Fetal Mortality and Sex Ratio

McMillen (*1*) may be correct in "conservatively" estimating the primary ratio of human male to female conceptuses to be at least (Y_1:100 =) 120:100, but the argument she presents shows only the consistency of that range of ratio values with certain selected empirical data.

Since the quantity of interest is y_1, the proportion of males at conception [or equivalently $Y_1 = 100\ y_1/(1 - y_1)$], the analysis is much simplified by solving the equation of her reference 14 for y_1 [d defined below, the other variables as in (*1*)]:

$$y_1 = y_2\,(1 - f)/[d(y_2 - 1) - f + 1]$$

where f = proportional female zygote loss, "reported" to be between 0.2 and 0.5; $d = m - f$ = excess male-over-female mortality ratio, "observed" to be between .04 and .18; y_2 = the proportion of live-borns that are male, the male secondary sex proportion, observed to be between .5115 and .5131 for the later (1950 to 1972) of two sets of data used in her analysis. With this equation for y_1, the cited ranges of f, d, y_2, and the definition of Y_1, we obtain Y_1 values between 110 and 165; this range is quite close to the previous estimates, 110 to 170, that McMillen cites in her first paragraph. Thus she has not succeeded in reducing the range of uncertainty regarding the primary sex ratio.

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References

1. M. M. McMillen, *Science* **204**, 89 (1979).

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McMillen's (*1*) study of fetal mortality and her estimate of fetal sex ratio serve as a reminder of the difficulty of obtaining accurate data on the sex of aborted fetuses. The tables she has used for fetal mortality (*2*) are compiled from fetal death certificates and are inevitably fraught with uncertainty. On these certificates sex is recorded on the basis of morphological criteria, which are notoriously unreliable, especially as early as 3 months of gestation; and throughout gestation these criteria will give inaccurate or ambiguous indications of sex when external genitalia are abnormal. Many of the tables (*2*) report large numbers of fetuses of unknown sex, and it cannot even be certain that those classed as males and females are what they seem to be. Cases of polyploidy and sex chromosome aneuploidy, such as Turner's (XO) and Klinefelter's (XXY) syndromes, which lack the normal sex chromosome constitution, should be eliminated from a rigorous study of fetal sex ratio. But there is no way of knowing how they have been classified in these tables.

It seems unlikely that such uncertainty will be overcome until first-hand data on fetal mortality are available from a systematic study of the karyotypes of spontaneous and induced abortions, examined by modern staining techniques. Although there have been several large-scale studies along these lines, the published reports (*3, 4*) are inadequate for a confident count of sex ratio. Because the major purpose of these studies was usually to look for abnormal karyotypes, only one report (*4*) gives full details of the rest of the population studied, and information about gestational age is sparse. The data on which credible values for fetal sex ratio can be based are not likely to emerge from the archives: they have yet to be published.

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References

1. M. M. McMillen, *Science* **204**, 89 (1979).
2. *Vital Statistics of the U.S.*, vol. 2, part A, *Mortality* (Government Printing Office, Washington, D.C