. *Cosmopolitan* Girl puts it this way in a full page *New York Times* advertisement (Oct. 27, 1983, p. D24): "What do women *want*? Remember that funny old question? I think it's been pretty firmly established by now we want what *men* want. . . . Someone to love and be cherished by and work that fulfills us. After that comes *other* important 'wantables' like children, friends, recognition, travel, security. My favorite magazine says go for the 'majors' as well as the 'minors' with all your might. . . ." Although this particular *Cosmopolitan* Girl's "wantables" are rather less materialistic than those of other *Cosmopolitan* Girls on the pages of the *New York Times*, it is clear that children are among her "minor" "wantables." And although children may take up a primary position on her list of the latter, any parent can tell her that only with great skill can they be made compatible with the remaining "wantables" on that list, with the possible exception of the last, i.e. security. For a caveat concerning even the last, see the following footnote.

9. A short discussion of the "economic" model of fertility reduction may be apropos here. Caldwell (1982) attributes the reluctance among modern industrial peoples to bear and raise children to the fact that the benefits no longer run from child to parent, as in a peasant economy, but rather almost wholly in the opposite direction. Schultz (1983) has objected to this model of parent as cost/benefit calculator on the grounds that it predicts no reproduction at all in the modern welfare state and yet some reproduction does take place. Hence, children must have some utility in and of themselves for their parents, if no net benefits of a material kind. Heinsohn and Steiger (1979), in an obscurely placed but brilliant article, deride the utility or "child as consumption good" model, noting that birth rates are falling to very low levels indeed in the developed world. They cite (p. 259) the extraordinary statistic that of the 600,000 births in West Germany in 1978, only 150,000 were planned! Thus, in Germany children have become an occasional byproduct of sex with which mostly the careless are stricken. The Germans, however, are the world's least reproductive people (Calot & Blayo 1982; Chaunu 1981) and may not be a proper test of the economists' model. It remains true, nonetheless, that most modern populations are breeding at below replacement rates and that therefore populations in which children only have value as consumption goods (Blake 1968) will eventually breed themselves out of existence. It would appear, therefore, that the economist's utility-maximization model of procreation is accurate only for a transient reproductive culture (see the related discussion in Hirshleifer 1980, p. 663). A recent critique by Ben-Porath of this model (as developed in Becker 1981) gets this point completely backward: "Becker's approach adheres strictly to the standard micro-economic analysis of household behavior. It is based on the maximization of stable utility, specified in the most general terms, and assumes equilibrium. Maximization can be implicit, i.e., a methodological device of the theorist. The implicit nature of this 'as if' methodology is very clear when it is being applied in social biology, as is done in the discussion of nonhuman species. I do not wish to elaborate on this controversy here, but let me just note in passing that in the social biology application, the maximand, fitness, is directly related to the evolutionary selective survival mechanism. Similarly, the fact that firms cannot survive for long with negative profits provides a basis for a survival mechanism on which implicit profit maximization rests. This is less compelling in household behavior where those who fail to maximize are not necessarily eliminated" (Ben-Porath 1982, p. 58). On the contrary, it is those who maximize who are necessarily eliminated (as genotypes), because they will re-

produce at a lower rate than those who do not. In saying this, I assume, of course, that the capacities for utility maximization are, to some extent, variable and heritable. Such an assumption would not have the blessings of modern economists, since the contrary assumptions of identical utility functions and identical capacities for their maximization are deeply embedded in the modeling culture of economists (Stigler & Becker 1977). Earlier contributors to utility theory, however, in particular Edgeworth (1881), would have regarded the identicality assumption as fanciful and unrealistic. Drawing upon the work of Galton on human trait variation and inheritance, Edgeworth "argued that individuals differed in their capacity for happiness" (MacKenzie 1981, p. 98).

10. "It would appear," write Moore and Wilson (1982, p. 209), "that married women who plan to reenter the work force after family formation should limit their family to two or fewer children or plan to suffer significant economic consequences" (see also Hofferth 1984). The fact that modern humans might consider the economic consequences they will suffer on account of a high reproductive fitness to be more significant than the loss of fitness they will suffer in their pursuit of economic success is not even remarkable to Moore and Wilson, which serves to demonstrate how little impact the sociobiological or evolutionary paradigm has had on the practice of economics, despite important and influential articles arguing for an evolutionary perspective (e.g. Alchian 1950; Hirshleifer 1977).

11. For example, in accordance with Lotka's maximum-power principle (Lotka 1922; Parsons & Harrison 1981) and allowing, for the moment, for species-level selection, the human species may find that it is more efficient to increase its size among all species by maximizing its control over energy through the development of exosomatic instruments (nonhuman capital, including nonhuman biological capital) than by maximizing fitness (human capital, i.e. children). (An individual analogy would be with a wealthy man who wishes to maximize the size of his estate under his name down through time and knows that this is not done through creating as many descendants as possible, though the creation of *some* descendants is probably necessary. It sometimes appears that wealthy men regard their estates with the same fondness as they do their biological offspring [Cooper 1977]) Higher fitness *generally* follows from and is promoted by the maximization of energy flow (Altmann 1984; Van Valen 1976), but when the latter interferes with the former, as it seems to at a certain stage of economic development, the maximization of fitness is abandoned. The problem with this strategy is that biological reproduction, at some level, may be necessary to maintain the culture or "meme" that is maximizing energy captured exosomatically. Moreover, as we discuss next, low relative fitness among certain genotypes, i.e. those innately predisposed toward the development and maintenance of the exosomatic instruments, may also ultimately defeat this strategy. On the other hand, a self-sustaining growth in the exosomatic instruments that gradually becomes independent of and detached from the humans who initially developed them is not inconceivable. For example, Simons (1983) writes: "Machines are evolving limbs, senses, brains, cognitive facilities, emotion, free will, and the capacity for reproduction. A machine capable of self-reproduction and of taking appropriate adaptive action – *must surely be regarded as alive.*" In this case, we (or our equivalents in such a world) should have to develop a theory of their reproduction, a theory which would most probably include the standard elements of evolutionary theory: variation, inheritance, mutation, and selection (artificial and natural). And the eugenic problem might then arise in the machine culture as well: Would the more successful machines (in capturing and using energy) be less inclined to reproduce themselves, thereby leading to the failure of the energy-maximizing culture (vis-à-vis other species)? [See also Searle: "Minds Brains and Programs" *BBS* 3(3) 1980]

12. Wilson and Charles Lumsden, virtually alone among sociobiologists, continue to promote sociobiology as a viable paradigm for the study of modern human societies. In a letter to the *New York Review of Books* responding to a review of their *Genes, Mind, and Culture*, Lumsden and Wilson challenge the assertion of historian Kenneth Bock "that the universals of biology and genetic theory cannot account for recent history or the differences betwen cultures" (K. Bock 1980) and hint that a breakthrough in sociobiology's ability to account for current history and culture "is about to happen" (Lumsden & Wilson 1981b, p. 74). But no such breakthroughs have been published, to my knowledge, at least none in a form recognizable to serious students of historical and social change and cultural diversity (see, on this same point, Gould 1983).

13. Still more evidence on this point has been published since Bock's review. Frisancho, Cole, and Klayman (1977) show, for example, that shorter than average parents have made a greater contribution to the positive secular trend in stature in the U.S. than taller than average parents. No genetic model of this trend is consistent with this fact.

14. Besides mean stature (see, for a general review, Meredith 1976), mean age at menarche and diameter of pelvic brim among females have also shown strong secular trends in Western countries (Angel 1973; Wyshak 1983) that are clearly due to overall environmental changes rather than differential fertility by genotype. There is also evidence of a secular increase in mean brain size (Miller & Corsellis 1977), an increase that is likewise too large to be explained by differential fertility.

15. Recall that these statistics are for a cohort which was born in the period 1942–53 and observed in the period 1976–78 and which, therefore, had not yet completed its fertility by the time of observation. Expected lifetime fertility for this cohort shows somewhat smaller differentials (Vining 1982a), particularly for whites, which would lead us to expect an even smaller selection differential than the one used here for illustrative purposes.

16. An example of this phenomenon at the population level is the recent significant slowdown in the rate of secular increase in the mean stature of the populations of developed countries (see Takahashi 1984, Figure 5, p. 431; Terrenato & Ulizzi 1983, p. 344), despite continuing rises in their standards of living (though the reader should also be aware of the existence of data indicating no such slowdown, as reported and summarized in Chinn and Rona, 1984). Apparently, further increases in mean stature, if such were desired, would have to be accomplished through the differential reproduction of genotypes rather than still greater improvements in environmental conditions. It is tempting to speculate that the secular rise in mean IQ recorded by Lynn and Flynn might be subject to similarly decreasing returns to environmental improvement.

17. Gould (1984, p. 27) and, surprisingly, Dawkins (1982, p. 26) both declare eugenic policies to be "illogical," an assessment whose own logic escapes me, as surely neither would deny that the same success that breeders have had with domestic plants and animals *might* be achieved with humans as well (Fuller 1983, p. 463) and that national eugenic experiments *might* bring competitive benefits vis-à-vis other nations and therefore be attempted by some nation somewhere sometime (Cattell 1972). Or would they deny it? The writings of such uncomprising antieugenicists as Gould and Lewontin rather tellingly avoid committing themselves on this question.

18. Though even so exceptionally intelligent a person as John Maynard Keynes confessed that he, and the Bloomsbury group as a whole, were incapable of grasping the basic principles on which modern culture rested: "We [the Bloomsbury group] were not aware that civilization was a thin and precarious crust erected by the personality and the will of a very few, and only maintained by rules and conventions skillfully put across and guilefully preserved. . . . It did not occur to us to respect the extraordinary accomplishment of our predecessors in the ordering of life (as it now seems to me to have been) or the elaborate framework which they had devised to protect this order" (Key-

nes 1949, pp. 99–100). But the failing here appears to be that of youth and training (Himmelfarb, 1985, cites the falling away of religious training in particular) rather than intelligence per se. Ortega y Gasset suggests the failure is along both dimensions, in the population at large. Himmelfarb's (1985) essay on the Bloomsbury group and its ancestry, incidentally, provides about as clear a demonstration as is available of the rapidity with which cultural changes (Himmelfarb would call it degeneration) can take place across generations within a breeding population – which the intellectual aristocracy of Victorian and Edwardian England was, to a good first approximation. These changes were literally self-destructive (very few of the Bloomsbury group left any issue) and, one presumes, took place within a more or less constant genetic background.

19. At least one prominent defender of sociobiology, the philosopher Michael Ruse, however, denies the lack of such a fit in the first place. According to a *New York Times* report on a recent conference on genetic engineering, "Ruse . . . questioned whether adaptations in the basic substances of human life would be useful. 'We are already highly adapted for the kinds of life we live,' he said" (Austin 1983, p. 27). Ruse, however, would be in a tiny minority among those sociobiologists who have thought about the matter, if my reading of their writings is at all representative. See, for example, Robin Fox (1982): "One thing is certain: our evolved repertoire was *not* intended for this environment" (p. 76). Or Konrad Lorenz: "Man is not bad from his youth up, he is just not quite good enough for the demands placed upon him by modern society" (as quoted in Eibl-Eibesfieldt 1979, p. 460). Baker (1984) gives a highly interesting discussion of the maladaptiveness of certain non-Western populations in the typical environments of modern Western countries.

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Editorial note

Although touched upon by various commentators, the following bears emphasizing: What are the criteria and evidence for inferring genetic causes and effects in the present context? A negative association between wealth and reproduction is not especially interesting biologically if there is also a *dis*sociation between wealth and genetic fitness. Unless it can be shown that *genetic* factors are playing a significant differential causal role in making and keeping enough people rich and successful and enough others poor and unsuccessful, Professor Vining's calculations can reflect only the context-dependent cognitive strategies of people – genetically homogeneous on the variables pertinent to this discussion – when they happen to be rich or poor. Such nongenetic variation in reproductive strategy represents no special challenge to sociobiological theory. In fact, the very same evidential burden must likewise be borne by all sociobiological hypotheses that posit genetic determination in place of more plausible cognitive alternatives. To attempt to attribute specific cognitive strategies *themselves* to genetic causes would entail a still heavier evidential burden.

Passion for sexual pleasure, the measurement of selection, and prospects for eugenics

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Vining's is an important contribution in the quest for a better scientific understanding of human sociobiology. I wish to make brief comments on (1) the "novel environments" hypothesis, (2) how to measure selection, and (3) prospects for eugenic selection.

The passion between the sexes for erotic pleasure is probably the major proximate cause of offspring. Socioeconomic changes associated with modernization have caused children to shift from being economic assets to expensive liabilities for parents in many societies (Caldwell 1982). Advances in contraceptive and abortion technology have made it easier for individuals to separate the procreative and recreative (erotic) functions of sexual intercourse. Many scholars have contended that the "novel environment" created by costly children, more effective technological means for separating the procreative and recreative dimensions of sexual intercourse, and so on, has generated selection producing an inverse relationship between socioeconomic power and reproductive success (see Bajema 1976).

Selection is produced by the ecological interactions organisms have with the physical conditions of their environment, with individuals of other species, and with members of the same species. Because selection is a function of the environment, the direction and intensity of selection are as changeable as the social environment and the interspecific and physical environment. Whether a given human phenotypic characteristic such as social status or intelligence is selected for or against may very well be a function of the social practices prevailing at the time (Bajema 1963). Consequently there is no reason to expect that selection will always favor the reproductive success of humans having such phenotypes as socioeconomic power and intelligence. Foresight about the parental costs of reproducing in a given socioeconomic environment, for example, may well be the major reason why individuals in the upper socioeconomic classes are restricting their fertility more than others and thus generating selection against intelligence (Hardin 1968).

Studies that measure only certain components of selection may lead to erroneous conclusions about both the direction and intensity of selection, particularly with respect to socioeconomic power or intelligence, because the observed relationships with reproductive success are quite low. For example, the Minnesota (Higgins, Reed & Reed 1962), Michigan (Bajema 1963), and Massachusetts (Bajema 1971) studies all found that the proportion of individuals not reproducing at all was inversely correlated with IQ. Studies that (1) exclude nonreproductive individuals, never-married individuals, or those not currently married or that (2) report the fertility of individuals who have not completed their childbearing years must be analyzed with extreme caution. The life table method, which involves computing the intrinsic rate of natural increase, provides the only means currently available whereby all of the biological variables (differentials in mortality, fertility, and generation length) can be taken into account simultaneously. The intensity of selection against individuals in the 80–94 IQ range compared to individuals in the IQ $\geq$ 120 range decreased by 22.5% when generation length was taken into account in addition to completed fertility by Bajema (1963).

Hermann J. Muller (1934), Julian S. Huxley (1936), and other have contended that eugenic environments are a prerequisite for eugenic selection (Bajema 1976). The prospects for eugenic selection appear bleak in Western industrial state democracies unless significant reductions in the cost of child-rearing are made. More intelligent women are more likely to opt for more

children if governments not only provide adequate child allowances but also assume most, if not all, of the costs of child day care and education, including higher education.

Exactly 50 years ago (1935), both H. J. Muller and H. Brewer published proposals that artificial insemination using the sperm of a donor that is not the woman's sexual partner be used to achieve eugenic goals (Bajema 1976). This system of human reproduction has the same effect as a polygynous mating system. The extent to which it is and could be used as a means of eugenic selection needs to be more carefully explored.

It is desirable to investigate reproductive differentials in a variety of human societies at frequent intervals in order to assess the biological consequences of various social practices. The academic community is indebited to Vining not only for doing this but also for discussing scientific questions concerning the applicability of sociobiologiocal theories to contemporary, urbanized societies.

Central problems of sociobiology

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Except for the question of just what is sociobiology's real "central theoretical problem," I mostly agree with Vining. We still do not know why upper classes should have relatively high fertility rates during periods of overall population increase and relatively low rates during periods of population decrease. But Vining's point that sociobiology can account neither for the demographic transition nor for the complexities of modern society in general is nonetheless clear and, one hopes, henceforth uncontroversial.

But I would go further. I would argue that sociobiology can account for the complexities of *no* society, modern or otherwise. This is because, for me, the field's current "central theoretical problem" is the relationship between sociobiological explanations and those at the levels of psychology and sociology/anthropology.

The sociobiologists of whom Vining is critical (e.g. Irons 1983) are mistaking a theory of relative gene frequencies for a theory of individual motivation (psychology) and of society (Barkow 1980a; 1984). Sociobiology is certainly the underpinning of psychology, and psychology underlies sociology. These fields must be consistent with sociobiology (and with one another) or else our theories are either incomplete or false. But we must not imagine that sociobiology's inclusive fitness maximization hypothesis *is* human psychology and that we human beings have some sort of drive to maximize our biological fitness. Rather, natural selection has produced in us a complex host of psychological traits which in earlier environments interacted together to generate fitness-enhancing behaviors. In similar fashion, we must not imagine that cultural traits are reducible to fitness-enhancement strategies. History is constrained by our biology but is more than a mere reflection of it. Only if one does not accept the concept of levels of organization can one believe that the demographic transition data in some way falsify sociobiology.

Nancy Burley and I (1980) argued that fertility falls in postdemographic transition societies because fertile women happen to be gaining more control over their own fertility in such societies. This conclusion is not inconsistent with sociobiology; we explain how there is no reason to suppose that selection has ever favored a powerful, autonomous female lust for pregnancy. Evolution is only concerned with outcomes, after all, and a better-substantiated lust has been sufficient to ensure the outcome of reproductive success.

In similar fashion, our societies are marvelously intricate systems perpetually generated by daily interactions among human beings bearing the complex psychologies resulting from the genes whose relative frequencies can be explained by sociobiology. Most societies have no doubt tended to enhance the fitness of their participants, just as most businesses have tended to make a profit. No economist would automatically assume that every business (or component of a business) is necessarily profitable: Why should we assume that every society or component of a society is necessarily enhancing fitness? Some businesses lose money, some societies may suffer population decline. Either these tendencies are corrected or else neither business nor society can long continue unaltered. Economists do not therefore throw out the profit motive and sociobiologists cannot ignore inclusive fitness, but neither the assumption of profit maximization nor that of gene maximization, respectively, precludes the need for understanding the complex processes subsumed under the disciplines of economics and sociology/anthropology.

Vining's point that the means of even highly heritable traits can alter immensely due to changes in environment bears emphasizing. Empirically verifying Lumsden and Wilson's (1983) "thousand-year rule" would, as I pointed out prior to their coining that term (1980b), probably be impossible for that very reason. For example, many Latin American societies have a value complex known as *machismo*, a sort of cultural preoccupation with an exaggerated form of male honor. Males who are not sufficiently "macho" are low in prestige. Suppose we were to hypothesize that the most "macho" males had the greatest reproductive success (or the least "macho" the lowest success): Could we then verify that, under the 1,000-year rule, Latin Americans males tended to be "genetically" more "macho" than other populations of males? I would suggest that the culturally patterned socialization experiences of these societies would have moved the "mean-macho-rating" two or three standard deviations away from any previously existing mean, and made the contribution of any change in allele frequencies probably trivial and certainly undetectable (Barkow 1977; 1980b).

Sound and shoddy sociobiology

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Vining's provocation is a salutary rebuke to the adaptationist zeal of some sociobiologists. I doubt, however, that he has produced a negative instance falsifying sociogiology. My remarks are meant to distinguish the sound core of the field from epiphenomenal ad hoc hypotheses and gratuitous extensions that prompt such refutations as the one before us.

Sociobiology is the science of the social structure of sexually reproducing animal species (Wilson 1980). The assertion that individuals strive to optimize their reproductive fitness is not a quantifiable truth about behavior or motivation. It is a simplifying assumption needed to generate models that map observed behaviors and habitat conditions into calculi of Darwinian population genetics. Sociobiology exhibits structural properties of the aggregate effects of behaviors within and across species, in perspective of evolutionary adaptation. This is its achievement. But for that reason it is neither an applied science nor a general science of behavior.

1. Assuming that Vining's data do indeed establish an inverse correlation between social rank and reproductive fitness, the time scale is much too brief to indicate a trend significant in the evolutionary time scale. The trend extends over but four generations, and for 25 of those 80 years it was reversed. I obviously disagree with sociobiologists who think that they have something to explain. Vining's criticisms of their attempts are apt, but in my estimation do not cut deeply enough. Apart from the point just stated, they can be objectionable because they rely upon

psychological and motivational assumptions that have no basis in sociobiology. Psychological premises ought to be imported legitimately only, via bridging principles to other fields. For example, from endocrinology one could take the evidence concerning impairment of fertility by stress, and generate the hypothesis that the cohorts deemed high status in Vining's data were differentially stressed during the period studied. Operationalizing this hypothesis onto a sociobiological grid would require some new theory and very arduous data collection. Unfortunately, sociobiologists often evade such efforts, preferring to invent motivational psychology by projecting introspective plausibilities onto behavior (Barnett 1983; Etkin 1981). Sociobiologists who want to play the psychologist should do so scientifically.

2. The data pertain mainly to white American since 1900, corroborated by thin data from Japan and England. A control is not identified. This data base does not appear to warrant the inference that the negative correlation is "one of the universals characterizing modern culture."

3. Exactly what trait is being correlated? Vining characterizes it variously as endowment, rank, and social status, which is assumed to be sampled by occupational differences, *Who's Who* listing, and IQ. He does not identify the relevant trait by its ethological name: dominance. He assumes that the trait correlates strongly with IQ, although no evidence to this effect is presented. He relies instead implicitly on Francis Galton's assumption, which has been uncritically retained by eugenicists despite sociobiology's correlation of reproductive fitness with dominance (Hamilton 1964). To put my objection intuitively: Bullies are notoriously poor scholars, while bureaucratic institutions reward traits of subordination, such as the ability to carry out instructions and be a team player. A better index of dominance in political societies is leadership, regardless of socioeconomic status. In brief, the refutation fails because the appropriate trait has not been selected. [See also Bernstein: "Dominance" *BBS* 4(3) 1981.]

4. It would still be a matter of consequence if the data showed that *any* group is not optimizing its reproductive fitness. I am not convinced that Vining's data give any more precise indication to this effect than do statistics on the use of contraception. It may seem obstinate to insist that widespread use of contraception is not conclusive evidence of voluntary reduced reproductive fitness. Yet one must insist, because reproductive fitness is a scientific concept requiring measurement and evaluation according to methodological rules. In the framework of those rules, reasons humans may give for their reproductive behavior are disregarded in favor of a search for material causes. Because there is at present no causal theory of fertility fluctuations (Ehrlich & Ehrlich 1979), sociobiologists and their critics lack an objective independent basis for evaluating an apparent deviation from postulated reproductive optimization. This does not prevent some sociobiologists from speculating about the causes. They owe Vining an answer.

5. Vining's knockout punch is the eugenics anxiety that the gene pool is increasingly polluted by deleterious genes because the cultural safety net inhibits the operation of natural selection. Even if this charming nonsense were true, it is without theoretical significance. In evolutionary terms it would mean only that another sport of nature went to the wall. The prospect may fill one with dread, but the cosmic significance of nightmares is zero. Yet in Vining's distinction between social and reproductive fitness there lurks a significant theoretical problem that has not yet been adequately formulated. In crude terms, it is the problem of how *Homo* managed to jerry-build civilization on the social structure evolved for the hunter-gatherer band. Although human sociobiology is an essential ingredient to a refined formulation of the problem, sociobiologists until now have not recognized that entry into the man-made urban habitat altered the flow of cost-benefit reciprocities by creating public goods available to large politically organized aggregates. Kin reciprocity, in other words, was supplemented by a set of artificial reciprocities supported by the productivity increases stemming from the Late Neolithic technological explosion. The Industrial Revolution turned this supplement into the main show, so that kinship exchange was minor compared with market exchange. The nuclear family was the result. The talents needed to operate this highly artificial culture are indeed far from those that conferred fitness on the hunter-gatherer. But this phase represents a scant 0.003% of *Homo sapiens sapiens'* existence, and if the experiment fails, it is nothing to get excited about . . . theoretically.

A theoretical challenge to a caricature of Darwinism

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Vining has several distinct agendas in his target article, and they are based on several distinct misunderstandings.

In the first place, he has assembled some evidence that human fertility has been negatively associated with material success in certain societies during certain time periods. That is an interesting phenomenon, and one that should provide some valuable clues for anyone wishing to develop a Darwinian psychology of fertility decisions, but Vining's claim that it constitutes a "violation of sociobiology's fundamental premise" is empty rhetoric.

The main reason why an occasional reversal of the usual positive relationship between affluence and fitness is potentially instructive is that we generally need to find circumstances that will disrupt adaptive functioning in order to elucidate the mechanisms by which animals ordinarily achieve adaptive ends, that is, to discover what proximal goals they actually monitor in order to achieve such distal goals as fitness. You're starting to get a real handle on how the hunger mechanism works, for example, when you know how to get a stuffed rat to eat or a starved one to abstain. Similarly, knowing the circumstances under which people pursue material and social goals without converting them to reproductive advantage may help us choose between alternative psychological models of the mechanisms that ordinarily produce adaptive outcomes.

The proposition that psychological mechanisms have been shaped by selection is obviously not in itself a motivational theory, although it can usefully guide the efforts of motivational theorists. (The textbook distinction between "proximate" and "ultimate" causation makes essentially the same point.) Vining's efforts to portray his argument as a challenge to a monolithic "sociobiology" are predicated upon an obfuscation of this elementary distinction. The discipline is caricatured as having a "central tenet" or "fundamental postulate" that everything is adaptive, so that the obvious fact that animals do not always maximize fitness can then be presented as a "violation." This was a feeble debating tactic of Lewontin, Gould, and others during the tiresome "sociobiology debate" of the 1970s (e.g. Gould & Lewontin 1979), and it is a feeble debating tactic now.

Vining apparently infers from the data he reviews that nothing very close to reproduction is an evolved psychological goal for *Homo sapiens*, but rather that we have been selected simply to pursue the Kantian trio of possession, power, and honor, and to let the production of offspring (historically correlated with each of the three) take care of itself. We think that such a parsimonious evolved psychology is not plausible in view of people's passionate and cross-culturally universal concerns with genealogy, with heterosexual transactions, and with children, among many other things. But if the evolved mechanisms of the human mind are indeed more complex than Kant's threesome, as we believe they must be, then the psychological characterization of those mechanisms is indeed a challenge. One intriguing possibility is that a proximal token of fitness with which people

are universally concerned is something like *lineage survival,* perhaps as a result of a selective history of interlineage hostility. This idea seems to gibe with many professed values and fears across the gamut of human societies, and perhaps also with the fact that even where average fertility declines with increasing affluence, so too does childlessness.

A quite distinct subject of Vining's paper is the relationship, if any, between intelligence and fitness. And if Vining's inferences from the imperfect relation between affluence and fitness are problematic, his inferences from intelligence data (Tables 5–8) are even more so. He assumes without supporting argument that the quantity measured as IQ is an unqualified asset, and even conflates it with wealth in a conception of "endowment." Yet he concedes that IQ "does not adequately capture the entire spectrum of mental abilities classified under the general rubric of intelligence." Vining then asserts that the thing that IQ *does* capture, namely "scholastic aptitude," is of "importance to modern, technologically advanced societies." Well, perhaps it is, but that is no theoretical basis for expecting that IQ should afford a fitness advantage, let alone for equating IQ with "social success," as Vining does. Anyone who ever attended high school knows that high IQ and high social success are not necessarily positively related!

Surely the intelligence that has been positively selected is of several domain-specific varieties, which may or may not be strongly or even positively correlated with IQ. There is an intelligence to establishing networks of reciprocity and obligation, an intelligence to shaping your child's behavior or your parent's, intelligences to foraging, to hunting, to tool-making, to calculating the probable actions of rivals and enemies, to courtship, to deceit, to revenge, and there may well be trade-offs among these various competences. Moreover, even if IQ *were* a useful unitary characterization of a dimension with important fitness consequences, where is the theoretical rationale for expecting selection upon it to be directional rather than stabilizing? Perhaps people with too little IQ do dumb things, while those with too much are paralyzed by cogitation. If the brainy folks in Mensa are lost in thought and therefore relatively unfit, so be it; we see neither theoretical nor pragmatic grounds to lose any sleep over the matter. (Vining miscites us – with a page number, no less – among a list of people who are alleged to have made something of a reputed correlation between intelligence and fitness. Our book contains not a word on this subject. A paragraph later, we are listed among those finding support for "the gene-culture coevolution model," a citation we find equally baffling.)

Having attributed to sociobiologists an interest in IQ that they do not have, Vining then maintains that the fact of a secular change in IQ "clearly argues against the relevance of human sociobiology as even an applied science." This non sequitur depends not only upon granting the IQ variable a meaningfulness that it lacks, but also upon yet another caricature of researchers operating within a Darwinian framework, namely that they are concerned solely with genetic rather than environmental sources of phenotypic variability.

Finally, if we understand Vining correctly, he counts himself among those who regard eugenics as the "logical outcome of consistent Darwinism" [Collini 1984] quoted by Vining in the Introduction to the target article] and is puzzled that evolutionists are so often skeptical or downright hostile. To our minds, a consistent Darwinism, with its relentless emphasis upon the nonidentity of individual interests, inspires precisely such skepticism. Indeed, a consistent Darwinian is likely to view with suspicion not just eugenicists, but *all* those ideologues, "applied social scientists," and totalitarians of the left or right, who propose schemes to advance society's interests. If Darwinism has *any* "logical" implication of applied relevance, it is that societies do not have interests, *people* have interests. Appeals to society's interests are typically smokescreens intended to advance some people's interests at other people's expense.

Wealth, polygyny, and reproductive success

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Sociobiologists (I can just about bring myself to identify with the name) are so used to being blamed for overenthusiastic Darwinizing that it makes a nice change to be accused of the opposite. Vining is dissatisfied with the response of some of us to the fact that, in Western society, worldly success is not reflected in reproductive success. We shrug the anomaly off as another product "of an animal that is living in an environment radically different from the one in which its genes were naturally selected." Natural selection can only favour behavioural rules of thumb which, without the behaver being aware why, tend to have the effect, in the environment where most of the selection took place, of maximizing reproductive success. Change the environment and, of course, you'll be lucky if the rule of thumb works. Vining notes this general cop-out, but wants us to be more constructive and specific. "What is the precise agent in the modern environment which causes its better endowed inhabitants . . . to violate in so fundamental a way the central behavioral premise of the Darwinian theory?"

Normally I hesitate to offer specific suggestions of this kind, preferring to emphasize the general disclaimer I have just made. In the present case, however, there are two possibilities that I can't resist mentioning. These are the welfare state and legally enforced monogamy. In nature, individuals that attempt to rear more offspring than they can afford end up with low reproductive success, because most of their offspring starve (Lack 1954). In Western society no child starves to death, however poor and reproductively profligate its parents. The state steps in with unlimited resources. Lack measured clutch size and final reproductive success in birds and showed that clutches both smaller *and larger* than average were penalized by natural selection. Does anyone imagine that he would have obtained this result if he had showered Wytham Woods with unlimited surplus food? Most of Vining's data are counts of "total live births rather than children surviving to adulthood." He agrees that this is "unfortunate," but justifies it on the grounds that in modern populations "variations in reproductive fitness do not seem to depart significantly from variations in fertility. . . ." Well, yes, but that does rather give the game away!

Second, Western society lives under a legally enforced system of monogamy, which seems to have its roots in Christianity. What if the mating system under which we mostly evolved was not monogamy but polygyny? In most polygynous animals the equivalents of wealth, power, social success, and dominance are turned into reproductive success through *number of mates.* Nineteenth-century Mormon church leaders averaged 25 children each (Daly & Wilson 1983). If a religious law limits males of a basically polygynous species to one mate each, it is hardly surprising that worldly success is no longer reflected in reproductive success.

Polyandry, of course, is just as conceivable as polygyny and monogamy, but there are strong Darwinian reasons for regarding polygyny as far more likely, especially among mammals. If the sex ratio is equal the mean reproductive success of males and females must be equal, but the maximum reproductive success to which a male can aspire is far greater than the maximum to which a female can aspire (Trivers 1985). The number of offspring a female mammal can bear is limited by the burden of gestation to something not far from the average, and in many species it is not improved by increasing the number of mates. The maximum number of children a male can hope to father is orders of magnitude higher, and under ideal conditions is proportional to the number of different females he can inseminate. It is therefore expected that, where there are adaptations for increasing the number of mates, these are most likely to be found in males. Where there are adaptations for increasing the

number of offspring per mate, these are most likely to be found in females. Because nothing is free, there will probably be trade-offs between these two classes of adaptation. Males are more likely to become specialists in trying to secure multiple matings. Females are more likely to become specialists in trying to secure the successful rearing of the offspring of any one mating.

Obviously not all males will succeed in their aspiration to acquire many mates. Indeed, if there is an equal sex ratio – which is also expected for good theoretical reasons (Fisher 1930) – many males will never mate at all. Nevertheless, even unsuccessful males in a polygynous species will tend to display adaptations to acquire many mates, adaptations inherited from their male ancestors most of whom, of course, were successful polygynists. In polygynous species, then, all males tend to "aspire" to a greater than average number of mates, even though, as a matter of simple logic given an equal sex ratio, only a minority can succeed in this aspiration.

Nevertheless some species are habitually monogamous, though fewer among mammals than birds. Several theorists have attended to when polygyny or monogamy is expected to evolve (e.g. Emlen & Oring 1977; Maynard Smith 1977; Orians 1969; Vehrencamp & Bradbury 1984). These conditions are best regarded as economic characteristics of the species, and include such variables as the "monopolizability" of resources. If the food and other resources needed for breeding happen to be such that one male can easily hog more than his "fair" share, polygyny is likely. This corresponds to a human society in which there is great inequality in wealth. From a female's point of view, a "wealthy" male is a better bet as a mate than a poor male, even if she has to share the wealthy male's resources with other females. If, on the other hand, the resources needed by the species are, by their very nature, scattered and difficult to amass, it is not possible for any one male to become much more "wealthy" than average, and monogamy is expected.

How can we know whether our species evolved under conditions of monogamy or polygyny (or even polyandry)? Lacking a time machine, we must use clues from contemporary biology. First, we are mammals, and mammals, unlike birds, are nearly always polygynous. Second, of the human societies listed in Murdock's *Ethnographic Atlas*, fewer than $\frac{1}{8}$ are monogamous by custom, and fewer than $\frac{1}{200}$ are polyandrous (Daly & Wilson 1983). The majority are either normally polygynous or sometimes polygynous: The "aspiration" to polygyny, in the sense used above, is there. Third, we know from comparative surveys of mammals that certain morphological traits are correlated with degree of polygyny. For example, there is a good correlation among mammal species between degree of polygyny and degree of sexual dimorphism. Our own species's sexual dimorphism falls on the graph in a position suggesting a moderate degree of polygyny (Alexander, Hoogland, Howard, Noonan & Sherman 1979).

I suggest that Vining should redo his analysis of rank and fitness, but among habitually polygynous societies (just as "contemporary" as the monogamous ones to which he has so far limited himself). I bet my boots that he will find a strong positive association. The Darwinian expectation is that human males will tend to follow a rule of thumb that maximizes wealth and power, because in nature wealth and power tend to become translated into harem size. In modern societies in which "harems" are limited, by law, to one female, the rule of thumb ploughs on regardless, and wealth and power become amassed for their own sake, at times, no doubt, at the *expense* of reproduction. And if Vining really wants to develop sociobiology as an applied science (I don't, although Vining misunderstands me when he says that I described eugenics as illogical; I wasn't talking about eugenics, but about racial discrimination), he might think in terms of trying to get the law against bigamy repealed. It seems rather hard to defend on nonreligious grounds. Isn't it an infringement of personal liberty? And if polygamy were permitted, mightn't this have the effects, especially beneficial for children, of reducing the divorce rate and increasing the availability of care-giving adults in the family?

Intelligence and selection

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Intelligence is counterselected in modern society. Should we be worried? Cynics may argue that most of the life around us is carried on successfully by creatures which are not particularly intelligent. They all survive in offspring whereas man, in spite of all his intelligence, imagination, and ingenuity is about to endanger his further existence by his advanced technology of warfare and by overexploiting his environment. A less intelligent *Homo* is perhaps the solution. He might be less harmful to his environment and thus less suicidal.

But as things are, we characterize ourselves as "*sapiens*" and rightly or wrongly consider this trait a characteristic to be valued. Those, therefore, who argue that we should strive toward a more intelligent and enlightened mankind will have at least most of the intelligent on their side – with the cautionary remark in mind that the person acting solely by the guidance of his supreme intelligence, but without man's admitted, archaic emotions of love, hate, and pity, would probably not fit our image of an angel.

Mankind could perhaps survive with mediocre intelligence, and this may even prove better adapted than we are today (for example, by rigid genetic programming for being compliant to the state). But perfect adaptation to the present is not all that counts. We should also consider our potential for further evolution. There is some truth in the argument that it is the universalists, the less rigidly specialized, who have the better chance to continue, to open new avenues in evolution. Man is certainly a universalist, and it is the community composed of intelligent, responsible individuals, gifted with imagination, language, culture, a concern for mankind, the readiness to question and learn and thus to be open to new ideas and the willingness to adapt individually and culturally to changing demands, that gave our species a head start.

The course of further evolution does not necessarily continue along this line. We all know that species can lose differentiations by specialisation. For example, in its adult stage, *Sacculina* infiltrates the abdomen of its host like a rhizome. All crustacean characteristics – the sense organs, the nervous systems, the locomotor organs – have been lost. Only the larvae tell us that the species derived from a once highly organized crustacean. The term "involution" was coined for such developments which lead to the loss of differentiations. The result is a highly specialized and perfectly adapted creature. But its prospects for further evolution are certainly narrowed. A counterselection against intelligence in the long run would certainly adversely affect our creative capacity to individually and culturally adapt.

Should (and can) something be done to guide mankind's fate? We might comfort ourselves with the thought that the trend found by Vining could be temporary and that for the time being the range of modifiability by education leaves enough room to compensate for the genetic change of our intellectual capacity. The trend might also change in time. There are in fact indications that intelligent women are beginning to value motherhood again. But what if the other trend continues? One IQ point per generation is a lot, in evolutionary terms. To wait passively for the mechanisms of natural selection to operate – and hopefully correct this – may prove disastrous to *sapiens*. We must face the problem. No measures can be proposed at this early stage, except that the horrifying experience of Nazi eugenics should be kept as a cautionary reminder. Education and perhaps a differential reward system based on social achievement could be

considered in future. In the long run no civilisation can afford the socially successful and intelligent failing in reproductive competition with the less gifted. Vining has presented the case clearly.

Sociobiology and IQ trends over time

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I find Vining's target article admirable in almost every respect and merely wish to add to his evidence plus make a few coments about sociobiology, evolutionary biology, and eugenics. First, what has happened to man poses no problem whatsoever for evolutionary biology. Competition for mates within a species of deer selects in favor of large antlers, but antler size affects the deer's relationship with predators as a byproduct. Initially, larger antlers may frighten off predators, but after a certain point, movement is more cumbersome and the deer become easier prey. Assume that status within the deer group goes with antler size: A positive correlation between rank and reproductive fitness becomes negative. As for humans, intelligence has survival value and this selects for larger brain size, with multiplication of other mental functions such as art, new aspirations, and the like as a byproduct. Initially, the byproducts of larger brains do not interfere with the advantages of intelligence, but after a certain point, the aspirational system (middle-class values) harnesses intelligence to goals that run counter to reproductive fitness. Assume that staus within human society goes with higher mental functions, including intelligence: A positive correlation between rank and reproductive fitness becomes negative.

Sociobiologists or anyone else should be surprised by such a development only if they are ignorant of evolutionary biology, have a political bias which encourages an arbitrary selection from evolutionary history, or are committing an old mistake for which the history and philosophy of science provides an ideal corrective. If you want to show that status-seeking is good (another black eye for the Marxists), you will ignore evolutionary examples of its being counterproductive and be upset when man's evolution forces you to admit their existence. If you are ignorant of the fallacies that have attended evolutionary biology ever since its inception, you will be tempted to read the mechanism of evolution (natural selection) into the genes of evolving organisms as a drive and posit a drive in man to do well in terms of natural selection! Making natural selection the purpose of "evolution," which has no purposes, may be worse than making it the purpose of evolving organisms, but the latter is quite bad enough. It makes no more sense than making the law of gravity a purpose of those planets whose movements are governed by the law of gravity [Cf. Dennett: "Intentional Systems in Cognitive Ethology" *BBS* 6(3) 1983.]

Vining says that negative selection for IQ looks worrisome, qualifies this by noting that environmental IQ gains are far more potent than genetic losses, and then reflects that in the long run genetic deterioration could bring environmental gains to a halt. His estimates of the maximum effect of negative selection for IQ in a generation are undoubtedly correct: I have presented data from the WISC-R standardization sample in terms of the mean IQ of children classified by the occupational status of their parents (Flynn 1984b, Table 1); if you assume that the children of professionals were the product of two-offspring families, that the children of unskilled workers were the product of four-offspring families, and that all categories in between increase in a linear way between those extremes, you find that the mean IQ loss for that generation was .8 IQ points. Realistically, differential reproduction could not have been so great, and something like .4 points as a genetically based IQ loss is more likely. Vining cites my evidence that Americans during that generation made environmentally based IQ gains at a rate of .3 points per year for a total gain (over 30 years) of 9 points. He also notes the possibility of measurement error for this estimate.

Evidence is pouring in from all over the technologically developed world that the U.S. gains are below average, and the new evidence sets aside any doubts about measurement error. For example, on culturally reduced tests, saturation samples of 18-year-old Dutchmen gained 20 IQ points from 1952 to 1982, 19-year-old male Norwegians gained 11.4 IQ points from 1954 to 1980 or 13.2 points if projected over a full generation, and so on (Leeuw & Meester 1984, Figure 10; Rist 1982, pp. 39–51). Even if Dutch environmental gains ceased tomorrow, genetic losses at .4 points per generation would take 50 generations or 1,500 years to get the nation's mean IQ back to the 1952 level. Still, if natural selection tended to reduce brain size in humans over 1,500 years, I would worry. But before we turn to eugenics, I think we should consider what may happen long before even a hundred years have elapsed. I fully expect we will soon have a contraceptive in our water supply, so that those who wish to conceive will have to take an antidote. Making conception a conscious choice rather than a result of ignorance or carelessness alone might restore a positive correlation between IQ and reproductive fitness. If not, we know how to encourage such a correlation: To get an antidote you must fill out a form, available at your doctor's office, have the signed consent of your spouse or partner, and so on.

In sum, human society may well provide its own correctives for negative trends for IQ long before it makes any difference. Note that the thesis that there was positive selection for IQ in the past, when mankind had high fertility and mortality rates, would at .4 points per generation put the mean IQ in 400 B.C. Athens at 68. Despite our present hypotheses about such trends, clearly things have happened to keep human intelligence pretty stable over the last two or three millennia.

Fitness by any other name

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Vining both makes and answers his own point in a way. The mechanical approach to sociobiology which simply assumes that all human behavior can be accounted for with a combination of appeals to "inclusive fitness," "reproductive success," and "degrees of relationship" is forever being backed into embarrassing corners and, as in the case of Barkow and Burley (1980), for example, driven to even wilder "just so" stories to maintain untenable positions. The answers given by Alexander (1980) and Dawkins (1982) are basically correct: The human organism is now trying to adapt to an environment wildly beyond the range of its environment of evolutionary adaptation (EEA). If it no longer seems to be acting in its own best interests this is scarcely surprising.

Vining correctly cites me as an exponent of this "novel environment" viewpoint (although he would have done better to cite the final chapter of *The Red Lamp of Incest*). This is, as he points out, an honorable tradition in Western thought. What has been added over the past two decades is a wealth of method, theory, and data which enables us to say with ever greater accuracy what the EEA is, and consequently to what degree we are, increasingly catastrophically, departing from behavior appropriate to it.

The mechanisms described by that branch of evolutionary genetics that has come to be known as "sociobiology" are clearly an important component of the EEA, and their usefulness in accounting for "recent history or the differences between cultures" (Bock 1980) lies not so much in telling us why we *do* certain things (although they can help there), but in *why we do*

certain things at our peril. I think once this is understood, and an analysis (which Vining correctly points out is needed) begins to be undertaken, then "sociobiology" will have an important contribution to make, but not perhaps the one it claims at the moment.

On the specific issue of fertility and fitness, there are problems that Vining does not address. Like the old-time eugenicists (with whom he clearly associates) he assumes that "fittest" should be defined in terms of capitalistic success or its supposed necessary tool of "academic intelligence." There is no reason why this should be so, as various early Socialist Darwinians realized and argued (Pearson [1901], Enrico Ferri [1904], etc.). If the proletariat is indeed the class destined by the dialectic of history to defeat, dominate, and extinguish the decadent bourgeois capitalist class (as the latter had dethroned and outbred the aristocracy), then it would figure that the proletariat, as the fittest, would by definition leave the most offspring, and that the decadent bourgeoisie would give ample evidence of their decadence by their declining fertility. This is indeed what alarmed the capitalist eugenicists (see Pickens 1968), and lies behind the unease that seems to inspire Vining and the revived eugenics lobby. It has been a constant tension in "bourgeois reactionary science" (to use a favorite phrase of the proletarian revolutionary scientists).

Yet as the early enthusiastic Socialist Darwinians realized, the logic is relentless: Fitness is defined in Darwinian terms solely by reproductive success; if the proletariat has a much higher average reproductive success then it *is* fitter (or rather its individuals are fitter) and there's an end to it. This is of course deeply disturbing to bourgeois scientists, who proceed to make the error of defining "fitness" by some criterion other than reproductive success and then creating the "problem" of why their criterion does not correlate with fertility. But this is (if I can be pardoned for stealing a cliché from my supposed opponents) a reflection of false consciousness, not of any real scientific problem.

As Vining tells us himself, at the height of the capitalist era, the bourgeoisie *did* outbreed the workers (as the landed aristocracy had previously outbred – individually of course – the burghers.) With the decline of capitalism, the inevitable reverse occurred. The sociobiological argument is indeed ruthless: Those with the greatest reproductive success *are* the fittest; there is nothing really to add. When one puts this onto a world scale, the same applies. If levels of fertility are much higher in "underdeveloped" countries and the industrial nations continue to fall below net reproduction, then "fitness" belongs to the third world – provided the children can be raised to viability. We shall see. Vining is working on a short-term timetable: Nature is infinitely patient and interested only in the long run. In the long run, of course, *we* may indeed be dead, but *they* – whoever they may be – will not. And that is the biological bottom line.

As to explaining the lower reproductive success of the current "dominants," here is a suggestion: Sociobiology speaks of organisms "maximizing their reproductive success" or their inclusive fitness or whatever. It is not at all clear that this describes their *motivations,* however. What it describes is the *outcome* of various of their activities. After all, it is a commonplace that animals do not know they are reproducing, much less maximizing, anything. What they are doing is accumulating resources or power, for which they are proximately motivated, and then copulating, for which they are also proximately motivated. If they get all these right, then maximum reproductive success should follow. But it is these intermediaries they are motivated to achieve, not the success itself. This is no less true of humans. They will strive to accrue resources of all kinds – wealth, power, access to sex – and normally reproductive success (inclusive fitness) will follow. But it is equally possible that a consideration such as the enormous expense involved in raising offspring to a point where they too can accrue these things might well lead them to limit families. Indeed, this is the usual argument for the smaller families of the better educated. Again, there is no real discrepancy here since they are *not* – in this argument – motivated to maximize reproductive success per se, but those things that will, in the normal course of events, lead to it. This they continue relentlessly to do, and if in the end it leads to their success in the realm of material dominance and failure in the area of reproductive dominance, then it will be interesting to see the outcome, if anyone is still around. But it only raises Vining's theoretical problem if we assume that reproductive success was their primary motivation in the first place, and there is no reason to believe that this is so.

The use and abuse of sociobiology

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Vining feels that the central postulate of sociobiology is contradicted by the fact that "modern" humans strive for social success despite a negative correlation between social rank and reproductive success. He argues that this contradiction warrants the rejection of sociobiology as a "descriptive model of the evolution of modern human populations." This would be damning criticism if Vining's logic were sound and his evidence were clear, but they are not.

Vining's measures of rank and reproductive success are inadequate. Except in Table 1, reproductive success is measured as the number of children born (fertility), rather than the number of surviving offspring. Since the ability to provide for offspring and thus successfully rear them may increase with rank, counting births may overestimate the ultimate reproductive success of low-ranking individuals. Worse yet, for some of what he regards as his most telling analyses, involving post-baby-boom reproduction, the subjects have not yet completed their reproductive careers. This is a source of bias because high social status and extensive education tend to delay childbearing.

Rank is variously operationalized as occupation, or appearing in *Who's Who,* the *Forbes 400,* or a social register, and eventually, it is mysteriously equated with higher IQ and education. This may all mean something in Vining's explicitly eugenic world view, but of these only *Forbes 400* membership estimates what sociobiologists mean by rank, namely, disproportionate access to resources. Vining's own and Essock-Vitale's (1984) more complete analyses show that *Forbes 400* members have significantly *above*-average fertilty. Vining questions whether this effect is limited to the highest income levels, but Easterlin (1980b) persuasively demonstrates that economic well-being has strong positive effects on fertility throughout contemporary U.S. society.

There are other problems with Vining's analysis. For example, in large-scale societies – the only ones that might contradict the rank/fitness expectation – much competition for reproductively important resources is within rather than between social classes, simply because few individuals actually change social class. In that case the relevant fitness comparisons are between higher- and lower-ranking individuals within each social stratum. Again, taking income as a superior proxy for access to resources, Simon (1974, p. 69) concludes that within such narrow comparisons "the relationship [between income and fertilty] is almost invariably positive."

Vining admits that *prior* to the demographic transition (to low mortality and low fertility; a change yet to occur in many human populations) there is a positive correlation between rank and reproductive success. Indeed, he admits that *since* the demographic transition there have been significant periods (e.g. approximately 1935–1960 in the U.S.) when reproductive success and rank were again positively correlated. What is the glaring countertheoretic observation? Let me summarize. Over the vast majority of human evolutionary history rank and fitness

have been related in the way predicted by sociobiology. This long-standing pattern of association may, on questionable evidence, be contravened for a few generations, in a few populations under evolutionarily novel circumstances. Vining would have us take these few possible exceptions as the rule!

The ease of finding fault with Vining's method and evidence must not distract us from attending to his even more erroneous evolutionary logic. Even if Vining were to present good evidence of a reversal of the relationship between rank and reproductive success, human sociobiology should be little affected. He challenges the sociobiologists to explain the prevalence of a particular behavioral trait: striving for social rank. One answer would be that the trait conferred a reproductive advantage in ancestral populations, and therefore was differentially transmitted to succeeding generations. Vining grants the advantage in the past, but he fails to realize that it is a sufficient explanation in itself.

Vining's treatment of IQ in a correlational study of rank and fitness is confused. In fact, an evolutionary analysis of human intelligence serves to exemplify my counterargument to his proposals. Morphological evidence (brain size) indicates that selection for increased intelligence has been a dominant facet of human evolution. However, intelligence, as measured by IQ, presently exhibits a high ratio of additive genetic to total phenotypic variance (Cavalli-Sforza & Bodmer 1971). This implies that in modern human populations IQ is not undergoing directional selection, either positive or negative (Falconer 1960). The cessation of selection for increased intelligence does not reduce our intellects to protohominid proportions, however. Selection for intelligence has merely reached an equilibrium. Similarly, the cessation of selection for increased dominance striving would not make us socially passive. Our intelligence and social behavior are what they are because of our evolutionary *history*, irrespective of whether the selective pressures that have shaped that history continue to operate. Any "descriptive model of the evolution of modern human populations" that fails to take that history into account will be inadequate and misleading.

Vining shares some widespread misunderstanding of sociobiology that ought to be corrected. Most sociobiologists reject genetic determinism. They think that genes affect behavior but that the best behavioral programs would be context-dependent. After selection does its work, there will be little genetic variance underlying behavior (inferior programs having been eliminated), but individuals will behave differently in different situations. Vining erroneously thinks that sociobiology focuses on these individual differences when in fact the universals, the shared context-dependent programs, are the most central.

This orientation also explains why sociobiologists worry about "novel conditions." Selection can only evaluate the efficacy of context-dependent behavioral programs in environments that actually occur. Thus, our present endowment of programs comprises those that worked best for our ancestors in the range of environments they faced. Only by chance would these programs specify an adaptive response to novel circumstances. Yet Vining would like to see our adaptive response to the myriad nutritional, ecological, epidemiological, and demographic changes that mark modern society. It's not that sociobiology fails us here; it's simply that evolution doesn't work that way.

Even though current behavioral responses might not be adaptive, sociobiologists can hope to specify what the *likely* behavioral responses are by comparing environmental pressures and behavioral programs across species. Recent work by Daly and Wilson (1981) and Lightcap, Kurland, and Burgess (1982) on child abuse suggests the phylogenetic basis and context-specific triggers for this type of behavior. Here is a case where we might very profitably follow Lumsden and Wilson's (1983) suggestion of modifying the environment so as to alter the behavioral pattern. I argue that such understandings are the unique fruit of sociobiological research.

The bioeconomics of phenotypic selection

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Throughout his career J. B. S. Haldane tried to get across points such as this: "Selection may be genotypic or phenotypic. Phenotypic selection may or may not result in genotypic selection" (Haldane, 1957, p. 512). Selection works so long as the traits that are selected are somehow transmitted from one generation to the next, and there is a positive correlation between the traits of the parents and those of the offspring. If parents who teach their children to eat pizza reproduce more than the general population, then pizza eating will become more common. If in a given environment children derive their pizza-eating habit exclusively from their parents, then that trait will by definition show a high degree of heritability – as assessed by the standard statistical procedures. But this does not imply that there are "genes for" eating pizza, and similar lines of reasoning apply with respect to "genes for" all sorts of other things.

One would think that such simple points would be obvious and that whether selection is genotypic or phenotypic – and if genotypic what for – would be questions to be asked, not assumptions to be presupposed. Unfortunately, revolutionary theory is not easy, and the habit of introducing simplifications can become a pernicious vice. In particular, those who are disinclined to believe in genes for pizza eating or sexual promiscuity tend to consider behavior as having only a very remote connection with the genes, granting of course reflexes whose universal existence cannot be easily denied. In so far as hemophiliacs, for example, are careful with knives, behavior obviously has, in a trivial sense, a "genetic" basis. However, an animal lacking "instincts" altogether would be a complete slave of conditioning. For many of the questions that interest us, what matters is not the genetic basis, but the economic causes of behavior. One is not apt to find out why people eat pizza rather than caviar by studying cookbooks.

The theme Vining deals with has a long history. In the classic work on evolutionary theory by R. A. Fisher (1929, second ed. 1958) it was treated much as "sociobiologists" treat it today. Thus, the statistics on the number of progeny of, say, elected peers compared with those for unskilled laborers in the 19th century make it abundantly clear that attainment of high socioeconomic status far too often does not go hand in hand with high reproductive success, at least in civilized countries. This is hardly surprising, however. Economists know well that it is virtually impossible to raise the level of employment and lower the rate of inflation at the same time. Applied geneticists are equally well aware that one cannot maximize the quality of beef and the yield of milk in the same breed of cattle. Consequently statistics such as the ones above would simply illuminate the ways in which societies regulate their numbers, both in absolute terms and relative to their classes. If, however, one believes that social achievement has a large genetic variability, and so does fertility, the same statistics would also imply a very disturbing consequence – that is, all socially valuable traits would be very rapidly selected against. This is precisely Fisher's major point and one which, as Vining stresses, his contemporary followers only present in oblique, implicit form.

Vining does appreciate the subtlety of the issues, and his refinements help to overcome the oversimplifications and the intellectual ambiguities of his recent predecessors. In our opinion, however, he does not go far enough. Thus he does make it abundantly clear that number of offspring is not the same thing as reproductive success (RS). But to what extent is he justified in using survival of offspring as a measure of RS? If one's children were all to become friars and nuns, one's RS would be zero. The number of grandchildren (a measure Pearson used) would provide a better estimate of this ideal notion, though one that still

risks being affected by a number of relatively obvious biases. Yet, if one wished to consider IQ, the number of grandchildren surviving up to some "adult" age would be a better approximation to RS. Then one would probably discover that the top 1% of an advanced industrial population probably have more surviving grandchildren that do the bottom 1%.

It is hard to find a rational explanation in the recent scholarly literature for why that might be so. The best we were able to find is one of popular wisdom rather than academic science. We translate from the Venetian dialect (Durante 1970, p. 39):

> It so happens that he who is stupid looking is generally indeed stupid and this is an important law of nature. This first of all since one immediately perceives it when someone is stupid, and thus one can handle him appropriately. In addition, by this perception the race is saved, because stupid people have a tendency to marry among themselves. In their progeny stupidity is thus concentrated, but localized, so that intelligent races do not get contaminated. Unfortunately, being stupid, these people have lots of children so that, were it not for Divine Providence, they would swamp out the intelligent ones. Fortunately for the human race, however, from a very young age these children fall from high chairs on their heads, which get split like nuts; they fall from windows, they drown, they get run over by automobiles, and their percentage does not rise. (Even some intelligent children fall from high chairs etc., but . . . .)

In other words, worries about eugenics and euphenics largely arise by not properly taking into account how strongly humans assort in their pairings – a point that did not escape older eugenicists such as Galton and Pearson. Their worries might have appeared legitimate even in eugenic terms, and they were indeed very legitimate in euphenic ones, due to the high disparity in reproductive output that characterizes industrial societies prior to their "demographic transition" – a notion that deserves some clarification.

Simply stated, a demographic transition is the rapid spread, to a large proportion of a population, of a reproductive restraint analogous to the one by elective peers in Galton's classic analysis – that is, a shift to reproducing at or somewhat below replacement. In lumped statistics, as for a whole nation, this can be readily detected only when involving a substantial proportion of its citizens, the term "transition" denoting the empirical fact that the spread of this restraint tends to be a rapid one. That a process of this sort was taking place in some advanced industrial state since the turn of the century has been clear to demographers since the 1920s. However, the sophisticated analytical machinery required to handle such matters properly, and the uncertainties that derive from not using it, much retarded a precise understanding of this phenomenon by the scientific community at large. As a matter of fact, the first precise studies of this phenomenon, performed independently by Lotka and by Kuczynski, degenerated into a bitter controversy of priority between the two that further impeded its general appreciation. Through such a transition large human populations were in fact returning, for the first time after millenia, to reproductive patterns akin to those of their presettlement ancestors.

Reproductive strategies of all organisms can be analyzed from a microeconomic point of view (Charnov 1982; synthetic review in Ghiselin 1974). A balance is struck among a variety of selective pressures. In birds a smaller family means less potential gross reproductive output, but there are diminishing returns to scale as foraging time and area go up, and time away from the nest increases loss through predation. Many animals can and do regulate their reproductive output according to local economic circumstances. We can only expect human beings to assess the quality of their environment and adjust their reproductive output to it. Such adjustments can have a ratiocinative basis or they can involve cultural procedures – rules and customs. The latter are apt to change at a rate that does not keep pace with changing circumstance. Different means of coping might coexist in a balanced equilibrium. A policy of reproducing at above-average fecundity might pay in good periods, at below-average fecundity during bad periods. As long as all averaged out, it would make little difference. Such stratagems might also coexist with ones in which environmental changes are tracked. An organism would then base its reproductive output on forecasted conditions. It would reproduce more in periods of rising expectations and cut back in periods of falling ones. Neither kind of adjustment is likely to be perfect, for various reasons. Those best able to engage in such practices are, of course, the most intelligent and those with best access to resources, including information and power. Their investments are, furthermore, apt to be conservative, opting for the security of having at least some successful offspring over the possible catastrophe of losing everything, both pecuniary and genetical. It is characteristic of such a speculation that adaptation will tend to lag, and overshoot maladaptively. (Inflationary expectations keep prices rising and recoveries are held back because potential buyers hesitate.) So perhaps the ordinary demographic picture in preindustrial Europe reflected adaptation worked out over a long period to "ordinary" economic conditions. The unusual fecundity among the socially dominant classes from 1935 to 1960 might reflect rising economic expectations, whereas the current fad of reproducing very little might be viewed as a response to falling expectations. But this is purely conjectural, and it is not clear what the population is anticipating.

Professional intellectuals habitually overemphasize the role of "reason" in social and economic life. Modern evolutionary economics (Nelson & Winter 1982) has focused on the alternative of a kind of natural selection, as has the new evolutionary theory of the common law. Firms can experiment and repeat whatever happens to work without understanding why, and they can also imitate the practices of the most successful firms. Such behavior, however, does not inevitably produce a tight adaptive fit. If the dispropensity to reproduce is imitated excessively or under the wrong circumstances, maladaptation may result. Adding twice as much phosphate to one's field as one's prosperous neighbor does, especially when nitrate is limiting, is not good economic policy. Neither is reducing one's number of offspring to zero. The Lumdsen–Wilson model (1981a) of gene-cultural evolution is rendered nugatory to the extent that such structures apply (Ghiselin 1982).

We fully agree that sociobiology ought to be looked upon as an applied science. The trouble has been, however, not with science being applied but with its turning into a kind of secular religion, with genes being the final cause for the sake of which organisms exist, and right conduct being defined as attempting to maximize one's inclusive fitness. Much of the effort that has gone into genetical research has been done in order to increase crop yields. So too the hope of Adam Smith and other early economists – that we might benefit from understanding the nature and causes of the wealth of nations – was perfectly reasonable. Fisher believed that selection for socially desirable traits is mostly genotypic and, had his premises been correct, it would have been hard to deny the legitimacy of his eugenical arguments. The foes of eugenics can take little comfort in any facts or arguments that attribute the socially desirable traits to nurture rather than nature. For it is still abundantly clear that phenotypic selection is one of the most effective ways of shifting the proportion of some desired properties in a population. Any society aiming at such a goal may reasonably consider the adoption of policies that encourage certain kinds of people to reproduce more, and others less. Paying university professors higher salaries and giving their graduate students a dependency allowance would reduce the incentive to defer reproduction, but of course many will consider that unfair. On the other hand, the current experiment the Republic of China with an upper limit to how many children each person may have (one, in this case) has the same general effect, but it also has other consequences, such as restricting individual liberty. It is one of the fundamental principles of economics that one cannot optimize everything.

Proximate mechanisms and distal objectives

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"That individuals exploit favorable environments to increase their genetic representation in the next generation" can be taken in several ways, and Vining seems to infer direct motivation. However, there is no reason to presume that human evolution, any more than rabbit evolution, has depended upon an abstract desire to have offspring. All sexual species have been selected to have sex, and until recently in one species, that has been a sufficient proximate mechanism.

The vulcanization of rubber probably had a more profound effect on patterns of human reproduction than have all the wars, droughts, floods, famines, plagues, and economic systems that previously shifted the relative position of affected groups of humans on the r-to-K selection continuum. Cheap, efficient, effective contraception has seriously disconnected sexual intercourse from sexual reproduction. Were this not the case, people on the high end of the socioeconomic ladder would have larger families. This leaves much less to be explained, but there are some important residuals. As Keyfitz (1977) has shown, high-status humans in the United States and Europe do appear: "(a) to devote much effort to overcoming sterility, (b) to aim at having two children with a uniformity unknown in the fertility schedules of the past, and (c) to want to have at least one boy and one girl child. Whether or not they have such an objective in mind, couples act as though each is doing its utmost to maintain both its male and female lines and wants the smallest possible number of children consistent with a high chance of infinite lines of descent."

So the rich are more K-selected than the poor. Still, there is probably a bit left to explain. Vining has mentioned (though I think he has not done justice to them) several explanations/elaborations put forth by several sociobiologists. These explanations are not mutually exclusive, most are logically complementary, and when all of them are fully appreciated, the question becomes whether there is anything substantive left to explain.

Explanations aside, given the phenomenon explained, is the potential importance of human sociobiology somehow diminished? Vining is concerned that an inverse correlation between social success and reproductive success will confine human sociobiology "to the study of those constants of human nature that act as important constraints on and inhibitors of the evolution of modern societies." This is a curious notion of "confine," albeit given an agenda of this magnitude, sociobiologists need not worry about accomplishing so much so fast that they will exhaust the important potential of their field.

Indeed, that prospect is approximately as remote as the possibility that environmental improvements will exhaust "the upper limit on attainable intelligence" in the forseeable future. Vining argues that intelligence is highly heritable and that its heritability will increase with increasing access to high-quality education. Evidence for the former argument (Cyril Burt aside [Kamin 1974]) is at least substantial, and recent work by Teasdale and Owen (1984) makes it nearly irrefutable. The second assumption is almost irrefutable on logical grounds, and the work of Heath, Berg, Eaves, Solaas, Corey, Sundet, Magnus, and Nance (1985) supplies massive empirical support. Unfortunately, Vining infers that these two assumptions/facts imply that we are approaching the limit of the positive effect that education can have on average intelligence. This is a non sequitur. The heritability of a trait is a descriptor that applies only to a trait's variance, and not to a trait's mean. If we were to uniformly lower educational opportunity rather than uniformly raise educational opportunity, this would have the exact same effect on the heritability of intelligence (viz., to increase it), but the effect on the mean would be the opposite. That is, if the environmental component is made uniform (held constant, or "controlled" for) the heritability of a trait increases, but high heritability says nothing about the position of the mean or the ease with which the mean can shift under different conditions.

Vining argues that *if* we are approaching the limit to which education can elevate human ability, *and if* the most innately able humans are underreproducing while the less inherently able are overreproducing, *then* "gene-culture evolution is divergent rather than self-reinforcing" and we have capped off our ability to evolve . . . so *perhaps* (he seems to want to hold this for the next installment) eugenics is, at least in principle, not such a bad idea.

These strategies are as unnecessary as they are untenable. Without appealing to the contrived danger of devolution, eugenics is, on its face, the most glorious prospect that has ever arisen. Indeed, if it ever occurs, the first truely eugenic act will be the most important event that has occurred since the first molecular conglomerate took on a configuration that caused it to self-replicate.

Humans wanted to fly long before they gained the technical ability to do so. The fact that individuals occasionally tried and failed never seriously diminished the acceptability of the objective. Unfortunately, the history of eugenics has been one of schemes that would be either socially repulsive or genetically ineffective, or both. What if would-be aviators had proposed schemes that would have required the cooperation of thousands of people who did not share their objective and whose future might have been jeopardized in the process? The idea of flying would have gained a very bad name. Programs based on affecting differential reproductive success of subpopulations (whether races, socioeconomic classes, or people on different ends of the IQ distribution) have rightly earned eugenics a bad name. Even the strictly positive and voluntary eugenics that Muller and Brewer eventually advocated (which received support from the likes of no less than G. B. Shaw, J. B. S. Haldane, L. Penrose, R. A. Fisher, and J. Huxley [Kevles 1985]) redounded badly on eugenics – not so much because it was socially repugnant, but because the effect on the human gene pool, even after several thousand years, would be negligible.

But what if we gain the technical ability to guide our own evolution without resorting to breeding schemes? What if individuals who carry a subfunctional gene for phenylalanine hydroxylase (phenylketonuria) could have that individual gene altered (see Ledley, Grenett, Dilella, Kwok & Woo 1985) so that it would produce functional phenylalanine hydroxylase in their offspring? What if we gain the ability to alter individual genes so that they function better than ever before? Should we let the bad reputation that eugenics has earned interfere with the good reputation that it may come to deserve?

If 4.5 billion years of evolution were to culminate in a species that might be able to make life an eternal phenomenon (see Frautschi 1982), would it not be the ultimate arrogance for that species to refuse to make the attempt (see Hartung 1985)?

Success in a dual evolutionary model

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Vining's target article falls into two parts: his demographic analysis and his discussion of its significance for sociobiology. Although his analysis provides hard facts that coevolutionists must face, it is not the crucial test for them, for their models require not just that the socioculturally successful should have more surviving children than the unsuccessful but that this should be inevitable. When it ceases to be inevitable, whether or not it actually happens in some particular cases, coevolution fails as an explanation. Before discussing this, however, I must make it clear that I support a dual evolutionary model, not a coevolutionary one as Vining implies.

Unlike coevolutionary models, the dual evolutionary model treats socioculture as a distinct evolutionary system in its own right with its own evolutionary factors, including sociocultural fitness based on the spread of concepts rather than genes (Hill 1978; 1984a). This system is linked to organic evolution through a correlation between reproductive and sociocultural success in poorer human societies. It has several advantages over coevolutionary models, but the only one which need concern us here is the possibility of socioculture breaking free from organic evolution. Coevolutionary models do not permit this and hence are not applicable to situations like that uncovered by Vining's analysis in which there is an inverse relationship between sociocultural and reproductive success.

I have argued (Hill 1984b) that economic investment in offspring is subject to the law of diminishing returns, and ultimately to no returns, for once adequate food and shelter are provided further expenditure has no effect on viability (and, in the case of overeating, may actually reduce life expectancy).[1] In many societies, including the rich hunter-gatherers of the North Pacific coast of America such as among the Kwakiutl and the Tlingit, there is surplus wealth which cannot be used to promote reproductive success and which is used instead to raise the spender's prestige. This devotion of resources to prestige has a sound biological basis because in primitive and poor human societies sociocultural success leads to reproductive success, but in the novel environment of surplus wealth the link is severed. Sociocultural success is no longer the proximate goal to reproductive success but becomes an ultimate goal in its own right alongside reproductive success. It is this situation which falls outside the scope of coevolutionary models.

It is only necessary for the richest members of a society to have surplus resources: devoted to nonreproductive ends because their position at the top of society ensures that whatever they acquire with their surplus becomes prestigious. Other less wealthy members have to imitate them as far as possible if they are not to lose prestige themselves. This introduces an element of conflict between their sociocultural and their biological goals which the former may win. Under these conditions the richest members may still raise more children than the least successful, thus appearing at first sight to support coevolution, but the situation is quite different from that prevailing in subsistence societies where any available wealth will be used to maximise inclusive fitness partly because any extra food and shelter will increase the viability of parents and offspring and partly because there is no other source of prestige to spend it on (Fortes 1949). It is the emergence of prestige as a goal in its own right that makes the difference, and that is possible only when there is more wealth than can be devoted to reproductive success, at least among the richest members of the population.

Irons (1983, p. 206), thinking along parallel lines, has suggested that in Palaeolithic hunting-gathering societies "occasional conscious attempts to space births or to limit additional births," due to a concern with the quality of life for parents and children, were adaptive under the prevailing conditions. Under these conditions any increase in the quality of life would be reflected in the increased viability of the children, and deliberate spacing of births would help to prevent women from having more children at any one time than they could raise successfully. In rich societies, however, this concern with the quality of life is maladaptive because the great cost of maintaining it deflects resources away from reproduction and reduces births below the level at which all the resulting children could be raised to maturity. Vining refers to Irons's paper but his account of it is so involved that the original point is quite lost.

Although the dual evolutionary model explains how there can be an inverse relationship between reproductive and sociocultural success in affluent societies, it does not explain why reproductive and sociocultural success should be directly related in these societies when the population is rising. I would like to suggest, however, that an explanation might be easier to find if the implicit cause/effect sequence were reversed. Why does the population rise when sociocultural and reproductive success are directly related and fall when they are not? This form of the question is based on the possibility that the inverse relationship between social status and birth rate may have predated the overall decline in Western birth rates by one or two generations (Vining, quoting Wrong, 1980).

NOTE

1. The law of diminishing returns applies only in the absence of technological change, the relevant technology here being medicine, so that medical advances would alter the picture. But this possibility exists only within our own society; it can be ignored elsewhere, although among us the cost of medical treatment may sometimes be a limiting factor.

Social and reproductive success: Useful data but rethink the theory

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Vining has performed a great service to the scientific community by pulling together and analyzing an impressive and useful body of data demonstrating an inverse correlation between reproductive success and social success in association with modern low fertility. I accept his conclusion that such a correlation exists. I disagree strongly, however, with his main theoretical conclusions and his suggestion that this correlation justifies a practical program of eugenics.

Theoretical problems. This inverse correlation is not a serious challenge to sociobiology, the evolutionary theory of social behavior. Natural selection produces organisms adapted to the environments in which the selection occurs. One expects adaptation only to the extent that contemporary environments are similar to those of past evolution. Organisms can often accommodate some novelty, but the more novel the environment, the less likely their behavior is to be adaptive. Modern environments are extremely different from those of human evolution; nonadaptive behavior is not surprising.

The nonadaptiveness of modern social behavior does not vitiate evolutionary theory as a tool for understanding modern behavior. A number of researchers have increased our understanding of modern social behavior using evolutionary theory. Many of the behaviors they have studied are nonadaptive but nevertheless predictable from what we know of the environments of human evolution. Space does not allow a complete review of such work, but let me cite a few examples: Daly and Wilson on homicide, child abuse, and nepotism (1981; 1982), Essock-Vitale and McGuire on nepotism (1980), and Symons on male–female difference in sexual behavior and attitudes (1979). [See also multiple book review of Symons's *Evolution of human sexuality, BBS* 3(2) 1980.]

Vining has also greatly distorted what I have said about modern low fertility, and the record needs to be set straight. He says I explicitly reject Barkow and Burley's (1980) explanation of modern low fertility. In fact, I never referred to it. I did refer to Burley (1979) and said: "This hypothesis is worth testing and I believe may eventually provide a partial explanation of modern low fertility" (Irons 1983, p. 205). Vining says I hypothesize an "innate psychological mechanism . . . [to react] to abundance and security by limiting offspring," but I said no such thing and can find nothing I have written that could reasonably be misconstrued as saying this. Nor am I aware of anyone else who has ever suggested this as an idea or prediction derived from evolutionary theory. Vining nevertheless devotes a paragraph and a half to arguing against this view.

Vining also says that all I did in my 1977 and 1983 papers was to "restate the general proposition that novel environments may produce maladaptive behaviors." This is very misleading and I feel I must devote some space to summarizing what I did say. I

started with the assumption that organisms in general could raise their inclusive fitness (Hamilton 1964) by lowering fertility, if such lowered fertility did one of three things in sufficient quantity: raise the organism's own survival probability, raise its offspring's survival probability, or aid the reproduction of relatives. The conditions under which human beings may have evolved to do this can best be judged by looking at societies more like those of earlier human evolution. In these societies, I see little evidence that people lowered fertility in order to aid collateral relatives, but a fair amount of evidence that they lowered fertility to limit the burden of child-rearing (see, for example, Blurton Jones & Sibly 1978; Bugos & McCarthy 1984). It is possible that increased burdens of child-rearing translated into higher death rates for both parents and offspring and that lowering fertility in this way was therefore adaptive in that it increased the number of adult offspring produced over a lifetime (cf. Lack 1954).

Thus, human beings may have evolved to avoid having children, or to abandon or destroy those born, when the cost of rearing such children was perceived as imposing too large a burden on the parents. This could lead to waiting until resources for child-rearing are more abundant before having a first child, or to spacing children. This may be a factor in modern low fertility in that parents may see having a first child as imposing an unacceptable cost on themselves, or second or later children as imposing too large a cost on both themselves and their current children. In modern environments, however, high costs of child-rearing do not translate as readily into high death rates. The difference in life circumstances associated with socioeconomic status in the modern world is associated with higher death rates for those of lower status, but the difference is not large enough to reverse the difference in fertility documented by Vining. However, in earlier environments differences in social circumstance may have been associated with larger mortality differences. This can be investigated. How do mortality rates vary in traditional societies? Does a woman abandoned by her husband who tries to rear a child alone have a high risk of death? (see Bugos & McCarthy 1984). How about the child's risks? If in the environments of evolution an individual, in order to marry, moved to a distant community where few or no kin could be called on for aid, how did this affect the person's survival chances, those of her or his children, and the mating opportunities of the children? (see Chagnon 1979.)

Documenting how such different circumstances affect survival and mating opportunities in both traditional and modern societies could test the proposition both that people tended to lower fertility in earlier, more traditional societies when resources for child-rearing (in forms such as kin aid) were limited and that such premodern lowering of fertility was adaptive. Modern populations may react in a similar way to increased costs of child-rearing, but the very low correlation between the cost of child-rearing, on the one hand, and survival and mating opportunity, on the other hand, may be the specific element in our novel environment that is different and renders nonadaptive a formerly adaptive behavior.

A specific feature of the environment which people may have evolved to respond to carefully is the effect of siblings on one another. In societies most like those of evolution, wide child-spacing was necessary to limit the burden of child-rearing (and risk of death) for mothers (Blurton Jones & Sibly 1978). In adulthood, however, the overwhelming effect of siblings on one another was positive (Chagnon 1979). There was variation in the extent to which adult siblings aided one another, however. Often siblings of one sex were more helpful than those of the other (see, for example, Irons 1983). Also, widowed, divorced, or abandoned parents who had to move to a new local group, leaving children behind, faced a different situation in terms of future aid between siblings than those who could continue to have more children by the same committed spouse.

Thus, the effect of additional children on existing children's long-term welfare would have been a variable for individuals to respond to carefully in evolving human communities. If people did evolve to limit further births in those occasional situations in which additional children would have a probable long-term negative effect on existing children, then the modern world presents them with a novel reason for having few children. The former usually positive effect of siblings on each other in adult life is almost totally gone, and the negative effect in the form of competition for parental resources is vastly increased. This negative effect is probably perceived as greatest by individuals who wish to provide their children with extensive educational opportunities, that is, by members of Mensa and people in *Who's Who*. This model cannot explain deliberate childlessness, as Vining notes. I do not mean to suggest this as the only proximate mechanism, only one of many. As such it could explain low fertility but not lifelong voluntary childlessness. In modern environments, however, the motivation to have some children – albeit few – is much more common than a choice of complete childlessness (Daly & Wilson 1983, pp. 333–34). It is impossible to restate my entire argument in a short commentary, but this should suffice to make it clear that I said more than merely that novel environments produce nonadaptive behaviors.

Practical program. I oppose any governmental eugenic programs. Free choices by individuals to govern their own reproduction in ways that harm or constrain no one else are something I can accept, but a government policy based on the assumption that unskilled workers should ideally have fewer children than members of Mensa or people listed in *Who's Who* is something I find morally odious. Since Vining does not discuss specific programs, I cannot be sure what he has in mind, and how I feel about it. But I can say that I do not want anyone coercing or manipulating my own reproductive behavior. I wish to control that myself, and the best way to encourage maximum control for myself is to advocate it for everyone – including the least skilled of unskilled workers. If the effect of such a policy of individual autonomy is to change the course of future civilization, then let it change. I see no strong reason to expect that high reproduction by individuals of lower academic achievement and lower social visibility (people not in *Who's Who*) will have any future harmful effects. It may even have a salutary effect. Government policy to manipulate reproduction, however, would have clearly predictable, immediate harmful effects. Whether this is in conflict with Vining's view, I cannot say until he discusses practical programs.

Sexual strategies and social-class differences in fitness in modern industrial societies

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We agree that evolutionary models predict that those organisms able to acquire greater resources or access to resources should do so in order eventually to increase their fitness relative to other members of a population. We also agree that the quest for status and prestige makes sense in evolutionary terms only if higher status ultimately results in higher fitness. However, although Vining makes an admirable attempt to test predictions from this perspective for modern industrial societies, we have several reservations about his conclusion. In this commentary, we argue that the data presented here do not adequately test whether in modern human populations higher socioeconomic status results in lower fitness.[1]

The data presented in the target article represent a small proportion of the available studies that address the question of whether members of higher socioeconomic classes have more children. The available data do not show a clear pattern of

positive corelation for earlier times followed by negative correlation in more recent time periods, but instead show mixed results from numerous studies throughout all Western industrial societies and time periods (e.g. Shorter 1975; Simon 1974). Although Vining concludes that modern populations show a negative correlation between socioeconomic status and reproductive success, he notes numerous exceptions, such as increasing fertility periods, the very rich, modern Japanese, and quasi-isolated religious groups. These patterns should be explained rather than considered aberrations. In addition, we believe that there are methodological problems which call into question all current and past studies that appear to show a negative correlation between socioeconomic status and fitness. These problems, when solved, are likely to alter radically the results presented.

Methodological problems: 1. Individuals who do not marry and reproduce or who die prior to completing their reproductive careers. Vining's Table 1 measures the number of surviving offspring of couples over 45 years old by profession. This measure of fitness fails to consider the number of individuals who fail to marry and reproduce.[2] The historical pattern of delaying or foregoing marriage among poor people because they lacked the necessary resources to establish a family is well documented in 18th- and 19th-century Europe (Hajnal 1965; Laslett 1977). Laslett (1977, Table 1.7, collapsed) presents date showing that approximately 20% of all women between the ages of 15 and 40 in six preindustrial settlements were servants and therefore not married. This effect is probably greater for males due to more intense mate competition. Because most of these women come from the lower classes (Laslett 1977), it is probable that much greater numbers of lower-class people fail to marry and reproduce than is the case for men and women of the upper class.

Selecting the completed family sizes for couples over 45 years old also factors out the effects of adult mortality. Maternal mortality in childbirth, as well as other forms of adult mortality, were quite high in 19th-century England (the average duration of a marriage before one spouse died was only 12 years). Adult mortality was greater among poor people (Llewellyn-Jones 1974), hence choosing couples over 45 years old biases the results of the test.

2. The fitness of males of different socioeconomic levels. Increased access to resources may affect fitness in two ways. First, it can increase the number and viability of offspring produced by females. Second, males with more resources can increase the number of females with which they mate. Because human investment in offspring is both intense and longterm, potential variability among females in the number of offspring produced is relatively low. In contrast, because sperm production is relatively cheap, some males can potentially father a disproportionate number of the total number of offspring produced by women (Emperor Moulay Ismail is reported to have fathered 888 offspring–cited in Diamond 1985). Therefore comparing the reproductive output of females of different classes may be missing the most important potential fitness differential that wealth produces.

An important problem with all the tables presented in the target article is their failure to deal with illegitimacy and incorrectly assigned paternity. The official rate of illegitimacy in 19th-century England reached 5% (undoubtedly an underestimate because not all illegitimate births were registered). These illegitimate births were disproportionately produced by female servants and daughters of the lower classes. In Scotland in 1883, 47% of all illegitimate children were produced by female servants, 40% by women working in low-paying jobs, and only .5% by the daughters of professional men (Leffingwell 1976). It is probable that wealthy men disproportionately fathered these offspring (see Depauw, 1976, for data demonstrating that this is the case for 18th-century France). Simply put, rich men may have been practicing a dual investment strategy; having few legitimate offspring to whom they passed down their wealth (and hence their mating advantage) and some number of illegitimate offspring in whom they invested little (and who therefore did not deplete their resources).

We suggest that the offspring of high-class males in more modern data sets (Tables 2–5) are also undercounted due to illegitimacy and incorrectly assigned paternity. Illegitimacy rates have risen considerably in recent years, representing 16.5% of all births in the U.S. (U.S. Bureau of the Census 1984). The number of births to unmarried women is again disproportionately due to poor women. Since there are many more poor people than rich people, rich men would only have to father a small percentage of the total number of illegitimate children to gain a large fitness advantage over poor men. If, for example, the richest 1% of the men in the population are responsible for 4% of the illegitimate children, their reproductive output would be twice the population mean for men.

Children born to unmarried women are not the only problem. Marital infidelity is also quite high. In one sample of urban American married women, 23% admitted having affairs and (significantly) wealth was inversely correlated with number of sexual partners (Essock-Vitale & McGuire, in press). In another study of 106,000 married American women, 69% of all women over 34 years old reported having extramarital sex (Wolfe 1981). Although many of these affairs do not result in pregnancies because of modern contraceptive techniques, there is still some reason to believe that the rate of incorrectly assigned paternity is high. A great deal of data exist on incorrectly assigned paternity through analysis of blood proteins, but they are not published because of protection of human subjects. Diamond (1985), without citing his source, writes that one immunologist in the 1940s found that a minimum of 10% of a sample of 1,000 children had their paternity incorrectly assigned (see, however, Schact and Gershowitz, 1963, for a figure closer to 1.5 whites and 9% for blacks). Because data suggest that adultery is more prevalent now than then by almost 100% (Wolfe 1981), the rate of incorrectly assigned paternity has probably risen as well. Moreover, many of the children born to married women are assigned by those women to fathers other than their own husbands (Hartley 1975).

If the wives of wealthy men are less likely to have affairs (they have more to lose if they get caught and are deserted by their husbands) and there is some tendency for males to be wealthier than their consorts in illegitimate and extramarital affairs, the average fitness of wealthy men may exceed that of their poorer counterparts. Thus gene flow would be primarily unidirectional. This could be true even if poorer men have absolutely more sex partners than rich men because poor men who mate more will achieve their higher fitness only at the expense of other por men (and thus not affect the overall average for poor men).

The growing practice of serial monogamy may further increase the fitness of wealthy males. The reproductive span of women ends at around 40–45 years of age, but men can potentially reproduce until they die. If wealthy men are more likely to raise a set of offspring with a woman and then divorce her during middle age in favor of a younger reproductive age woman, wealthy men may increase their fitness relative to their poor counterparts by having two reproductive careers in their lifetime. Data from cohorts which have not reached advanced age do not test for this possibility (e.g. some of the data in each of Tables 2–5).

3. Appropriate comparisons. The problems mentioned above are further exaggerated by the comparisons employed. Tables 2–4 compare reproduction high-class men to average women. This doubles the error of not including illegitimate offspring because they are not included in the number of children reported for high-status men but are counted as the offspring of someone else (i.e. the mates of average women).

4. The use of current income as a measure of wealth. Individuals may attain high socioeconomic class as a result of (involuntarily or voluntarily) limiting reproduction. In many cases it is unclear whether mating is delayed *in order to* further

education and occupational standing, or whether individuals continue to pursue such training intensively *because* they have been unable to find a suitable mate or to reproduce. If such a strategy were voluntarily chosen, one might expect the status gains to be evident only in later generations. High socioeconomic status may be acquired late in life and thus be unlikely to correlate with lifetime fertility; but it may correlate with long-term reproductive success if the reduced fertility in the upwardly mobile generation is made up by higher reproductive output in subsequent generations. In support of this suggestion, Essock-Vitale (1984) found that women who inherited their wealth have more children than those who acquire it in their lifetimes. In addition, anticipated future increases in income may lead to delayed reproduction until a more opportune time. Rindfuss and Sweet (1977, pp. 78–82) present data showing that modern Americans time reproduction in relation to positive changes in their economic status and that *current* income positively correlates with *recent* fertility.

5. Long- versus short-term measures of fitness. Single generation measures of fitness ignore the possible long-term fitness advantages associated with wealth. One possible long-term advantage may occur in the context of a changing socioeconomic environment. If during periods of war and intense economic crises the poor suffer sufficiently higher mortality than the rich, small fitness advantages to the poor which occur at times like the present may be overturned during harsher times (consider the great wars of the 20th century and current and impending food shortages in the world, for example).[3] Also, as the data presented here suggest, the wealthy produce more children than the poor during times of expanding economic opportunities and low job competition. Thus, it is important to determine whether fitness differentials measured in one generation are maintained or reversed over time. Using, for example, geneological data from cohorts born in early 19th-century England and following their genetic representation to their present descendants, such a test should be possible.

The demographic transition and a dual investment strategy for the wealthy. On the basis of these suggestions, we offer the following hypothesis to account for the pattern of reproductive behavior found in modern industrial society: The great demographic transition beginning in the 18th and 19th centuries in Europe is the result of increasing competition for resources and increasing costs of raising viable children (i.e., children who will themselves mate and raise viable children). In industrial settings, training, mainly in the form of formal and informal education, is the key to attaining high socioeconomic position. During periods of high competition (resources in short supply, contracting job market), it is more difficult for parents to insure that their children will achieve a social status as high as or higher than the one they themselves achieved, and the higher the social status, the more costly it is to remain there. Thus wealthier people curtail their legitimate reproductive output to maintain their social position over time and achieve higher fitness through increased extramarital mating success in the male line. The wealthy female would thus achieve higher fitness not through her own reproductive output but through her sons. In times of low competition for high-status employment (e.g. the baby boom period), the cost of raising children decreases. All people therefore increase reproductive output, but wealthy people can afford a disproportionately greater increase in their number of legitimate children.

This dual investment strategy for wealthy people would lower potential reproduction in the short run, by by maintaining social status over many generations, it would produce small fitness advantages which would increase exponentially over time. According to this model, poor people face a different set of fitness trade-offs. Because raising low-status offspring is considerably cheaper than raising high-status offspring (living in a poorer neighborhood with a lower tax base, no college expenses, no foreign travel, no private tutors, etc. – see Espenshade 1984), poor people face the choice of raising very few high-status offspring versus raising several more lower-status offspring. It may be difficult, however, to greatly increase a child's status in a low-class parental environment. Because poor people are generally less educated and trained in the norms of high-status behavior, it is more difficult for them to provide the necessary background for their children to achieve large gains in upward mobility. Given these trade-offs, the optimal reproductive strategy for the poor is to produce, through their marriages, more children in which they invest less. According to this hypothesis, the phenotypic differences in fertility between classes are not due to genotypic variation, but rather to the different conditions in which they live. Although not critical to its predictions, the model assumes that selection has favored genotypic monomorphism with respect to a decision mechanism which generates different reproductive responses under varying circumstances.

Changed conditions and departures from fitness maximization. If carefully designed future tests still show an inverse correlation between socioeconomic class and fitness, however, we must then consider the possibility that for some populations of modern humans, there is neither a short- nor long-term fitness advantage associated with wealth and status. We wish to suggest two specific reasons why this might be so. Both are based on the possibility that the environmental context in which humans currently find themselves has changed radically enough that behaviors which have generally been favored by natural selection in most human contexts no longer maximize fitness in modern societies. The first is that mortality rates have dropped dramatically in the last century. Although there is still a negative relationship between class and mortality rates by age, the overall drop in infant and reproductive age mortality has decreased the effect of wealth on fitness. When infant mortality rates were as high as 30%–50% (Shorter 1975), for example, differences in offspring survival would have conferred a much higher fitness advantage with wealth. It is therefore possible that the effects of changing selection coefficients on alternative reproductive strategies that have emerged as a result of changing mortality curves have not yet produced a measurable change in modern phenotypes but will do so in the future.

A second changed condition is that with contraception, women can now cheaply and efficiently control their fertility. As products of mammalian evolution (male paternal investment is characteristically low in mammals and males are often not present when their offspring are born) human males may be designed to increase their fitness through a desire for sex rather than a desire for children per se. If the proximate mechanism of male fitness is a desire for sex and if females are using contraceptives when engaging in extramarital sex, then it may be that wealthy males are using their higher social status to gain greater sexual access without gaining an associated fitness advantage. This proximate mechanism is especially likely in humans where estrous is hidden. Thus, until selection changes male sexual strategies, we may observe the putative negative correlation between wealth and fitness.

In conclusion, we applaud Vining's attempt to address "head on" the empirical status of a central theoretical prediction of evolutionary biology or behavioral ecology (terms we prefer to "sociobiology"). However, because of the differences between male and female reproductive strategies, there is good reason to believe that the data he presents do not adequately test whether wealth and reproductive success are positively correlated in modern industrial settings. We do not want to create conditions that are so difficult as to make the hypothesis nonfalsifiable, but we feel that it is premature to accept several poor tests of the hypothesis as equivalent to one good test. We hope that the target article will encourage more empirical work which tests for fitness differentials among males, which distinguishes newly acquired wealth from inherited wealth, and which measures fitness differentials over many generations.

NOTES

1. We consider the relationship between IQ and fitness to be irrelevant to this debate. Since wealth, education, and social position in modern industrial societies are not only determined by intellectual ability but also by the wealth and status of parents, personality, and social skills, etc., it is not clear whether intelligence should be positively associated with fitness. We will also not comment on the applied sociobiology section of the paper. The question of whether wealth and intelligence are positively associated with reproductive success in modern populations is a scientific issue, but what to do with that information is a public policy problem which we are no more qualified to discuss than the average concerned citizen.

2. Table 24 of the Census of England and Wales cited by Vining does attempt to measure this but gives equivocal results because percentage of men never married by social class cannot be calculated from the data, and because the inability to find a suitable mate early in life is more likely to lead to a male's attainment of middle class. The fact that few men in the highest social class are younger than 35 highlights this problem. In addition, the authors of that volume discuss the problem that the measure of class that they use does not appear to correlate well with wealth (pp. 76–77). This difficulty applies to the data Vining presents in Table 1 as well.

3. Unemployment disproportionately increases among the less educated during economic recessions (Young 1985).

The trouble with human sociobiology is . . .

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Sociobiology, construed as a discipline that seeks to identify the evolutionary explanations of animal social behavior, takes inspiration from sophisticated models that allow us to see adaptive advantages in apparently maladaptive behavior. A sociobiologist of overwhelming vulgarity might conclude that *all* aspects of the behavior of *all* animal species (including our own) are actually fitness-maximizing. Vining's data on social and reproductive success give the vulgar their comeuppance. What is the impact on the more refined practitioners who inhabit the actual world?

Nobody ought to deny that animal behavioral phenotypes, including our own behavioral phenotypes, should be explicable in ways that ultimately involve evolution. But this leaves numerous possibilities for historical explanation. Let's pretend, to simplify matters, that all evolutionary explanations are selectionist explanations, that is, that we explain the presence of genes, developmental programs, proximate mechanisms, or whatever by specifying the benefits that these entities brought to individuals possessing them in ancestral environments. Then the simplest form of evolutionary explanation of a behavioral trait is to propose that there is an allele which, in the homozygous condition, in the typical environments encountered by the animals, produces a behavioral disposition that increases the reproductive success of those animals that have it, relative to those that do not. A quite different form of evolutionary explanation is to suggest that the behavioral disposition is the product of independent behavioral mechanisms (more elementary dispositions) that develop in animals with certain genotypes in certain environments, that these genotypes were favored in ancestral populations because, in the ancestral environments, the mechanisms that developed in animals possessing these genotypes combined to yield dispositions that brought certain advantages. Once we recognize that there are unspecifiably many ways in which the links between genes, proximate mechanisms, developmental environments, resultant adult dispositions, and selective environments may be made, then it will seem absurd to think that an evolutionary explanation for a behavioral phenotype will reveal that that behavioral phenotype maximizes the inclusive fitness of its bearers. The absurdity will be magnified when we remember that selection is not the only evolutionary force.

Is there a historical explanation for the failure of social success to correlate with reproductive success? I suggest that, although there may be no single account that applies to all cases, we can anticipate a collection of variations on the theme just indicated. Perhaps one of these variants will be as simple as the explanation offered by Irons that Vining rejects, an explanation that identifies a proximate mechanism – the drive to suppress reproduction in the acquisition of status – that once led individuals possessing it to produce a few "high-quality" offspring whose contributions to subsequent generations would be greater than the larger broods of lower-status overproducers. But to spin such stories is an idle exercise. We presently lack any serious taxonomy of the many alternative forms of historical explanation, all of which appeal, somewhere down the line, but possibly a long way down the line, to our evolutionary heritage. Any such taxonomy should be constructed on the basis of a theory of gene-culture coevolution. That theory should articulate a general picture which everyone should find compelling.

According to this general picture, gene-culture coevolution can be conceived as a process consisting of a series of discrete stages. At the first stage, a population of individuals with particular genotypes, developing in particular biological and cultural environments, acquire certain phenotypes. Because of the characteristics of the environments, a certain distribution of matings takes place. From these matings the gene pool of the next generation is produced, and as a result of cultural transmission, there is an assignment of new biological and cultural environments to the offspring. The process repeats itself, with natural selection acting via differential mortality of prereproductives, differential abilities to mate (or to choose mates), and differential fertilities. Cultural selection acts via the differential transmission of cultural environments. Although the picture is simple and intuitively plausible, it is not hard to appreciate the fact that the entire process is sensitive to a large number of factors whose contributions we do not currently know how to assess.

The first step in tackling the question of why reproductive success relates to social success must be to have a clear view of the possibilities. The theory of gene-culture coevolution must achieve the same precise delineation of alternatives that Fisher, Wright, and Haldane brought to the genetics of the evolutionary process. Yet, even if we had an adequate account of the possible gene-culture coevolutionary scenarios, it is far from obvious that the complex of problems raised by Vining's data would be resolvable without vast amounts of research in areas in which we can currently only make guesses. It is quite possible that many aspects of human psychology and human culture are implicated in the proximate causation of decisions to abstain from reproduction. If so, then any successful application of a theory of gene-culture coevolution to the phenomenon Vining discusses must await detailed understanding of the developmental connections among numerous aspects of the human behavioral phenotype. When we consider how difficult it is to construct detailed evolutionary explanations of complex characteristics in nonhuman species in cases in which we do not have to worry about psychological connections or about cultural transmission, we can appreciate how far sociobiology is from explaining the phenomenon to which Vining draws attention.

I have been arguing that Vining reaches the right conclusion for the wrong reason. On his account, there is a fundamental sociobiological principle which is falsified by the data he assembles. I claim that this is the wrong way to look at sociobiology. Any canny sociobiologist should disavow any "fundamental principle" that Vining's data would falsify. Better to consider human sociobiology as a strategy for explaining human behavior. But, conceived in these terms, existing human sociobiology is little more than untutored speculation (with an unfortunate tendency to false advertising). The reason is that we lack any convincing account of the forms of historical explanation, or, to put it another way, any serious theory of gene-culture coevolu-

tion. Moreover, even if we had any such theory, the problem to which Vining draws our attention would hardly be the analogue of industrial melanism, cowbird-oropendula mutualism, chromosomal inversions in *Drosophila*, sex-ratio allocation, or any other of the great examples on which contemporary evolutionary theory prides itself. Thus it seems simultaneously unfair and overgenerous to chide human sociobiology for this particular failure.

In suggesting that we have no adequate account of gene-culture coevolution, I depart from Vining's assessment of the work of Lumsden and Wilson (1981a). Vining claims that Lumsden and Wilson cannot explain the data he cites, but he seems happy with the general theoretical machinery that Lumsden and Wilson have constructed. Unless I misinterpret Lumsden and Wilson, their enterprise does not lend itself to any immediate conclusions about expected correlations between reproductive and social success. Instead, they offer general models of the coevolutionary process. What is deeply wrong with the theory they present is that it provides no adequate representation of genetics and development, no adequate representation of mind and behavior, and no adequate representation of culture. Results are generated on the basis of unmotivated simplifications, with cunning juggling of the numbers so as to reach the kinds of conclusions that the authors want. (To cite just one example, the form of the reward equation [Lumsden & Wilson 1981a] is carefully chosen so that certain fitness ratios can be made extraordinarily high; see Maynard Smith and Warren, 1982, and Kitcher, 1985, chapter 10). [See also multiple book review, *BBS* 5(1) 1982.]

But there are reasons for optimism. Other writers have done better than Lumsden and Wilson. For example, the theory of cultural transmission developed by Cavalli-Sforza and Feldman (1981) and the approach to the gene-culture coevolutionary process outlined by Boyd and Richerson (1985) avoid many of the pitfalls and may provide a basis for more sophisticated accounts of how evolution leaves its mark on contemporary human behavior. By lumping together discussions of gene-culture coevolution, Vining fails to see that the fundamental *theoretical* problem of human sociobiology – the problem of identifying the space of possibilities for gene-culture coevolution – is being pursued with different degrees of sophistication and success by different groups of people.

One final puzzle. Convinced that his data show the failure of sociobiology as a theoretical discipline, Vining is nevertheless prepared to find room for it as a practical guide. Waiving the ethical issues that arise in connection with eugenics (even positive eugenics) it seems legitimate to wonder how a theoretical failure is to offer advice on issues that may have profound effects on human well-being. I claim that the Lumsden–Wilson theory of gene-culture coevolution is theoretically flawed; Vining takes it to be falsified. Either way, we have to ask whether it can be trusted to point out the consequences of not intervening to alter the inverse correlation between reproductive and social success. By what right do we appeal to so dubious an authority to pronounce the inevitable Decline of the West?

Proletarian hominids on the rampage

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Despite the impressive demographic and sociological data on which he builds his case, I must disagree with Vining's claim that the negative correlation between cultural and reproductive success is "the central theoretical problem of human sociobiology."

Vining provides few data on evolutionary fitness. Fertility is not equivalent to fitness, because other components, such as viability, are ignored. Completed fertility is a good measure of reproductive success but not always an accurate index of relative fitness. That is, the failure to compare corresponding points in the life cycle of different generations can lead to erroneous conclusions about the operation of natural selection (Lewontin & Cockerham 1959; Wallace 1958). Moreover, because it is not specified whether there is gene flow between socioeconomic classes, it is unclear how relative fitness is to be calculated: Who is competing with whom? The course of evolution within Vining's "modern" stratified population remains indeterminate.

Why Vining believes that IQ is a natural index of social status, cultural success, and thus "endowment" is also unclear. The use of IQ tests to measure "intelligence" is unfortunate. If the heritability of IQ is as large and as uncorrelated with high fitness as Vining claims, then IQ measures a cognitive ability to which selection has been essentially indifferent during the course of human evolution (Feldman & Lewontin 1975; Fisher 1958). Vining mentions this problem but certainly does not dwell on its fatal consequences for his arguments. Rather than capitulating the entire IQ controversy, I will simply add my voice to those who do not find IQ an evolutionarily salient phenotype but who nonetheless accept that it may have some limited utility for diagnosing severe cognitive disability (its original function), diverse extraordinary talent, or sufficient ability for some middle-class occupations. Other than identifying cognitive outliers, this culturally restricted test offers little to the study of human behavioral evolution.

Vining notes that a comparable analysis of Japanese demography and sociology does not seem to substantiate a negative correlation between cultural and reproductive success. Are there other "modern" societies that also do not reveal this negative association? Given that "modern" society is broadly defined as "urban or urbanizing societies of the present and recent past" (note 1), it would be nice to know which proximate factors are supposed to underlie the negative association. In fact, the data are restricted to very recent, industrialized, urban, stratified, technologically advanced, large, primarily Western, and mostly white populations. A number of Asian, Indian, and Middle Eastern societies have been stratified and urbanized for millenia. In some of these groups there is positive correlation between cultural and reproductive success (e.g. Dickemann 1979; Hill 1984b). Is the negative association between cultural and reproductive success a consequence of the demographic transition (or, more proximately, medical technology) or is it uniquely a result of Western attitudes toward the self and the family? Or, to play devil's advocate, might selection have had time to eliminate the less fit but socially successful members of non-Western societies? It is important to sample a wider range of "modern" societies before it is claimed that contemporary humans do not conform to the "fundamental postulate of sociobiology."

But even granted the validity of Vining's contention that cultural and reproductive success are negatively correlated in "modern" societies, how is this pattern to be explained? It might be useful first to put this in evolutionary perspective. For about $\frac{1}{10}$ of the time (300 out of 300,000 years), maybe 10% (10^9 out of 10^{10}) of the humans who have ever lived have been "modern" and thus potentially at "risk" of violating sociobiological dogma about the positive correlation between resource competition and fitness. In other words, for the vast majority of time that they have been on this planet, the vast majority of humans may well have maximized fitness by means of socially mediated resource competition. From the perspective of the human genome, the "modern" social environment is indeed "novel," consequently it may be that the adaptability of humans has been finally overtaxed. Vining, however, is rather unhappy about the possibility that even a minority of humanity behave maladaptively or that the evolutionary novelty of "modern" society could explain this. He would like an answer to the not unreasonable question, "why the release from premodern circumstances cause non-fitness-maximizing behavior?" (Section 3).

A number of possible answers can be found in the evolutionary biology literature. Such reproductive behavior might be either a maladaptive pattern, that is, a susceptibility of the human genome's reaction norm to breakdown, or an adaptive pattern, that is, a better response to the ("modern") environment. There are at least two maladaptation hypotheses. Human egoism breeds a consciousness in which phenotypic interests come to outweigh genotypic interests (e.g. Alexander 1980; Barkow & Burley 1980; discussed by Vining). On the other hand, one can postulate that the positive association between prestige and reproductive success found in most human societies may become decoupled due to the accumulation of surplus wealth. Moreover, the application of the law of diminishing returns to reproductive effort would facilitate this decoupling (Barkow 1975; Hill 1984b). I find Vining's arguments about these maladaptation hypotheses somewhat confusing. He states that the negative correlation between cultural and reproductive success is devastating for sociobiology, but then uses the sociobiological logic of the egoism hypothesis to explain it.

The life-history literature, particularly as applied to avian clutch reduction, provides a number of adaptive hypotheses, some of which may be pertinent to the human case. The "bet-hedging hypothesis" predicts fertility reduction in environments where the survival of immature members of the population is unpredictable (e.g. Schaffer 1974; Stearns 1976). The "reproductive effort reallocation hypothesis" predicts that fertility may be reduced in order to allow an increase in other forms of parental effort so that the immature are more competitive (e.g. Brockelman 1975; Kurland & Gaulin 1984; Smith & Fretwell 1974). The "most productive reproductive rate hypothesis" predicts that limited parental effort selects against very high fertility. Very low fertility, though leading to high juvenile survival, usually produces low relative fitness. The result is therefore stabilizing selection for intermediate levels of fertility and an adaptive reproductive rate often well below the maximum (e.g. Lack 1966). Finally, the "parental investment hypothesis" predicts that fertility may be reduced if the costs of parental effort relative to reproduction increase (e.g. Goodman 1974; Williams 1966).

So far as I am aware, these hypotheses have not been rigorously tested in any modern human population. However, some of the economic and sociological data Vining reviews seem at least consistent with several. In summary, despite Vining's claim, it is still an open question whether human fertility reduction is adaptive, maladaptive, or nonadaptive.

If I have failed to undermine Vining's claim that those of high status are acting maladaptively, then at least I must point out that those of low status *are* acting adaptively, or so their relatively high fertility implies. If, on average, high genetic fitness is a reasonably good sign of a better phenotype, then the task becomes one of unpacking the behavioral strategies that allow those of lower socioeconomic status to adapt better to the vagaries and novelty of modern society. As both the subject and instigator of human sociobiology, we can too easily confuse "success" as it is culturally constructed with biological success as it is registered in evolution. There are some data (cited by Vining) to suggest that cultural success in traditional societies does correlate with reproductive success. But maybe our cultural perceptions mislead us when applied to the sociobiology of our own modern society. Perhaps the sociologist's description of human behavior is not at the correct level of analysis. Could an academic, middle-class bias falsely lead us to the conviction that what we find desirable ought also to be evolutionarily adaptive? Perhaps the proletarians are not "better endowed," but they may be better adapted. The question might rather be: Given that there is variance in fitness, what traits are correlated with a reproductive or survival advantage?

This confusion between cultural success and evolutionary adaptation leads Vining on a bizarre excursion into the ozone of eugenics. A subset of evolutionary theory, sociobiology is simply the study of the natural history of sociality. Perhaps, as Vining points out, it can specify some of the givens of the human condition, but like much of evolutionary biology it cannot be expected to make long-term predictions or short-term value judgments. Its applications are real, but limited (e.g. Daly & Wilson 1982; Lightcap, Kurland & Burgess 1982). Indeed, most sociobiologists are preoccupied with the more mundane but perhaps more reasonable task of identifying the adaptive function of restricted domains of behavior (e.g. sex-ratio or gregariousness).

To saddle sociobiology with a eugenics program seems absurd. Does it arise from some fear of an onslaught by the intellectually handicapped, culturally unendowed, marching morons who comprise the proletariat? Eugenics is impractical given the slowness of its supposed salutary effect, a point only mentioned in Vining's last paragraph. More importantly, eugenics is morally repugnant and pernicious when it is assumed that some of us know which human phenotypes are most desirable for everyone else. Vining's "better endowed" may or may not possess desirable traits. Long-standing ethical and political disagreements preclude an obvious answer. The recent history of "modern" Europe makes clear the horrific abuses of eugenics. It is peculiar that throughout this paper Vining assumes that his culture of "modern" society is of such obvious and lasting value that it is human nature that must be genetically tailored to that status quo. Human culture as well as the human genome can change or, more to the point, be changed. Vining's apparent concern to transform sociobiology into eugenics and to require eugenics for the maintenance of "modern culture and its current path of evolution" seems to rest largely on misinterpretation and misunderstanding.

Further evidence for secular increases in intelligence in Britain, Japan, and the United States

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We found ourselves substantially in agreement with the second half of Vining's target article but less persuaded by the initial sociobiological arguments. The paper begins by casting doubts first on whether *Homo sapiens* has a biologically programmed drive to produce children and second on whether the strength of this drive (if it exists) is positively associated with rank (and intelligence). The paper does not note the useful data on these questions derived from social surveys of ideal family size. The results of surveys of this kind show that around 98% of the population of economically advanced nations would like to have children. Typical mean values for ideal family size found in these surveys range from 2.3 (West Germany) through 3.4 (United States) to 4.7 (Ireland) (Wilson-Davis 1980). We consider that these data suggest that Vining has exaggerated the reluctance of the contemporary human female to bear children. We conclude that the prediction from sociobiological theory that human beings will want children is substantiated by numerous studies of this kind.

It is true that the mean ideal family size is of the order of 2.3 to 4.1 and not, say 30–40, as some readings of sociobiological theory might require. However, Wynne-Edwards (1962) showed that many species do not strive to optimise their inclusive fitness in the immediate term by having vast number of offspring. Wynne-Edwards argued that to do so would exhaust the food supply and ultimately threaten the population's own survival, and for this reason many species have evolved control mechanisms to keep their populations broadly constant. These mechanisms are apparently working quite well among human populations in economically advanced nations.

Turning now to the inverse association between rank (to-

gether with intelligence) and fertility which has tended to be present in most if not all advanced nations for much of the present century, we agree that this appears to be a breakdown of sociobiological mechanisms which normally ensure that this relationship is positive. The new data that Vining has assembled on this point represent one of the most important contributions of the paper. We are convinced by his demonstration that the small positive association between IQ and fertility found by Bajema (1963) for cohorts born between the two world wars has turned negative in subsequent cohorts. We agree that the most reasonable inference from the more recent data is that genotypic values for intelligence are declining.

We also agree that the decline of genotypic intelligence is being masked by the rise of phenotypic intelligence (due to improvements of various kinds in environmental conditions). In support of this proposition Vining cites data for Japan and the United States (Flynna 1984; Lynn 1982). However, he expresses some reservations about the possibility of measurement errors in these data, and it may therefore be of interest to report that we have recently collected further data from Japan and also from Britain on the rise of national intelligence over the last half century or so. The conclusion based on four studies in Japan from 1950 onwards is that the mean Japanese IQ has been increasing at a rate of 7.7 IQ points per decade, over double the 3.0 IQ points per decade calculated by Flynn for the United States. In Britain, calculations based on 11 studies over the period 1932–1982 show that the mean IQ has been increasing at a rate of 1.7 IQ points per decade (Lynn & Hampson, in press). Attention is also drawn to a little-known study in New Zealand over the period 1936–1968 showing a gain of 2.2 IQ points per decade among New Zealand school children (Elley 1969). It therefore seems that there is now quite widespread international evidence showing that mean population IQs have been increasing over the last half century and moreover that they have been increasing at a far faster rate than was formerly considered probable. These results reveal a very different situation from the gloomy prognostications of the 1930s and 1940s that national phenotypic intelligence in advanced societies might actually be declining (e.g. Cattell 1937), although these did not seem unreasonable at the time on the basis of the existing evidence.

Nevertheless, Vining suggests that all may not be so well as the rapid rise of intelligence appears to indicate. He proposes that a diminishing returns effect for environmental inputs should be anticipated and in due course the rising IQ curve must be expected to flatten out. There is in fact already some evidence that these expectations are correct. First, for the United States Flynn could find no evidence for any falloff in the rate of IQ gain over the period 1932–1978. But if his figure of a 3 IQ point-per-decade gain for this period is compared with Tuddenham's (1948) data of a gain of 5.0 IQ points per decade for the period of approximately 1916–1942, based on conscripted men in World Wars I and II, then a deceleration of the rate of gain has evidently taken place. Second, in our calculations for Japan we find a rate of gain of 10.6 IQ points per decade in the immediate post World War II years decelerating to approximately 5.0 IQ points per decade for the period after around 1960. Third, in our studies of the rise of mean IQ in Britain over the period 1932–1982 we have found that the rise in the lower half of the intelligence distribution has been about double that in the upper half. Our interpretation of this is that among the upper half of the distribution environmental conditions in the 1930s were closer to their optimum and hence diminishing returns have set in more quickly. These three lines of data evidently confirm Vining's view that secular increases in mean IQ cannot be expected indefinitely. Indeed, there is already evidence of diminishing returns. Extrapolation of this trend should lead first to stabilisation of mean population IQs and ultimately to decline. We agree with the author that intelligence is an important quality for all human societies and certainly for the advanced Western nations, and therefore that the possibility of genetic deterioration is a matter of social importance and merits more attention than it has received in recent years.

Fertility, intelligence, and socioeconomic status: No cause for surprise or alarm

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Although there is little reason for confidence in what will be the final steady state in industrial societies of the relationship between fertility and socioeconomic status, I shall assume in my comments that the substantive empirical claims made by Vining are justified, and shall concentrate on their implications, moving from general to more specific issues.

Are these findings surprising, and do they pose a real problem for sociobiology? Given that modern men and women wish to limit the number of their offspring, the results are hardly surprising: The well-informed and the affluent are inevitably going to be more efficient in the use of techniques of birth control, and best able to afford whatever techniques they prefer. So, even if the rich and the poor aimed at similar sizes of family, their efficiency in attaining their goals would be expected to differ. This does, however, assume the goal of limiting family size, and it is this, surely, rather than any difference between rich and poor, which may pose a difficulty for sociobiology. Why should people seek to limit the number of their offspring?

There are, of course, many answers to this question, some of which are given by Vining, and it is possible even likely that many of the factors involved vary in their effect between rich and poor. There may, then, be a further reason for the differential fertility observed, namely, that the rich set lower targets for family size. Whether this is of any real interest to sociobiology, which I doubt, depends on answers to the basic question, How can sociobiology explain voluntary birth control in humans?

Humans actively seek to limit family size and nonhuman animals do not because nonhumans have no concept of family size: Nonhumans do not have a drive to procreate – they have a set of drive systems, such as those for defending territory, attracting a mate, building a nest, copulating, rearing the young, which have as their consequence procreation. Those proximal drives and their associated behaviors evolved, and were selected for, because of that distal consequence, precisely as sociobiologists argue. Humans are endowed genetically with analogous drives, but, as a result of our technology, we can behave so as to satisfy those drives (in particular, the drive for copulation) without allowing their "natural" consequence. So, there is no sensible noncircular answer to questions relating to innate drive systems, such as: Why do you want to eat? Why do you want to sleep? Why do you want to copulate? But it is sensible to ask the question, Why do you want (or not want) to have a child (or children). There does not, then, appear to be a problem for sociobiological theory: Insofar as sociobiology can "explain" any nonbasic human drive, such as the urge to gain status or amass wealth, it can explain why some humans might choose to limit family size. Indeed, it can be seen that the difficulty may lie in the opposite direction, in trying to find an answer to the question: Why do the great majority of humans decide that they do want children?

A more specific issue concerns the notion that differential fertility according to socioeconomic status may be detrimental to the population gene pool. Vining reports that people with high IQ scores tend to have lower fertility than people with lower scores; it is reasonable to suppose that this is a consequence of the relationship between socioeconomic status and fertility. Vining asserts that academic intelligence has high heritability, and that there is a considerable consensus among

behavioral geneticists and psychologists that this is so. I do not think it necessary to raise the long-standing dispute about the heritability of intelligence, because what is relevant here is that all parties agree that the environment does contribute substantially to variations in IQ scores – Vining himself reports much evidence on precisely this issue. This being so, what reason is there to suppose that the higher IQ scores of the rich reflect anything more than the enriched environment that they enjoy? Why should we go on to suppose that they also possess higher "innate" intelligence? The possibility that some part of the variation among normal human beings in IQ scores is due to genetic differences related specifically to mental capacity – a possibility which I see no need to accept (Macphail 1985) – does not provide any grounds for inferring a difference in innate capacity between two populations having different IQ scores but occupying different environments, particularly when the population with the lower score is exposed to an environment known to be disadvantageous for performance in intelligence tests. There is no reason, therefore, to suppose that reduced fertility in the rich will reduce the mean intellectual capacity of the population.

I come finally to the case made by Vining for the particular importance of high intelligence in industrial societies. That argument I find merely rhetorical, with little appeal to evidence. Vining argues, for example, that increases in brain size are correlated with increases in intelligence; this claim, which is largely irrelevant, is introduced to bolster the notion that mental capacity has increased through genetic change. There are, however, no data for any species, including man, which suggest that intraspecific variations in intelligence are correlated with brain size. Moreover, for vertebrates in general, there is no convincing evidence of any systematic relationship between relative brain size of various species and intelligence (Macphail 1982). Although man does, according to most of the allometric formulae adopted, possess the largest relative brain size of any vertebrate, it is not the case that our large brain must be interpreted as due to (or the cause of) increased intelligence. Man also possesses language, and has highly developed perceptual and motor faculties, all of which would demand increases in brain size and may be quite independent of general intelligence. There is no evidence that modern man, and in particular modern man in industrial societies, shows any recent increase (or decrease) in mental capacity. Given Vining's faith in brain size, it is perhaps worth concluding with the observation that the brain of Neanderthal man appears to have been somewhat larger than that of modern man (Washburn 1978).

The "eugenic dilemma" revisited

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Vining has clearly restated what in the past, with less critical data, has frequently been termed "the eugenic dilemma," that is, the lower fertility in the recent past and present of those whom society judges by one criterion or the other to be the more successful. The genetic implications of those fertility differentials remain unclear and debatable. However, it seems safe to generalize that nowadays dummies seldom become business entrepreneurs or successful professional types. Beyond this, a precise quantitative treatment is difficult if not impossible. I look forward to the interesting discussion this ambiguity should create.

As this exchange progresses, we need to sharpen the use of the word "endowment," as it appears in Vining's second paragraph. Until this is done, we lack the precision necessary for an important discussion such as this. For instance, if endowment is defined by IQ tests, then our arguments cannot be precise until we can remove the cultural contribution to IQ as usually measured. One of my frustrations in our studies on the Yanomamo, frequently cited by Vining, is that whereas I judge the reproductively more successful headmen to be innately better "endowed," I have been unable to put that judgment on the necessary objective basis.

Demography and sociobiology

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The theme of Vining's informative and thought-provoking target article is that the modern pattern of negative differential reproduction by social status results in a divergence between cultural and biological evolution that is inconsistent with the basic postulate of sociobiology. According to this basic postulate, individuals maximize reproductive fitness.

This postulate does not seem to me to accurately portray human populations, even when reproduction and status are positively associated. Fitness maximization seems inconsistent with demography's concept of natural fertility, which is fertility in the absence of conscious family limitation (Henry 1961). There are many mechanisms, such as prolonged breastfeeding (which lengthens birth intervals) and negative population pressure feedback on age at marriage, that tend to keep natural fertility well below the biological maximum. Moreover, natural fertility varies widely among premodern populations, whereas the biological maximum, which is much higher, varies comparatively little (Bongaarts 1978; 1983; Henry 1961). Fitness maximization seems to be the exception rather than the rule in human populations.

Most demographers would say that individuals or couples tend to maximize utility rather than reproduction. Reproduction is optimized, not maximized, in the process of utility maximization. Utility maximization involves an allocation of scarce resources among competing goods, only one of which is having children. Indeed, the notion of competing goods is essential for understanding the process of fertility transition and for resolving the problem posed by Vining, because the increasing importance of competing goods provides part of the explanation of why fertility declines with modernization and why fertility differentials by social status shift from positive to negative.

It seems to me that a first step toward resolving the theoretical problem posed by Vining is to reformulate human sociobiology's basic postulate as fitness optimization within a broader process of utility maximization. Before demographic transition, utility maximization tends to produce a positive association between status and reproduction. After demographic transition, the reverse tends to be true. A fairly adequate explanation of this shift, couched in the language of utility maximization, can be found in the theories of transition developed by demographers (see, for example, Easterlin 1975; Freedman 1979; Retherford 1985).

In other parts of his paper, Vining examines the evidence on status and reproduction in premodern and modern populations. He concludes that status and reproduction tend to be positively associated in premodern populations and negatively associated in modern populations. But his citation of evidence in support of these conclusions is neither systematic nor comprehensive. In fairness, it must be acknowledged that a thorough review of this evidence would be a major in itself. Still, some partial reviews of evidence (in addition to Wrong's 1956 review, which was republished in 1980 and which Vining mentions) have been published, and they might have been cited. Cochrane (1979) has reviewed a large amount of evidence on the relationship between fertility and education in developing countries. Mueller and Short (1983) have similarly reviewed a sizeable data set

concerning the relationship between fertility and income and wealth. Both reviews tend to support Vining's conclusion that fertility differentials by status reverse with demographic transition. Unfortunately, many of the studies cited by Cochrane and by Mueller and Short control for other socioeconomic variables and do not present the simple two-way relationships without controls (except age, which should be controlled) that are of interest here. A thorough review of evidence on the simple two-way relationship between fertility and status remains to be done. Incidentally, Vining suggests that Japan may be an exception to the rule that fertility and status are negatively associated in modern societies; some very recent evidence indicates that Japan is not an exception (Kawasaki 1985).

Most of Vining's discussion of evidence aims to show that the positive association between fertility and IQ for some U.S. birth cohorts was confined to a brief period of rising fertility associated with the post-World War II baby boom. A more careful critique of this evidence, some of which is rather weak, would have been useful. Very little of the evidence of a positive relationship is based on nationally representative samples. Van Court and Bean's results, at least, are based on a series of national surveys, but they may be biased by high nonresponse rates, because persons of low IQ are undoubtedly disproportionately represented among nonrespondents: these results show that the association gets as high as zero but never becomes positive. Vining's own findings of substantially negative differential fertility by IQ after the baby boom, also based on a national survey, are likewise plagued by high nonresponse rates; moreover, his results may be biased by the fact that the fertility experience of his respondents, who were rather young, was far from complete at the time of the survey, and by the fact that he substituted SAT scores and grade point averages for IQ scores when IQ data were lacking. The *Who's Who* and similar evidence cited by Vining is weakened by the fact that the fertility of men is usually compared with the fertility of women. The difficulty here is that, in modern populations, differential fertility by status tends to be considerably more negative for women than for men (see Cochrane 1979, for evidence on this point relating to fertility differentials by education). There is no question that negative differential fertility tended to narrow during the baby boom and widen again during the baby bust, but it is uncertain whether the relationship between fertility and IQ in the United States as a whole has ever been positive in this century.

A few tabulations of fertility by education complement Vining's tabulations of fertility by IQ. The implicit assumption is that education is positively associated with intelligence. Evidence in support of this assumption could have been cited and would have strengthened the paper. For example, in a follow-up study of high-school graduates in the state of Wisconsin, the correlation between 11th-grade IQ score and subsequent educational attainment was about 0.5 (Sewell, Haller & Ohlendorf 1970).

Vining's target article implicitly raises the more general question of what sociobiology and demography have to offer each other. A great deal, in my judgment. Sociobiologists, as Vining has pointed out, have paid scant attention to the modern pattern of negative differential fertility by social status. Demographers have analyzed this pattern in great detail, but they have tended in recent decades to ignore the evolutionary implications of their work. The two disciplines need to get together.

What is sociobiology's central dogma?

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There is much to like in Vining's target article. His analysis of the inverse relationship between social success (among and within nations) and reproductive success in "modern" societies is a valuable addition to the debate on human sociobiology. Some sociobiological writings are contradictory on precisely this point. Some sociobiologists stress the seamless continuity of life from amoebae through humans – the social behavior of *all* species responding constantly, universally, and exclusively to the same biological determinants – only to dig a new "Cartesian gulf" at the demographic transition. This gulf divides all animal forms, including early humans, from *Homo sapiens* living in certain societies during the last 200 or so years. Vining's paper also demonstrates the problem that the acceptance of such a gulf creates for sociobiology's claim to be able to "biologize the social sciences" (Wilson 1975).

Also, Vining's discussion of the environmental and genetic codetermination of the population means for morphological and cognitive traits is an excellent presentation of this dynamic. Much of the ink spilled over human sociobiology in the last decade could have been saved if more commentators had had a clearer understanding of the process of codetermination. The necessity for the constant repetition of this point is indicated by the fact that the dynamic was described by Boas (1913; 1931) and the first generation of American academic anthropologists but has often been lost sight of by later social scientists.

Our admiration for Vining's work is tempered somewhat by two major reservations. First, Vining does not examine critically the implications of his analysis for more sophisticated applications of the principles of evolutionary biology to human behavior. Second, we wish he had analyzed more critically the all too common assumption that maintenance of our present culture and future increases in cultural complexity are dependent upon whatever traits are measured by IQ tests.

Vining's data obviously provide an ethnographic veto to any sociobiological explanation of human behavior that has as its "central dogma" that increased access to resources is always directly correlated with reproductive success. There is no doubt that some sociobiologists hold this simplistic view and that other sociobiological writings are often subject to such an interpretation. Vining has performed a valuable service in demonstrating that the version of this thesis holding that social class is directly correlated with reproductive success is not supported by the data from some industrial societies. However, we are struck by how frequently Vining criticizes this simplistic interpretation while acknowledging that a sociobiologist, especially Alexander (1980), Dawkins (1982), and Irons (1979; 1983) has already published a similar criticism. This creates suspicion that Vining is attacking a straw man and that many theoretically more sophisticated sociobiologists do not hold the simplistic position often attributed to them.

What is the central dogma of a more theoretically sophisticated sociobiology? Many of the texts cited by Vining suggest that sociobiology's key claim is that genetically based behavioral tendencies which lead to greater inclusive fitness will be selected for when in competition with alternative tendencies that result in lower inclusive fitness. Sophisticated sociobiologists seek to identify genetically based behavioral "rules of thumb" (or proximate mechanisms) that shape the social behaviors of a species. Such rules of thumb may not maximize inclusive fitness if the species exhibiting them inhabits an environment different from that in which the rules evolved. This is presumably why most sociobiologists presented with the data on class and fertility in postdemographic-transition societies advance a "novel environments" hypothesis. In his section on the sociobiology of nonmaximizing behavior Vining implicitly identifies the logic of "rules of thumb" arguments, but he does not discuss the implications of these arguments for his position that sociobiology cannot be a descriptive science of modern populations.

Vining argues that because the central dogma of simplistic sociobiology is violated by his data on class and reproductive success the theory has no value as a descriptive science of modern society. We anticipate that a more sophisticated sociobiology would want to defend its descriptive validity by

carrying out two empirical tasks. The first is to demonstrate how the behavioral rules of thumb selected in previous environments still operate in postdemographic-transition societies. Several studies cited by Vining attempt to provide such demonstrations and many others are available (Gray 1985). The validity of such rules of thumb and of their genetic basis is, of course, still subject to debate. Such studies are not designed to show that following these rules maximizes inclusive fitness in contemporary societies. Rather, they attempt to demonstrate that the existence of the rules can be explained by the logic of individual selection, kin selection, reciprocal altruism, or whichever aspect of evolutionary theory is hypothesized to be applicable to their evolution.

The second task of a more sophisticated sociobiology is to identify those features of postdemographic-transition societies that cause rules of thumb selected in humankind's earlier environment of evolutionary adaptation to result in lower inclusive fitness than alternative behavioral patterns. Vining's review demonstrates that, with few exceptions, sociobiologists have not addressed this second task. The review also indicates that the arguments produced by those theorists who have undertaken the task so far are not particularly convincing.

We are not arguing that a more sophisticated sociobiology has demonstrated conclusively its value in explaining human behavior; this is still an open question. What we wish to point out is that one aspect of Vining's target article makes it impossible for him to reject a sophisticated sociobiology's claim to be a descriptive science applicable to all human societies, including those that have experienced the demographic transition. This is his apparent acceptance of the argument that sociobiological principles are sufficient to explain patterns of rank and fertility in "premodern, economically primitive societies." If Vining holds this position – he seems to concede this high ground to the sociobiologists, despite his passing reference to premodern high cultures and his footnote reference to other social species in which a direct correlation between social and reproductive success appears to be absent – it is incumbent upon him to explain why the rules of thumb maximizing inclusive fitness in societies on one side of the "demographic gulf" no longer do so in the modern societies on the other side. The argument that cultural and environmental factors change more rapidly than gene frequencies is not a sufficient explanation of this situation. In fact, the slow rate of change for gene frequencies is to the advantage of the sociobiologists, who would not predict massive changes in geneticallybased rules of thumb in only 200 years, especially in large populations.

The preceding arguments suggest that if one accepts the applicability of sociobiological principles to "premodern" human populations one is forced to advance novel environment hypotheses to explain the lack of correlation between social and reproductive success in demographically modern societies. This further requires that one reject Vining's claim that sociobiology is not a descriptive science of modern populations. We do not have the space to detail reasons for questioning the applicability of sociobiological theory to small-scale, predemographic-transition societies, but we suggest that the evidence for sociobiology's validity in such societies is not as clear as Vining's article makes it appear (Gray 1985; Sahlins 1976; Silverberg 1980; Williams 1980).

Our second major reservation is that Vining does not examine critically enough the relationship between IQ and culture. He does question adequately the supposed correlation between high IQ and sociocultural success claimed by various sociobiologists. However, perhaps because he is an academic who resides in a technologically complex society, he leaves unexamined the more basic assumption that societies that promote skills relevant to academic success (IQ scores) will survive better than those that do not have the desire or ability to do so.

We believe Vining's target article would have been strengthened if he had devoted more space to the examination of some alternative assumptions. Among many others, these include the possibilities that: (1) the relationship between a society's "success" (however measured) and IQ or other academic skills is not particularly strong and therefore raising or lowering the IQ will not significantly affect cultural evolution, either in the short or the long run; (2) postdemographic-transition societies are still selecting for the same rules of thumb that led to reproductive success in earlier societies, but IQ is not one of these (perhaps IQ is an evolutionary epiphenomenon of selection for social skills?); or, (3) the probability that *Homo sapiens* have a future on the planet will not be increased significantly by social and/or genetic engineering designed to produce individuals with higher IQs as opposed to selection for other traits (greater sociability, lower aggressiveness, etc.). Vining's discussion of sociobiology as an applied science for postdemographic-transition societies (surely tongue-in cheek; e.g. how can a theory that cannot describe adequately a situation be used as an applied science in that situation?) illustrates the need for a complete discussion of these and other alternatives.

What is adaptive?

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Consider two families, the Smiths and the Johnsons. Coincidentally, both families live in the same neighborhood, both have the same family income of $40,000 a year, and both have the same parental IQs of 130 for each parent. In fact, the Smiths and the Johnsons are identical in every respect save one. The Smiths have decided to have two children, whereas the Johnsons have decided to have six. If one accepts the notion of reproductive success as the measure of adaptation, the Johnsons are to be congratulated, as they have produced three times as many children as the Smiths. In contrast, the Smiths are to be chided for limiting their offspring, thereby contributing to the greater average propagation by people of lower IQ.

Although things started out the same for the Smiths and the Johnsons in their respective marriages, circumstances begin to change after the birth of their respective families. The Smiths, having just two children, do not find themselves financially strapped. Mrs. Smith takes on a part-time job, but spends the bulk of her time bringing up her children, both working and playing with them. She talks to them, reads to them, and makes sure that they have a lot of educational toys and books. The Smiths have the money to travel, and so they take their two children to a variety of places, trying to make each trip as educational an experience as possible for the children. The Smith children are able to go to an excellent nursery school because their parents are able to afford such schooling, and are thus well-prepared when they start elementary school. The Smith children do well in school, and when they do have problems, their parents have plenty of time to devote to helping the children to resolve these problems. The children do well throughout their primary and secondary school years and eventually go on to excellent colleges, which the Smiths are able to afford because they have saved up over the years. Eventually, the Smith children go into the professions of law and college teaching, and soon after get married. They then repeat the cycle that started with the marriage of their parents.

Things go very differently in the Johnson household. Because there are six children, the initial family income proves inadequate to support the household. Mrs. Johnson finds herself having to take on a full-time job, and puts her children into daycare. The daycare center is a good one, although it cannot provide the individualized attention and instruction that Mrs. Johnson's neighbor, Mrs. Smith, can provide for her children. The Johnson children do not get the same level of exposure to books, educational toys, and trips that the Smith children get,

because there is neither the time nor the money for such luxuries. When the Johnson children start school, they are less well prepared than the Smith children. They start at a bit of a disadvantage, and if anything, this disadvantage grows. The Johnson parents, working very hard and strapped for money, do not have the time or resources to devote to each of their children that the Smith parents have. When the children need help, the Johnsons do the best they can, but they are strained to their limits, and simply do not function as effectively as they would like. It comes time for college, and it is clear to the Johnsons that they will have to send all of their children to the state university, which is not among the best state universities in the country. Although some of their children would be capable of admission to better schools, the Johnson parents simply were unable to save the money to support this better schooling, and because of cutbacks on loans and financial aid, the colleges would be unable to provide the level of financial support that the Johnsons would need to send their children to the better schools. The children all eventually go to the state college, and a few go on to further education. But the state college has not prepared them for the rigors of the best postgraduate programs, nor are the admissions officers of these programs quite as impressed with the state-college credentials as they are with the elite college credentials of the Smith children. Eventually, the Johnson children find employment in a diversity of occupations – police work, retail sales, insurance, and so on – and move into communities that are not quite of the socioeconomic level of the community in which they grew up. The Johnson children eventually all get married, and they, too, repeat the cycle.

The hypothetical example of the Smiths and the Johnsons, idealized though it may be, highlights the difficulties in defining adaptation solely in terms of reproductive fecundity. Indeed, few intelligent parents in contemporary society would equate reproductive fecundity with reproductive success. They know from observation of the world around them that larger families carry with them risks. These risks are likely to lead to lesser adaptive success for themselves and especially their children. Their notion of adaptation is very different from that of certain sociobiologists and even, apparently, that of some of their critics.

Psychologists studying intelligence often define it at least partially in terms of the ability of the organism to adapt to the environment. Their view of adaptation comes much closer to the lay view than does the one expressed in Vining's target article. Despite the equal IQs of the Smith and the Johnson parents, psychologists studying intelligence in context might argue that the Smiths have made the more adaptive and hence more intelligent decision in their family planning. If either family has attained reproductive success and adaptation to the environment, than certainly it is the Smiths. In defining *adaptation* narrowly, Vining reaches a conclusion directly opposite to the correct one. More intelligent parents are, in general, behaving adaptively in having smaller families, and any parent living in contemporary society knows why.

Sociobiology and Darwinism

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There is no well-defined field of "human sociobiology." Rather, there is a heterogeneous group of biologists, anthropologists, psychologists, philosophers, economists, and their social scientists who are united by their interest in an evolutionary perspective on human affairs but divided by their notions of just what this perspective imples. I shall refer to this group as "Darwinians."

The central Darwinian "dogma" is that natural selection is the creative evolutionary process responsible for adaptive design. Evolutionary biologists do not unanimously endorse the collection of data on reproductive differentials as an effective way to demonstrate adaptation, even in the study of nonhuman animals in natural environments (Williams 1966). As Hailman (1980) remarks, the "correlation of individual variation with reproductive success is a mental briar patch that scratches all who enter" (p. 189). Nor do Darwinians necessarily believe that human adaptation can be characterized in terms of "behavior." In their study of human infanticide in evolutionary perspective Daly and Wilson (1984) write: "Our emphasis on a psychological, rather than behavioristic, level of description is intentional and, indeed, essential. The specific act of infanticide may or may not benefit the actor's fitness, whether in an individual case or on average, but the act need not contribute to fitness for a sociobiological [i.e. adaptationist] analysis to be illuminating. Infanticide can be viewed as one (rare) manifestation of variations in more abstract motivational states such as child-specific parental love and solicitude. Adaptation may then be sought at the level of these more abstract states" (pp. 487–88). And, as Vining himself demonstrates, the vast majority of Darwinians expect human beings in nonnatural environments, such as the modern world, to systematically fail to pursue fitness. Few Darwinians have singled out the negative correlation between status and reproduction as a particularly crucial datum or tried to elucidate the "precise agent" responsible for this correlation because to most Darwinians it has no special theoretical significance: People in the modern world fail to maximize fitness in innumerable ways, and there are innumerable differences between modern and natural environments. (Surely a reasonably young, healthy male member of the *Forbes 400* could, if he really put his mind to it, sire at least one, and probably two, orders of magnitude more offspring than the average for this group.) There is no more reason to expect high-status people to outreproduce low-status people than there is to expect, say, heavy tobacco use to promote fitness.

The question of a Darwinian contribution to the social sciences cannot be settled by examining reproductive differentials. Darwinism, writes Lloyd (1979), "merely provides a guide and prevents certain kinds of errors, raises suspicions of certain explanations or observations, suggests lines of research to be followed, and provides a sound criterion for recognizing significant observations on natural phenomena" (p. 18).

To apply Lloyd's thinking to the social sciences, one must first acknowledge that all scientific students of human affairs assent explicitly or, more often, implicitly to the following propositions: (1) Every hypothesis about human affairs necessarily entails assumptions about human nature, i.e. human psychology (Gordon 1978). (2) All psychological hypotheses – even the most extreme empiricist/associationist ones – imply that the human brain/mind comprises species-typical goal-directed mechanisms. (3) Because Darwin's theory of adaptation through natural selection is "the only workable theory we have to explain the organized complexity of life" (Dawkins 1982, p. 35), human brain/mind mechanisms – qua mechanisms – were necessarily designed by natural selection. It isn't a question of their theory (which is how Vining characterizes "sociobiology"), but of our theory: In a fundamental sense we are all Darwinians (Dawkins 1982). The real issue is not whether the mechanisms comprising the human phenotype were designed by selection (there is no other viable candidate) or whether people in the modern world deploy their time and energy so as to maximize fitness (there is no reason to expect them to); rather, it is whether "selectional thinking" can contribute significantly to the study of human affairs. Darwinians believe that it can, and this belief is neither new nor "sociobiological." Darwin himself wrote: "To study Metaphysics [psychology] as they have been studied appears to me like puzzling at astronomy without mechanics. – Experience shows the problem of the mind cannot be solved by attacking the citadel itself. – the mind is function of body. – we must find some *stable* foundation to argue from" (as cited in Barrett 1974,

p. 331). Darwin's stable foundation was this: The mind is an aspect of the brain, and the brain is the product of natural selection.

Because they have developed almost entirely innocent of Darwinism, the social sciences have, to paraphrase Lloyd, committed certain kinds of errors, put forward certain suspect explanations, failed to pursue certain lines of research, and, by and large, failed to develop a sound criterion for recognizing significant observations (Symons, in press a; in press b). One small example: In the social science literature male–female differences in sexuality are almost uniformly attributed to socialization (social learning, scripts, roles, culture, society, etc.). If these attributions imply anything at all, surely it is that the mechanisms of the human brain/mind that underpin sexual feeling, thought, and action are sexually monomorphic. In other words, although social scientists may not deny the possibility of sexual dimorphism in the human brain, in the absence of unambiguous laboratory evidence to the contrary it has apparently seemed to them reasonable, prudent, and parsimonious to assume sexual monomorphism. Selectional thinking and comparative data, however, would have suggested the precise opposite as the reasonable, prudent, and parsimonious assumption: To a Darwinian, the chances of the human brain being sexually monomorphic are virtually nil (e.g. Symons 1979, p. 165; see Swaab & Fliers 1985 for pertinent data on the human brain). A social science truly informed by Darwin's view of life would differ profoundly from any social science that exists today.

Surrogate resources, cumulative selection, and fertility

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Vining's main point is entirely apt, that cultural forces are more powerful than genetical ones in changing human behavior. Indeed, we are startled that the converse still seems often to be claimed. We have four supplementary comments.

A mechanism for normally achieving a selectively desirable end can, in a new or rare environment, be selectively disadvantageous. This is like the behavioral observation of hungry rats preferring a nonnutritive saccharine solution to real food; a usually appropriate response may in fact be inappropriate. High-status individuals at any one time in social species usually seem to have more offspring, whether or not this is true over their entire lifespan. The pursuit of status itself is thus selected for, perhaps the most obvious result being horns (from goats to beetles) and other home-grown tools of intermale combat. Behavior and desires must evolve more or less concomitantly or these structures would be ineffective. Thus status becomes a *surrogate resource* (Van Valen 1976), giving greater expected reproduction but itself a primary desideratum. It is in short supply and therefore is competed for. That it remains a primary desideratum past an environmental (cultural) change which removes its earlier selective value should not cause surprise. We therefore suggest that most or all of us are constructed so as, for example, to learn to desire status more easily than to learn not to desire it. Even if this suggestion is correct, its mechanism may be indirect; status, for example, is correlated with social power.

That the expected response (if any) to current selection for low IQ is small does not mean that the cumulative response will remain small. Observed fertility differentials constitute an unusually strong selective pressure. (The measure of "strong" here is relative to what geneticists call the load space, the variation in fitness among individuals, which measures the maximum amount of total selection possible.) Undoubtedly it is prudent to learn more about many things before trying to reverse the effects (if any) of this selection, but scientists who ignore its potential evolutionary effect may be as shortsighted as politicians who ignore the forthcoming progressive exhaustion of petroleum because it doesn't affect their own election prospects.

A possible counter to the argument of Barkow and Burley (1980) is the fact that humans as a species are more fecund than apes, so both intelligence and birth rate have probably increased in human evolution, though not necessarily together. Lovejoy (1981) postulates that an increased birth rate was selected so that early hominids could compete with monkeys invading the savannas. Because fertility is positively correlated with fatness, greater birth rates occur when food is not scarce. It is only in the more technological or centralized societies that birth rates drop. In these, social security via the national or local government can replace the security offered by children and "tools" can take the place of hands in providing the necessities of life. The poor have fewer of these aids and so may be tempted to have larger families with more hands to secure a living. Highly promiscuous female humans and chimpanzees tend to have lower fertility than monogamous ones (MacKinnon 1978), for reasons as yet not well understood. Promiscuity was probably the original condition if a female could trade sex for food, as postulated recently by Fisher (1982) and Tanner (1981).

Use of the word "devolution" (for a somewhat different usage see Meredith 1982) implies that evolution is normally positive with respect to the user's values. As they might say in Wisconsin,

> Onward, mammals; onward, primates; Worms and such may fail, But we're atop the scale of nature 'Cause we construct the scale (rah, rah, rah). On, the active; on, the social; Get and spend it all, While the quiet and the fecund Wait until we fall.

Intelligence, reproductive success, and social status: A complicated relationship

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In this commentary, I will discuss (1) Vining's citation of my own work, (2) the significance these citations have for his conclusions, and (3) other work I have done relevant to these conclusions.

1. *Vining's citation of my work.* The only work of mine that Vining cites (Weinrich 1978a) is my response to a letter to the editor regarding a previous paper (Weinrich 1977). Vining cites this reply twice. His first citation (Section 2) asserts that in this reply, I (like others) did not appreciate the universality and constancy of the inverse relationship he finds between rank and fertility (or reproductive success, RS). But my original paper (Weinrich 1977) was not an attempt to explain fertility. Fertility was prominently mentioned only by my critic, so I gave a one-paragraph summary of my ideas on this topic and referred readers to my Ph.D. thesis (Weinrich 1976, pp. 9–18, 24–36, Tables 1–2, Figs. 1–8).

Vining's other citation of my reply (Section 2) asserts that it cited at least one of eight papers demonstrate a small but significantly positive correlation between IQ and fertility. Taken literally, this is incorrect; I did not cite any of those papers. I did cite some remarks by Lewontin suggesting that *he* was aware of such papers: "Upper-class couples might restrict fertility yet succeed in raising more offspring to the corresponding point in the next generation's life cycle." (This is known to be true for some IQ classes: see Lewontin 1970.) My point was methodological. I wanted to demonstrate that a negative relationship between social class and apparent fertility (number of children born, say) can sometimes turn into a positive relationship when apparent fertility is replaced by more sophisticated measures better related to RS – such as number of

children raised to maturity who marry and are fertile. I cited the IQ data only to show that a correlation of something with fertility *can* change sign when the measure of fertility is adjusted so as to be more sociobiologically relevant.

2. *How these affect Vining's conclusions.* These inaccuracies in citation, though minor, are symptoms of a more important inaccuracy in Vining's target article. His first major point is that sociobiologists are intensely interested in correlations between social rank and RS; he begins Section 2 with a direct quotation from E. O. Wilson exemplifying this. Here, he is right. But another major claim is that sociobiologists are intensely interested in the correlation of IQ with RS:

> The relationship between intelligence and reproductive fitness probably has an interest for sociobiologists equal to that for the relationship between rank and reproductive fitness. (Section 2)
>
> This work [showing a small positive correlation between IQ and RS] has been widely cited by sociobiologists and others interested in whether cultural and biological evolution reinforce each other in contemporary societies. (Section 2)

These statements are incorrect. Tellingly, Vining cites no one at all in support of the first remark, and hardcore sociobiologists are conspicuous by their absence among the 15 authors he cites in support of the second. I am one of those 15, and as shown above, his citation in my case is misleading. The other 14 fall mostly into the "others interested" class (Lewontin) rather than the "sociobiologist" class or are authors publishing in psychology (Daly & Wilson; Broadhurst, Fulker & Wilcox), or sociology (Ecklund). That is, they are not socio*biologists*.

In fact, Vining is far more interested in the relationship between IQ and RS than are any of the pioneers in sociobiology (Hamilton, E. O. Wilson, Trivers, Alexander, Irons, etc.). Consult the index for Wilson (1975) for example, where there is no listing under "intelligence" at all! The statements Wilson does make about intelligence are broad generalizations; for example, in Chapter 18, Wilson (1975) explains how high degrees of relatedness cause complex social structure in ants, whereas high intelligence causes it in mammals. Sociobiologists have made few statements about individual variations in intelligence *within* species.

This is because every sociobiologist I know understands that the relationship between intelligence and RS is complicated; there is a long causal chain linking intelligence (especially whatever it is that IQ tests measure) to RS, with some links likely to be contributing to a positive correlation and others likely to contribute to a negative one. The sum of a series of effects with varying signs cannot be presumed to be positive, negative, or zero. Only measurements of a particular form of intelligence in a particular population at a particular time, combined with a measure of RS (itself a difficult variable to measure), can establish the sign (not to mention the magnitude) of the correlation. These problems – which are both substantive and methodological – are what keeps sociobiologists quiet on the issue.

3. *Other work relevant to Vining's conclusions.* In my Ph.D. thesis (Weinrich 1976), I directly address the question of fertility differentials by socioeconomic class (or its proxies, education and income). In particular, I point out that socioeconomic status and income covary with income *predictability*, and that the effects of the two are confounded in straightforward plots of rank versus family size. A simple model (Weinrich 1976, pp. 24–36) taking income predictability into account suggests that income and completed fertility should be positively correlated for very low and very high income levels, and negatively correlated for intermediate incomes. This S-shaped pattern is often missed by demographers and census-takers, who (like other perfectly good scientists) prefer linear relationships instead of doubly curved ones, and collapse the very-high- and very-low-income categories in order to get a straight line. Accordingly, it is premature for Vining to conclude that a negative relationship between social rank and RS is well established.

I happen to be one of the few sociobiologists who *has* published papers explicitly relating to IQ and RS (Weinrich 1978b; 1980). Alas for people who want the relationship to be simple and positive, these are papers suggesting a correlation between high IQ and *non*reproductive (or less than fully reproductive) traits such as homosexuality and transsexualism, and the theories supported by the data are those relating to the sociobiological theory of kin selection (Hamilton 1964; Wilson 1975, pp. 115 ff. and passim). If these theories are correct, they would lessen the positive correlation Vining finds between IQ and RS. (Kin-selection theory attempts to explain acts which *lower* the RS of the actor in terms of how they may *increase* the so-called inclusive fitness [IF] of the individual performing the acts. RS maximizes not natural selection, but inclusive fitness.) This simply underscores the problem, alluded to above, of establishing the empirical connection between IQ and RS.

I do not wish to suggest by my comments that Vining's ideas are without merit, or that these questions are too controversial to be addressed. I merely believe that they impinge on extremely complicated questions, and that in some cases Vining has been premature in giving definitive answers. I myself have been told by several people that I am foolish to be publishing in areas considered so controversial. Obviously, I disagree, and would welcome Vining to join me in going where angels fear to tread.

Avarice aforethought and the fundamental premise of sociobiology

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The basic premise of sociobiology is usually that social behavior evolves if it has a genetic basis and increases the Darwinian fitness of the animals evincing the behavior. A modification of this premise is that animals exploit favorable environments to increase their fitness. In this form, the premise does not appear to be consistent with what is observed in contemporary urban societies. On the contrary, various deleterious or even dysgenic consequences could be expected to follow from the persistence of the current negative correlation between reproductive performance and various measures of "success" actually observed. That these observations are correct is supported by a variety of evidence and seems to be well established. However, at the present this has only been a short-term observation; we have no way whatever of using these data to make long-term predictions, especially genetic ones, nor can we assume that the pattern will persist. One is unlikely to make accurate predictions about the evolutionary future based on observations made in the midst of a period of great disequilibrium. Hence, it is correct to warn against the application of simplified fundamental postulates to all human societies, wherein there may be many theoretical pitfalls.

I would like to direct my comments toward a different point. It is that the basic premise of sociobiology, phrased in terms of exploitation of environments to increase fitness, may confound primary and secondary aspects, or intent and effect, of social behavior. It is basic to the definition of Darwinian fitness that, all other things being equal, genetically determined traits such as behavior which lead to increased fitness will proliferate relative to such behavior with lesser fitness. However, this implies nothing at all about the intent of the behavior or about the level of awareness the organism producing the behavior may have regarding its ultimate fitness effect.

Because of its delayed manifestation, among other reasons, there is no reason to assume that procreation itself is the intent of social behavior on the part of the animals or that it was typically so during the evolution of the basic human niche and adaptations. It is sufficient that animals be genetically "driven,"

by whatever proximal mechanism, to seek and defend resources such as territory and sexual access. Animals successful at these pursuits will usually be reproductively successful as a consequence (though they may also need a subsequent "drive" to care of their young).

In the context of their biology, environment, and experience, therefore, animals need not exploit favorable environments knowingly "to increase their genetic representation in the next generation," but only in a way that has the effect of doing so. If reproductive success is a highly correlated secondary effect of primary drives for resource acquisition, then a correct understanding of the evolution of relevant behavior need *not* assess reproduction directly. It may be that, as observed in the aboriginal context, "intelligence" and other leadership characteristics were typically correlated with fitness via their resource-acquiring properties, and that natural selection has been based on the genetics of competition expressed in terms of such characteristics.

If this is correct, then what we observe today, namely, societies with active competition for resources measured in the varied coin of our own context, is completely consistent with what we suggest as the basic premise of sociobiology. That reproduction is not at the moment correlated with resource acquisition is an artifact of our times, and may be a central theoretical problem to *contemporary* human sociobiology, but not to human sociobiology in general.

There are many pitfalls in applying a mechanistic theory to a being who can imagine the immaterial, including immortality, and whose environment is largely composed of such components. Human behavior cannot be analyzed as if we were simply insects, mice, or even monkeys. Yet, in its context and on its own terms, nothing about our behavior contradicts the basic premise. If culture and the extremely unstable nature of contemporary human life are taken into account, we fit consistently into the world of animal behavior. In any case, so far, our "untoward" behavior has not led us into chaos or decline; in fact, we are at present a most successful biological species, better than most others at exploiting the world's environmental resources. We cannot expect evolution to be able to see into the future, so our assessment must be based only on the present.

We are the inheritors of the society whose apparent imminent genetic demise led the Victorians to found the eugenics movement. Eugenic concerns have been based on short-term observations, and on value judgments about social arrangements in stratified societies. They are probably inherently shortsighted. Such concerns historically reflect the frustrating inconsistencies and imperfections of our world, but this does not translate directly into reliable long-term scenarios. If anything, the human species evolved for adaptability, and we do not know how this might affect the fitness of the species or that of individuals in the unknown environments in which they will find themselves in the future.

Rejecting sociobiological hypotheses

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Vining is quite successful in confirming his central thesis that in societies that have undergone the demographic transition individuals with greater status and control of resources do not show a greater net reproduction than individuals controlling fewer resources – quite the opposite. Vining's evidence is good and the point he makes is important, given the rapid spread of sociobiology which, even in its less controversial form as behavioral ecology, deserves greater critical treatment.

I would disagree with Vining on one minor point and possibly one major one. The biological notion of fitness refers not to total number of offspring ever born but to surviving offspring. Indeed, fitness as conceived in population genetics can be equated with net reproductive rate. There is therefore a conceptual problem in sociobiology, though not in Vining's contribution. Net reproductive rate is a parameter characterizing a group, not an individual. In a genetic model of evolution this group is a genotype or a phenotype, but even in such a model net reproductive rate is not maximized; it is the fitness (net reproductive rate) of the breeding population that is maximized by selection, not the net reproduction of individuals. So this somewhat central thesis of sociobiology is in difficulty as genetic theory quite apart from Vining's evidence.

I shall confine my remarks to the sociobiological hypothesis as applied to socioeconomic status. Vining makes a good point about the changing relationships between IQ and fertility. But not only is this relationship presently the reverse of what might be expected under the sociobiological hypothesis, it is also not clear that IQ is, to any great extent, associated with the acquisition (as opposed to the inheritance) of status and power. It would therefore seem to be a cleaner case to stay with fertility and socioeconomic status itself.

The sociobiological model assumes that net reproductive rate, or "reproductive success," is the dependent variable in any given generation, with socioeconomic status (and power) (SES) the independent variable. But people in Western society entered the demographic transition when they became convinced that SES was a dependent function of fertility. People began to lower their fertility in order to raise their SES. There is a subsequent development often not distinguished from this: the newly acquired ability to reliably dissociate sexual access from fertility. As Vining notes, this is dealt with, using the jargon of sociobiology, by Barkow and Burley (1980), who handle the major problem fairly well. If sexual access was keyed to status and power in the evolutionary past this would product not only the correspondence between status and fertility assumed to have existed in the past but also the reverse situation as it exists today.

This leads to the major point – concerning sociobiology as an applied science – on which I might disagree with Vining. If as an evolutionary development sexual access is to an extent linked with status and power, as it seems to be, this can help us to understand quite a few service industries today, quite a bit of advertising, and quite a bit of what is seen on television. If sociobiology can put forward testable ideas it can also be a useful applied science. I take it as evident that Vining has, in this article, tested and falsified one of the major hypotheses put forward by a number of sociobiologists.

Editorial Note

Unfortunately, after the target article was circulated for Commentary Professor Vining suffered a stroke. His Response will be delayed until a later issue to allow time for his recovery.

References

Alchian, A. (1950) Uncertainty, evolution, and economic theory. *Journal of Political Economy* 58:211–21. [DRV]

Alexander, R. (1980) *Darwinism and human affairs.* Pitman Publishing. [RF, JAK, JS, DRV]

Alexander, R. D., Hoogland, J. L., Howard, R. D., Noonan, K. M. & Sherman, P. W. (1979). Sexual dimorphisms and breeding systems in pinnipeds, ungulates, primates and humans. In: *Evolutionary biology and human social behavior: An anthropological perspective*, ed. N. A. Chagnon & W. Irons. [RD]

Allen, E., et al. (1975) Against "sociobiology." *New York Review of Books* 22 (Nov. 13):43–44. [DRV]

Altmann, S. (1984) What is the dual of the energy-maximization problem? *American Naturalist* 123:433–41. [DRV]

Anastasi, A. (1956) Intelligence and family size. *Psychological Bulletin* 53:187–209. [DRV]

(1959) Differentiating effect of intelligence and social status. *Eugenics Quarterly* 6:84–91. [DRV]

(1983). Evolving trait concepts. *American Psychologist* 38:175–84. [DRV]

Angel, J. L. (1973) Paleogeology, paleodemography and health. In: *Population, ecology and social evolution*, ed. S. Polgar. Aldine. [DRV]

Austin, C. (1983) Conferees weigh work with genes. *New York Times*, Aug. 7, p. 27. [DRV]

Bajema, C. (1963) Estimation of the direction and intensity of natural selection in relation to human intelligence by means of the intrinsic rate of natural increase. *Eugenics Quarterly* 10:175–87. [CJB, RL, DRV]

(1971) Natural selection and intelligence: The relationship between intelligence and completed fertility among third Harvard growth study participants. *American Journal of Physical Anthropology* 31:273. [CJB]

ed. (1976) *Eugenics then and now*. Dowden, Hutchinson & Ross. [CJB]

(1978) Genetic implications of population control. In: *Encyclopedia of bioethics*, vol. 3, ed. W. Reich. Free Press. [DRV]

Baker, P. (1984) The adaptive limits of human populations. *Man* 19:1–14. [DRV]

Baltes, P. & Shaie, K. (1976) On the plasticity of intelligence in adulthood and old age. *American Psychologist* 31:720–25. [DRV]

Barkow, J. H. (1975) Prestige and culture: A biosocial interpretation. *Current Anthropology* 16:553–72. [JAK]

(1977) Conformity to ethos and reproductive success in two Hausa communities: An empirical evaluation. *Ethos* 5:409–25. [JHB]

(1980a) Sociobiology: Is this the new theory of human nature? In: *Sociobiology examined*, ed. A. Montagu. Oxford University Press. [JHB]

(1980b) Biological evolution of culturally patterned behavior. In: *The evolution of human social behavior*, ed. J. Lockard. Elsevier. [JHB]

(1984) The distance between genes and culture. *Journal of Anthropological Research* 40:367–79. [JHB, DRV]

Barkow, J. H. & Burley, N. (1980) Human fertility, evolutionary biology, and the demographic transition. *Ethology and Sociobiology* 1:163–80. [RF, WI, JAK, LMVV, DRV, BJW]

Barnett, S. A. (1983) Humanity and natural selection. *Ethology and Sociobiology* 4:35–51. [HC]

Barrett, P. H. (1974) Darwin's early and unpublished notebooks. In: *Darwin on man: A psychological study of scientific creativity*, ed. H. E. Gruber. E. P. Dutton. [DS]

Baum, D. (1984) Singapore schools to admit children on basis of their mothers' education. *Wall Street Journal*, Jan. 30, p. 30. [DRV]

Beall, C. & Goldstein, M. (1981) Tibetan fraternal polyandry: A test of sociobiological theory. *American Anthropologist* 83:5–12. [DRV]

Becker, G. (1981) *A treatise on the family*. Harvard University Press. [DRV]

Ben-Porath, Y. (1982) Economics and the family – match or mismatch? A review of Becker's *A treatise on the family*. *Journal of Economic Literature* 20:52–64. [DRV]

Betzig, L. (1982) Despotism and differential reproduction: A cross-cultural correlation of conflict asymmetry, hierarchy, and degree of polygyny. *Ethology and Sociobiology* 3:209–21. [DRV]

Blake, J. (1968) Are babies consumer durables? A critique of the economic theory of reproductive motivation. *Population Studies* 22:5–25. [DRV]

Bloom, D. & Pebley, A. (1982) Voluntary childlessness: A review of the evidence and implications. *Population Research and Policy Review* 1:203–24. [DRV]

Bloom, D. & Trussel, J. (1984) What are the determinants of delayed childbearing and permanent childlessness in the United States? *Demography* 21:591–612. [DRV]

Blurton, Jones, N. G. & Sibly, R. M. (1978) Testing adaptiveness of culturally determined behaviour: Do Bushman women maximize their reproductive success by spacing births widely and foraging seldom? In: *Human behavior and adaptation*, ed. N. G. Blurton Jones & V. Reynolds. Taylor and Frances. [WI]

Boas, F. (1913) Influence of heredity and environment upon growth. *Zeitschrift für Ethnologie* 45:622–25. [JS]

(1931) Race and progress. *Science* 74:1–8. [JS]

Bock, K. (1980) *Human nature and history: A response to sociobiology*. Columbia University Press. [RF, DRV]

Bock, R. D. (1974) Review of *Educability and group differences* by Arthur Jensen. *Perspectives in Biology and Medicine* 18:594–97. [DRV]

Bongaarts, J. (1978) A framework for analyzing the proximate determinants of fertility. *Population and Development Review* 4:105–32. [RDR]

(1983) The proximate determinants of natural marital fertility. In: *Determinants of fertility in developing countries*, vol. 1, ed. R. A. Bulatao & R. D. Lee. Academic Press. [RDR]

Bouchard, T. (1976) Genetic factors in intelligence. In: *Human behavior genetics*, ed. A. Kaplan. Charles C. Thomas. [DRV]

(1982) Twins – nature's twice-told tale. In: *1983 yearbook of science and the future*. Encyclopaedia Britannica. [DRV]

Boyd, R. & Richerson P. (1981) Culture, biology and the evolution of variation between human groups. In: *Science and the question of human inequality*, AAAS Selected Symposium 58, ed. M. Collins, I. Wainer & T. Bremmer. Westview Press. [DRV]

(1985) *Culture and the evolutionary process*. University of Chicago Press. [PK]

Brewer, H. (1935) Eutelgenesis. *Eugenics Review*. 27:121–26. [CJB]

Broadhurst, P., Fulker, D. & Wilcock, J. (1974) Behavioral genetics. *Annual Review of Psychology* 25:389–415. [DRV]

Brockelman, W. K. (1975) Competition, the fitness of offspring, and optimal clutch size. *American Naturalist* 109:677–99. [JAK]

Bugos, P. E., Jr. & McCarthy, L. M. (1984) Ayoreo infanticide: A case study. In: *Infanticide: Comparative and evolutionary perspectives*, ed. G. Hausfater & S. B. Hrdy. Aldine. [WI]

Burley, N. (1979) The evolution of concealed ovulation. *American Naturalist* 114:835–58. [WI, DRV]

Caldwell, J. (1982) *Theory of fertility decline*. Academic Press. [CJB, DRV]

Calot, G. & Blayo, C. (1982) Recent course of fertility in Western Europe. *Population Studies* 36:349–72. [DRV]

Carter, C. (1961) Promising families: Some conclusions. *Eugenics Review* 52:197–200. [DRV]

(1962) Changing patterns of differential fertility in Northwest Europe and in North America. *Eugenics Quarterly* 9:147–50. [DRV]

Cattell, R. B. (1937) *Fight for our national intelligence*. P. S. King. [RL]

(1972) *A new mortality from science: Beyondism*. Pergamon. [DRV]

(1981) *The inheritance of personality and ability*. Academic Press. [DRV]

(1982) Inflation and business cycles from the standpoint of psychology and sociobiology. *Journal of Social, Political and Economic Studies* 7:35–54. [DRV]

Cavalli-Sforza, L. L. & Bodmer, W. F. (1971) The genetics of human populations. W. H. Freeman. [SJCG]

Cavalli-Sforza L. & Feldman M. (1981) *Cultural transmission and evolution*. Princeton University Press. [PK]

Chagnon, N. A. (1979). Is reproductive success equal in egalitarian societies? In: *Evolutionary biology and human social behavior*, ed. N. A. Chagnon & W. Irons. Duxbury Press. [WI]

(1980) Kin-selection theory, kinship, marriage and fitness among the Yanomamo Indians. In: *Sociology: Beyond nature/nuture?*, ed. G. W. Barlow & J. Silverberg. Westview Press. [DRV]

Chapais, B. (1983) Reproductive activity in relation to male dominance and the likelihood of ovulation in rhesus monkeys. *Behavioral Ecology and Sociobiology* 12: 215–28. [DRV]

Charnov, E. L. (1982) *The theory of sex-allocation*. Princeton University Press. [MTG]

Chaunu, P. (1981). *Un futur sans avenir*. Calmann-Levy. [DRV]

Chinn, S. & Rona, R. (1984) The secular trend in the height of primary school children in England and Scotland from 1972–1980. *Annals of Human Biology* 11:1–16. [DRV]

Cliquet, R. & Balcaen, J. (1979). Intelligentie, Gezinplanning, en Gezinvorming. *Bevolking en Gezin* 3:311–53. [DRV]

Coale, A. (1965) Comment. *Eugenics Quarterly* 12:58. [DRV]

Cochrane, S. H. (1979). *Fertility and education: What do we really know?* Johns Hopkins University Press. [RDR]

Cohen, J. (1971) Legal abortions, socioeconomic status, and measured intelligence in the United States. *Social Biology* 18:55–63. [DRV]

Collini, S. (1984) From clerk to guru [review of D. P. Crook, *Benjamin Kidd: Portrait of a Social Darwinist*]. *Times Literary Supplement*, Dec., pp. 1435–36. [DRV]

Cooper, G. (1977) A voluntary tax? New perspectives on sophisticated estate tax avoidance. *Columbia Law Review* 77:161–247. [DRV]

Daly, M. & Wilson, M. (1978). *Sex, evolution, and behavior*. Duxbury Press. [DRV]

(1981) Abuse and neglect of children in evolutionary perspective. In: *Natural selection and social behavior*, ed. R. D. Alexander & D. W. Tinkle. Chiron Press. [SJCG, WI]

(1982) Homicide and kinship. *American Anthropologist* 84:372–78. [WI, JAK]

(1983) *Sex, evolution, and behavior*, 2d Ed. Willard Grant Press. [RD, WI, DRV]

(1984) A sociobiological analysis of human infanticide. In: *Infanticide: Comparative and evolutionary perspectives*, ed. G. Hausfater & S. B. Hrdy. Aldine. [DS]

Darlington, P. (1983) Evolution: Questions for the modern theory. *Proceedings of the National Academy of Sciences* 80:1960–63. [DRV]
Davis, B. (1976) Evolution, human diversity, and society. *Zygon* 11:80–95. [DRV]
Dawkins, R. (1982) *The extended phenotype: The gene as the unit of selection.* W. H. Freeman. [RF, DS, JS, DRV]
Depauw, J. (1976) Illicit sexual activity and society in eighteenth century France. In: *Family and society*, ed. E. Forster & O. Ranum. John Hopkins University Press. [HK]
Dewsbury, D. (1982) Dominance, rank, copulatory behavior, and differential reproduction. *Quarterly Review of Biology* 57:135–59. [DRV]
Diamond, J. (1985) Everything *else* you've always wanted to know about sex. *Discover* 6:70–82. [HK]
Dickemann, M. (1979) The ecology of mating systems in hypergynous dowry societies. *Social Science Information* 18:162–195. [JAK]
Dong-Sheng, S. (1981) Popularizing the knowledge of eugenics and advocating optimal births vigorously. *Renkou Yanjiu* No. 4, 37–41 (in Chinese), translated in *Mankind Quarterly* 24 (1984), 167–83. [DRV]
Draper, P. & Harpending, H. (1982) Father absence and reproductive strategy: An evolutionary perspective. *Journal of Anthropological Research* 38:255–73. [DRV]
Duncan, O. (1952) Is the intelligence of the general population declining? *American Sociological Review* 17:401–7. [DRV]
Durante, D. (1970) *Napoleone corni e gloria, overo l'inelutabile lege del Menga.* Abano Terme: Il Gerione. [MTG]
Easterlin, R. A. (1975). An economic framework for fertility analysis. *Studies in Family Planning* 6:54–63. [RDR]
(1980a) American population since 1940. In: *The American economy in transition*, ed. M. Feldstein. University of Chicago Press. [DRV]
(1980b) *Birth and fortune.* Basic Books. [SJCG]
Ecklund, B. (1967). Genetics and sociology: A reconsideration. *American Sociological Review* 32:173–93. [DRV]
Edgeworth, F. (1881) *Mathematical psychics.* C. Kegan Paul. [DRV]
Editorial (1984) Eugenics in Singapore. *Nature* 308:214. [DRV]
Ehrlich, P. R. & Ehrlich, A. H. (1979) What happened to the population bomb? *Human Nature* 2:88–92. [HC]
Ehrman, L. & Parsons, P. (1981) *Behavior genetics and evolution.* McGraw-Hill. [DRV]
Eibl-Eibesfeldt, I. (1979) Konrad Lorenz. In: *International encyclopedia of the social sciences – biographical supplement*, Vol. 18, ed. D. Sills. Free Press. [DRV]
Elley, W. B. (1969) Changes in mental ability in New Zealand school children. *New Zealand Journal of Educational Studies* 4:140–55. [RL]
Emlen, S. T. & Oring, L. W. (1977). Ecology, sexual selection and the evolution of mating systems. *Science* 197:215–23. [RD]
Espenshade, T. J. (1984) *Investing in children: New estimates of parental expenditures.* Urban Institute Press. [HK]
Essock-Vitale, S. (1984) The reproductive success of wealthy Americans. *Ethology and Sociobiology* 5:45–49. [SJCG, HK, DRV]
Essock-Vitale, S. M. & McGuire, M. T. (1980) Predictions derived from the theories of kin selection and reciprocation assessed by anthropological data. *Ethology and Sociobiology* 1:233–43. [WI]
(in press) Women's lives viewed from an evolutionary perspective: 1. Sexual histories, reproductive success, and demographic characteristics of a random sample of American women. *Ethology and Sociobiology.* [HK]
Etkin, W. (1981) A biological critique of sociobiological theory. In: *Sociobiology and human politics*, ed. E. White. D. C. Heath. [HC]
Falconer, D. S. (1960) Introduction to quantitative genetics. Ronald. [SJCG]
(1966) Genetic consequences of selection pressure. In: *Genetic and environmental factors in human ability*, ed. J. Meade & A. Parkes. Oliver & Boyd. [DRV]
(1981) *Introduction to quantitative genetics*, 2d ed. Longman. [DRV]
Feigenbaum, E. & McCorduck, P. (1983) *The fifth generation. Artificial intelligence and Japan's computer challenge to the world.* Addison-Wesley. [DRV]
Feldman, M. W. & Lewontin R. C. (1975) The heritability hang-up. *Science* 190:1163–68. [JAK]
Ferri, E. (1904) *Socialism and modern science (Darwin-Spencer-Marx)*, 2d Ed., tr. R. R. La Monte. New York. [RF]
Fisher, H. E. (1982) *The sex contract.* William Morrow. [LMVV]
Fisher, R. A. (1929) *The genetical theory of natural selection.* Oxford University Press. [MTG]
(1930) *The genetical theory of natural selection.* Clarendon Press. [RD]
(1958) The genetical theory of natural selection. Dover Press. [JAK]
Flynn, J. (1980) *Race, IQ, and Jensen.* Routledge & Kegan Paul. [DRV]
(1982) Lynn, the Japanese, and environmentalism. *Bulletin of the British Psychological Society* 35:409–13. [DRV]
(1984a) The mean IQ of Americans: Massive gains 1932 to 1978. *Psychological Bulletin* 95:29–51. [RL, DRV]
(1984b) Banishing the spectre of meritocracy. *Bulletin of the British Psychological Society* 37:256–59. [JRF]
Fortes, M. (1949) *The web of kinship among the Tallensi.* Oxford University Press. [JHi]
Fox, R. (1982) Inhuman nature and unnatural rights. *Society* 20:70–76. [DRV]
(1983) *The red lamp of incest: An enquiry into the origins of mind and society.* University of Notre Dame Press. [RF]
Frautschi, S. (1982) Entropy in an expanding universe. *Science* 217:593. [JHa]
Freedman, R. (1979) Theories of fertility decline: A reappraisal. In: *World population and development: Challenges and Prospects*, ed. P. Hauser. Syracuse University Press. [RDR]
Freedman, R., Freedman, D. & Thornton, A. (1980) Changes in fertility expectations and preferences between 1962 and 1977: Their relation to final parity. *Demography* 17:365–78. [DRV]
Frisancho, A. R., Cole, P. & Klayman, P. J. (1977) Greater contribution to secular trend among offspring of short parents. *Human Biology* 44:51–60. [DRV]
Fuller, J. (1983) Sociobiology and behavior genetics. In: *Behavior genetics – principles and applications.* ed. J. Fuller & E. Simmel. Erlbaum Associates. [DRV]
Galbraith, R. (1982) Sibling spacing and intellectual development: A closer look at the confluence model. *Developmental psychology* 18:151–73. [DRV]
Ghiselin, M. T. (1974) *The economy of nature and the evolution of sex.* University of California Press. [MTG]
(1982) On the mechanisms of cultural evolution, and the evolution of language and the common law. *Behavioral and Brain Sciences* 5:11. [MTG]
Godfrey, L. & Jacobs, K. (1981) Gradual, autocatalytic and punctuational models of hominid brain evolution: A cautionary tale. *Journal of Human Evolution* 10:255–72. [DRV]
Goodman, D. (1974) Natural selection and a cost ceiling on reproductive effort. *American Naturalist* 108:247–68. [JAK]
Gordon, M. M. (1978) *Human nature, class, and ethnicity.* Oxford University Press. [DS]
Gottesman, I. (1968) Biogenetics of race and class. In: *Social class, race, psychological development*, ed. M. Deutsch, I. Katz & A. Jensen. Holt, Rinehart, and Winston. [DRV]
Gould, S. (1983) Genes on the brain. *New York Review of Books* 30 (June 30): 5–10. [DRV]
(1984) Singapore's patrimony (and matrimony). *Natural History* 93 (May):22–29. [DRV]
Gould, S. J. & Lewontin R. C. (1979) The spandrels of San Marco and the Panglossian paradigm: A critique of the adaptationist programme. *Proceedings of the Royal Society of London* B205:581–598. [MD]
Gray, J. P. (1985) *Primate sociobiology.* Human Relations Area Files Press. [JS]
Hailman, J. P. (1980) Fitness, function, fidelity, fornication, and feminine philandering. *Behavioral and Brain Science* 3:189. [DS]
Hajnal, J. (1965) European marriage patterns in perspective. In: *Population and history*, ed. D. V. Glass & D. E. C. Eversley. Aldine. [HK]
Haldane, J. (1956) Alfred Kinsey. *Hindu Weekly Review* (Oct. 8). Reprinted in *Kinsey – a Biography*, by C. Christenson, pp. 228–30. Indiana University Press. [DRV]
Haldane, J. B. S. (1957) The cost of natural selection. *Journal of Genetics* 55:511–24. [MTG]
Hamilton, W. D. (1964) The genetical evolution of social behavior. *Journal of Theoretical Biology* 7:1–52. [HC, WI, JDW]
Hardin, G. (1968) The tragedy of the commons. *Science* 162:1243–48. [CJB]
(1972) Genetic consequences of cultural decisions in the realm of population. *Social Biology* 19:350–61. [DRV]
Harley, S. F. (1975) *Illegitimacy.* University of California Press. [HK]
Hartung, J. (1984) Heritable IQ – a reason to bother. *Nature* 311:515–16. [DRV]
Hashimoto, M. (1974) Economics of postwar fertility in Japan: Differentials and trends. *Journal of Political Economy* 82:S170–94. [DRV]
Hayami, A. (1980) Class differences in marriage and fertility among Tokugawa villagers in Mino Province. *Keio Economic Studies* 17:1–16. [DRV]
Heath, A. C., Berg, K., Eaves, L. J., Solaas, M. H., Corey, L. A., Sundet, J., Magnus, P. & Nance W. E. (1985) Education policy and the heritability of educational attainment. *Nature* 314:734. [JHa]
Heaton, T. (1984) Fertility in a pronatalist religion: The case of Mormons. Unpublished. [DRV]
Heinsohn, G. & Steiger, O. (1979) The economic theory of fertility. An

alternative approach for an economic determination of procreation. *Metroeconomica* 31:271–98. [DRV]
Henderson, N. (1982) Human behavior genetics. *Annual Review of Psychology* 33:403–40. [DRV]
Henry, L. (1961) Some data on natural fertility. *Eugenics Quarterly* 8:81–91. [RDR]
Higgins, J., Reed, E. & Reed, S. (1962) Intelligence and family size: A paradox resolved. *Eugenics Quarterly* 9:84–90. [CJB, DRV]
Hill, J. (1978) The origin of sociocultural evolution. *Journal of Social and Biological Structures* 1:377–86. [JH]
(1984a) Human altruism and sociocultural fitness. *Journal of Social and Biological Structures* 7:17–35. [JH]
(1984b) Prestige and reproductive success in man. *Ethology and Sociobiology* 5:77–95. [JHi, JAK, DRV]
Himmelfarb, G. (1985) From Clapham to Bloomsbury: A genealogy of morals. *Commentary* 79(2):36–45. [DRV]
Hirschman, A. (1982) Rival interpretations of market society: Civilizing, destructive, or feeble? *Journal of Economic Literature* 20:1463–84. [DRV]
Hirshleifer, J. (1977) Economics from a biological viewpoint. *Journal of Law and Economics* 20:1–52. [DRV]
(1980) Privacy: Its origin, function, and future. *Journal of Legal Studies* 9:649–64. [DRV]
Hofferth, S. (1984) Long-term economic consequences for women of delayed childbearing and reduced family size. *Demography* 21:141–55. [DRV]
Hopkins, K. (1983) *Death and renewal, sociological studies in Roman history*, vol. 2. Cambridge University Press. [DRV]
Horn, H. S. & Rubenstein, D. I. (1984) Behavioral adaptations and life history. In: *Behavioral ecology: An evolutionary approach*, ed. J. R. Krebs & N. B. Davies. Sinauer. [HK]
Huntington, E. & Whitney, L. (1927) *The builders of America*. Morrow. [DRV]
Huxley, J. S. (1936) Eugenics and society, *Eugenics Review* 28:11–31. Repr. in Bajema 1976. [CJB]
Irons, W. (1977) Evolutionary biology and human fertility. Paper prepared for the Symposium "Family, Fertility, and Economics" at the 1977 American Antropoligical Association annual meeting. [WI, DRV]
(1979) Cultural and biological success. In: *Evolutionary biology and human social behavior: An anthropological perspective*, ed. N. Chagnon & W. Irons. Duxbury Press. [JS, DRV]
(1983) Human female reproductive strategies. In: *Social behavior of female vertebrates*, ed. S. K. Wasser. Academic Press. [JHB, JHi, WI, JS, DRV]
Jay, A. (1971) *Corporation man*. Random House. [DRV]
Jensen, A. (1981) Obstacles, problems, and pitfalls in differential psychology. In: *Race, social class, and individual differences*, ed. S. Scarr. Erlbaum Associates. [DRV]
Jensen, A. & Reynolds, C. (1982) Race, social class, and ability patterns on the WISC-R. *Personality and Individual Differences* 3:423–38. [DRV]
Kaku, K. (1972) Are physicians sympathetic to superstition? A study of *Honoe-Uma*. *Social Biology* 19:60–64. [DRV]
Kamin, L. J. (1974) *The science and politics of I.Q.* Erlbaum. [JH]
Karlsson, J. (1978). *The inheritance of creative intelligence*. Nelson-Hall. [DRV]
Kawasaki, S. (1985) Fertility estimation by own-children method in Japan. Paper presented at the Tenth Population Census Conference, East-West Center, Honolulu. [RDR]
Kevles, D. J. (1985) *In the same of eugenics*. Knopf. [JH]
Keyfitz, N. (1977) *Applied mathematical demography*. Wiley. [JHa]
(1982) The limits of population forecasting. *Population and Development Review* 7:579–94. [DRV]
Keynes, J. (1949) *Two Memoirs*. Rupert Hart-David. [DRV]
Kirk, D. (1957) The fertility of a gifted group: A study of the number of children of men in *Who's Who*. In: *The nature and transmission of the genetic and cultural characteristics of human populations*. Milbank Memorial Fund. [DRV]
(1969) The genetic implications of family planning. *Journal of Medical Education* 44 (Supplement 2):80–83. [DRV]
Kiser, C. (1959) Current mating and fertility patterns and their demographic significance. *Eugenics Quarterly* 6:65–82. [DRV]
Kitcher, P. (1985) *Vaulting ambition: Sociobiology and the quest for human nature*. MIT Press. [PK]
Kurland, J. A. & Gaulin, S. J. C. (1984) The evolution of male parental investment: Effects of genetic relatedness and feeding ecology on the allocation of reproductive effort. In: *Primate paternalism*, ed. D. M. Taub. Van Nostrand Reinhold. [JAK]
Lack, D. (1954) *The natural regulation of animal numbers*. Clarendon Press. [RD, WI]
(1966) Population studies of birds. Oxford University Press. [JAK]
(1968) *Ecological adaptations for breeding in birds*. Metheun. [HK]
Laslett, P. (1977) *Family life and illicit love in earlier generations*. Cambridge University Press. [HK]
Ledley, F. D., Grenett, H. E., Dilella, A. G., Kwok, S. C. M. & Woo, S. L. C. (1985) Gene transfer and expression of human phenylalanine hydroxylase. *Science* 228:77. [JHa]
Leeuw, J. de & Meester, A. C. (1984) Over het intelligente-onderzoek bij de militaire keuringen vanaf 1925 tot heden. *Mens en Maatschappij* 59:5–26. [JRF]
Leffingwell, A. (1976, 1892) *Illegitimacy and the influence of seasons upon conduct*. Arno. [HK]
Lerner, B. (1983) Test scores as measures of human capital and forecasting tools. *Journal of Social, Political and Economic Studies* 11:131–59. [DRV]
Lerner, I. (1968) *Heredity, evolution, and society*. W. H. Freeman. [DRV]
Levine, E. (1983) Why middle class students aren't learning. *Journal of Social, Political and Economic Studies* 8:411–26. [DRV]
Lewontin, R. (1970). Further remarks on race and the genetics of intelligence. *Bulletin of Atomic Scientists* 26:23–25. [DRV]
(1982) *Human diversity*. W. H. Freeman. [DRV]
Lewontin, R. C. & Cockerham, C. C. (1959) The goodness-of-fit test for detecting natural selection in random mating populations. *Evolution* 13:561–64. [JAK]
Lewontin, R., Rose, S. & Kamin, L. (1984) *Not in our genes*. Pantheon Books. [DRV]
Lightcap, J. L., Kurland, J. A. & Burgess, R. L. (1982) Child abuse: A test of some predictions from evolutionary theory. *Ethology and Sociobiology* 3:61–67. [SJCG, JAK]
Llewellyn-Jones, D. (1974) *Human reproduction and society*. Pitman. [HK]
Lloyd, J. E. (1979) Mating behavior and natural selection. *Florida Entomologist* 62:17–34. [DS]
Lotka, A. (1922) Contribution to the energetics of evolution. *Proceedings of the National Academy of Sciences* 8:147–51. [DRV]
Lovejoy, C. O. (1981) The origin of man. *Science* 211:341–50. [LMVV]
Lumsden, C. (1983). Cultural evolution and the devolution of tabula rasa. *Journal of Social and Biological Structure* 6:101–14. [DRV]
Lumsden, C. & Wilson, E. O. (1981a) *Genes, mind, and culture*. Harvard University Press. [MTC, PK, DRV]
(1981b) Letter to the editor. *New York Review of Books* 28 (Sept. 24):73–74. [DRV]
(1983) *Promethean fire*. Harvard University Press. [SJCG, DRV]
Lynn, R. (1982) IQ in Japan and the United States shows a growing disparity. *Nature* 297:222–23. [RL, DRV]
Lynn, R. & Hampson, S. (in press) The rise of national intelligence: Evidence from Britain, Japan and the United States. *Personality and Individual Differences* [RL]
MacKenzie, D. (1981) *Statistics in Britain – 1865–1930*. Edinburgh University Press. [DRV]
MacKinnon, J. (1978) *The ape within us*. Holt, Rinehart and Winston. [LMVV]
Macphail, E. M. (1982) *Brain and intelligence in vertebrates*. Clarendon Press. [EMM]
(1985) Comparative studies of animal intelligence: Is Spearman's g really Hull's *D*? *Behavioral and Brain Sciences* 8:234–35. [EMM]
Maynard Smith, J. (1977). Parental investment: A prospective analysis. *Animal Behaviour* 25:1–9. [RD]
Maynard Smith J. & Warren N. (1982) Review of *Genes, mind, and culture*. *Evolution* 36:620–27. [PK]
Mayr, E. (1976) *Evolution and the diversity of life*. Harvard University Press, Belknap. [DRV]
McClearn, G. (1970) Behavioral genetics. *Annual Review of Genetics* 4:437–68. [DRV]
Medawar, P. (1984) Review of *What sort of people should there be?* by Jonathan Glover. *Times Higher Education Supplement* 586 (Jan. 27):16 [DRV]
Meredith, A. (1982) Devolution. *Journal of Theoretical Biology* 96:49–65. [LMVV]
Meredith, H. (1976) Findings from Asia, Australia, Europe and North America on secular change in mean height of children, youths and young adults. *American Journal of Physical Anthropology* 44:315–26. [DRV]
Miller, A. & Corsellis, J. (1977) Evidence for a secular increase in human brain weight during the past century. *Annals of Human Biology* 4:253–57. [DRV]
Miller, W. (1983) Chance, choice, and the future of reproduction. *American Psychologist* 38:1198–1205. [DRV]
Moore, W. & Wilson R. M. (1982) The influence of children on the wage rates of married women. *Eastern Economic Journal* 8:197–210. [DRV]

Mueller, E. & Short K. (1983) Effects of income and wealth on the demand for children. In: *Determinants of fertility in developing countries*, vol. 1, ed. R. A. Bulatao & R. D. Lee. Academic Press. [RDR]

Muller, H. J. (1934) The dominance of economics over eugenics. In: *A decade of progress in eugenics*, ed. H. Perkins et al. Williams & Wilkins. repr. in Bajema 1976. [CJB]

(1935) *Out of the night: A biologist's view of the future*. Vanguard Press. [CJB]

Neel, J. (1980) On being headman. *Perspectives in Biology and Medicine* 23:277–94. [DRV]

Nelson, R. R. & Winter, S. G. (1982) *An evolutionary theory of economic change*. Harvard University Press. [MTG]

O'Connell, M. (1981) Regional fertility patterns in the United States: Convergence or divergence. *International Regional Science Review* 6:1–14. [DRV]

Olneck, M. & Wolfe, B. (1980) Intelligence and family size: Another look. *Review of Economics and Statistics* 62:241–47. [DRV]

Orians, G. H. (1969). On the evolution of mating systems in birds and mammals. *American Naturalist* 103:589–603. [RD]

Ortega y Gasset, J. (1932). *The revolt of the masses*. W. W. Norton. [DRV]

Osborn, F. (1940) *Preface to eugenics*. Harper. [DRV]

(1968) *The future of human heredity – an introduction to eugenics in modern society*. Weybright and Talley. [DRV]

(1973) *The human condition*. Hugh Laiter Levin Associates. [DRV]

Osborn, F. & Bajema, C. (1972) The eugenic hypothesis. *Social Biology* 19:337–45. [DRV]

Parsons, T. & Harrison, B. (1981) Energy utilization and evolution. *Journal of Social and Biological Structures* 4:1–5. [DRV]

Pearson, K. (1901) *The ethic of freethought*, 2d Ed. London. [RF]

Pickens, D. R. (1968) *Eugenics and the progressives*. Vanderbilt University Press. [RF]

Plomin, R., DeFries, J. & McClearn, G. (1980) *Behavioral genetics: A primer*. W. H. Freeman. [DRV]

Population Reference Bureau (1983) *1983 world population data sheet*. Washington, D.C. [DRV]

Pulliam, H. & Dunford, C. (1980) *Prgrammed to learn*. Columbia University Press. [DRV]

Reed, S. & Palm, J. (1951) Social fitness versus reproductive fitness. *Science* 113:294–96. [DRV]

Retherford, R. D. (1985) A theory of marital fertility transition. *Population Studies* 39:249–68. [RDR, DRV]

Retherford, R. & Sewell, W. (1985) Family size and intelligence reconsidered. East-West Population Institute, East-West Center, Honolulu, Hawaii. [DRV]

Richards, G. (1984) Getting the intelligence controversy knotted. *Bulletin of the British Psychological Society* 37:77–79. [DRV]

Richerson, P. & Boyd, R. (1981) The search for an alternative to the sociobiological hypothesis. *Behavioral and Brain Sciences* 4:248–49. [DRV]

Rindfuss, R. R. & Sweet, J. A. (1977) *Postwar fertility trends and differentials in the United States* Academic Press. [HK]

Rist, T. (1982) Det intellektuelle prestasjonsnivaet I befolkningen sett I lys av den samfunns-messige utviklinga. Thesis presented at the Insitiute of Psychology, University of Oslo. [JRF]

Ruoyun, Y. (1983) Paying great attention to eugenics and improving the quality of population. *Renkou Yanjiu* No. 3, 33–35 (in Chinese), translated in *Population and Development Review* 9 (1984):757–61. [DRV]

Ruse, M. (1982) Is human sociobiology a new paradigm. *Philosophical Forum* 13:119–43. [DRV]

Sahlins, M. (1976) *The use and abuse of biology*. University of Michigan Press. [JS]

Saxonhouse, G. (1974) Comment. *Journal of Political Economy* 82:S195–99. [DRV]

Scarr, S. (1981) Toward a more biological psychology. In: *Science and the question of human equality*, AAAS Selected Symposium 58, ed. M. Collins, I. Wainer & T. Bremner. Westview Press. [DRV]

Schecht, L. E. & Gershowitz, H. (1963) Frequency of extra-marital children as determined by blood groups. In: *Proceedings of the second international congress of human genetics*, 1961, vol. 2. Instituto G. Mendel. [HK]

Schaffer, W. M. (1974). Optimal reproductive effort in fluctuating environments. *American Naturalist* 108:783–90. [JAK]

Schaie, K. W. & Labouvie-Vief, G. (1974) Generational versus ontogenetic components of change in adult cognitive behavior: A fourteen-year cross-sequential study. *Developmental Psychology* 10:305–20. [DRV]

Schiff, M., Duyme, M., Dumaret, A. & Tomkiewicz, S. (1982) How much *could* we boost scholastic achievement and IQ scores? A direct answer from a French adoption study. *Cognition* 12:165–96. [DRV]

Schilcher, F. & Tennant, N. (1984) *Philosophy, evolution and human nature*. Routledge & Kegan Paul. [DRV]

Schultz, T. P. (1983) Review of John C. Caldwell, *Theory of fertility decline*. *Population and Development Review* 9:161–68. [DRV]

Schumpeter, J. (1950) *Capitalism, socialism, and democracy*. Harper and Row. [DRV]

Sewell, W. H., Haller, A. O. & Ohlendorf, G. W. (1970) The educational and early occupational attainment process: Replication and revision. *American Sociological Review* 35:1014–27. [RDR]

Shorter, F. (1975) *The making of the modern family*. Basic Books. [HK, DRV]

Silverberg, J. (1980) Sociobiology, the new synthesis? An anthropologist's perspective. In: *Sociobiology: Beyond nature/nurture?* ed. G. Barlow & J. Silverberg. Westview Press. [JS]

Simon, J. (1974) *The effects of income on fertility*. Carolina Population Center. [SJCG, HK]

Simons, G. (1983) *Are computers alive? Evolution and new life forms*. Harvester. [DRV]

Sly, D. & Richards, S. (1972) The fertility of a sample of American elites. *Social Biology* 19:393–400. [DRV]

Smith, C. C. & Fretwell S. D. (1974) The optimal balance between size and number of offspring. *American Natuuralist* 108:499–506. [JAK]

Spuhler, J. (1962) Empirical studies on quantitative human genetics. In: *The use of vital and health statistics for genetic and radiation studies*. United Nations. [DRV]

Stearns, S. C. (1976) Life-history tactics: A review of the ideas. *Quarterly review of Biology* 51:13–47. [JAK]

Stern, C. (1973) *Principles of human genetics*, 3d Ed. W. H. Freeman. [DRV]

Stigler, G. & Becker, G. (1977) De gustibus non est disputandum. *American Economic Review* 67:76–90. [DRV]

Stone, L. (1977) *The family, sex and marriage in England: 1500–1800*. Harper and Row. [DRV]

Swaab, D. F. & Fliers, E. (1985) A sexually dimorphic nucleus in the human brain. *Science* 228:1112–15. [DS]

Sweet, K. & Rindfuss, R. (1983) Thos ubiquitous fertility trends: United States, 1945–1979. *Social Biology* 30:127–39. [DRV]

Symons, D. (1979) *The evolution of human sexuality*. Oxford University Press. [WI, DS]

(in press a) If we're all Darwinians, what's the fuss about? In: *Sociobiology and psychology: Ideas, issues, and findings*, ed. C. Crawford, M. Smith & D. Krebs. Erlbaum Associates. [DS]

(in press b) The evolutionary approach: Can Darwin's view of life shed light on human sexuality? In: *Approaches and paradigms of human sexuality*, ed. J. Geer & W. O'Donohue. Plenum Press. [DS]

Taeuber, I. (1948) *The population of Japan*. Princeton University Press. [DRV]

Takahashi, E. (1984) Secular trend in milk consumption and growth in Japan. *Human Biology* 56:427–37. [DRV]

Tanfer, K. & Horn, M. (1985) Contraceptive use, pregnancy and fertility patterns among single American women in their 20s. *Family Planning Perspectives* 17:10–19. [DRV]

Tanner, N. M. (1981) *On becoming human*. Cambridge University Press. [LMVV]

Teasdale, T. W. & Owen, D. R. (1984) Heredity and family environment in intelligence and educational level – a sibling study. *Nature* 309:620. [JHa]

Terman, L., Sears, R., Cronbach, L. & Sears, P. (1983) Terman life cycle study of children with high ability, 1922–1982. Inter-University Consortium for Political and Social Research (ICPSR 8092). [DRV]

Terrenato, L. & Ulizzi, L. (1983) Genotype-environmental relationships: An analysis of stature distribution curves during the last century in Italy. *Annals of Human Biology* 10:335–46. [DRV]

Tien, H. (1981) Demography in China: From zero to now. *Population Index* 47:683–710. [DRV]

Trivers, R. L. (1985) *Social evolution*. Benjamin/Cummings. [RD]

Tuddenham, R. D. (1948) Soldier intelligence in World Wars I and II. *American Psychologist* 3:54–56. [RL]

Udry, J. (1978) Differential fertility by intelligence: The role of birth planning. *Social Biology* 25:10–14. [DRV]

U.S. Bureau of the Census (1984) Fertility of American women: June 1982, *Current population reports*, series p-20, No. 387. U.S. Government Printing Office. [HK]

Van Court, M. & Bean, F. (1985) Intelligence and fertility in the United States, 1912–1982. *Intelligence* 9:23–32. [DRV]

Van Valen, L. (1974) Brain size and intelligence in man. *American Journal of Physical Anthropology* 40:417–23. [DRV]

(1976) Energy and evolution. *Evolutionary Theory* 1:179–229. [DRV, LMVV]
Veblen, T. (1915) *Imperial Germany and the Industrial Revolution.* MacMillan. [DRV]
Vehrencamp, S. L. & Bradbury, J. W. (1984) Mating systems and ecology. In: *Behavioural ecology: An evolutionary approach,* 2d Ed., ed. J. R. Krebs & N. B. Davies. Blackwell Scientific Publications. [RD]
Vickers, G. (1983) *Human systems are different.* Harper and Row. [DRV]
Vining, D. (1982a) On the possibility of the reemergence of a dysgenic trend with respect to intelligence in American fertility differentials. *Intelligence* 6:241–64. [DRV]
(1982b) Fertility differentials and the status of nations: A speculative essay on Japan and the West. *Mankind Quarterly* 22:311–53. [DRV]
(1984) Sub-fertility among the very intelligent: An examination of the American Mensa. *Personality and Individual Differences* 5:725–34. [DRV]
Walker, D., Harper, P., Newcombe, R. & Davies, K. (1983) Huntington's chorea in South Wales: Mutation, fertility, and genetic fitness. *Journal of Medical Genetics* 20:12–17. [DRV]
Wallace, B. (1958) The comparison of observed and calculated zygotic distributions. *Evolution* 12:113–15. [JAK]
Waller, J. (1971) Differential reproduction: Its relation to IQ test score, education, and occupation. *Social Biology* 18:122–36. [DRV]
Washburn, S. L. (1978) The evolution of man. *Scientific American* 239:146–54. [EMM]
Weinrich, J. D. (1976) *Human reproductive strategy.* Doctoral dissertation, Harvard University. Dissertation Abstracts International, 1977, 37, 5339-B. University Microfilms No. 77–8, 348. [JDW]
(1977) Human sociobiology: Pair-bonding and resource predictability (effects of social class and race). *Behavioral Ecology and Sociobiology* 2:91–118. [JDW]
(1978a) The author replies [to Lande's letter, "Are humans maximizing reproductive success?"]. *Behavioral Ecology and Sociobiology* 3:96–98. [DRV, JDW]
(1978b) Nonreproduction, homosexuality, transsexualism, and intelligence: 1. A systematic literature search. *Journal of Homosexuality* 3:275–89. [JDW]
(1980) On a relationship between homosexuality and I.Q. test scores: A review and some hypotheses. In: *Medical sexology: The third International congress,* ed. R. Forleo & W. Pasini. PSG Publishing. [JDW]
Weiss, V. (1980) Inbreeding and genetic distance between hierarchically structured populations measured by surname frequencies. *Mankind Quarterly* 21:135–49. [DRV]
Westoff, C. (1974) The populations of the developed countries. *Scientific American* 231 (Sept.):108–21. [DRV]
Whelpton, P. (1949) From eugenic abortion and sterilization to control of conception in Japan. *Eugenical News* 34:44–45. [DRV]
Williams, B. J. (1980) Kin selection and cultural evolution. In: *Sociobiology: Beyond nature/nurture?* ed. G. Barlow & J. Silverberg. Westview Press. [JS]
Williams, G. C. (1966) Adaptation and natural selection: A critique of some current evolutionary thought. Princeton University Press. [JAK, DS]
Wilson, D. (1978) A cultural to biological fitness. *Evolutionary Theory* 3:235–36. [DRV]
Wilson, E. O. (1975) *Sociobiology: The new synthesis.* Harvard University Press. [JS, DRV, JDW]
(1978) *On human nature.* Harvard University Press. [DRV]
(1980) *Comparative social theory.* Tanner Foundation. [HC]
Wilson-Davis, K. (1980) Ideal family size in the Irish Republic. *Journal of Biosocial Science* 12:15–20. [RL]
Wimperis, V. (1960) *The unmarried mother and her child* Allen and Unwin. [HK]
Wolfe, L. (1981) *The cosmo report* Arbor House. [HK]
Wrong, D. (1958) Trends in class fertility in Western nations. *Canadian Journal of Economics and Political Science* 24:216–29. [DRV]
(1980) *Class fertility trends in Western nations.* Arno. [JHi, DRV]
Wynne-Edwards, V. C. (1962) *Animal dispersion in relation to social behaviour.* Oliver & Boyd. [RL]
Wyshak, G. (1983) Secular changes in age at menarche in a sample of US women. *Annals of Human Biology* 10:75–77. [DRV]
Yamamura, K. (1974) *A study of Samurai income and entrepreneurship.* Harvard University Press. [DRV]
Yogev, S. & Vierra, A. (1983) The state of motherhood among professional women. *Sex Roles* 9:391–96. [DRV]
Young, A. M. (1985) One quarter of the adult labor force are college graduates. *Monthly Labor Review* 106:43–46. [HK]
Zajonc, R. (1983) Validating the confluence model. *Psychological Bulletin* 93:457–80. [DRV]
Zajonc, R. & Bargh, J. (1980) Birth orders, family size, and decline of SAT scores. *American Psychologist* 35:662–68. [DRV]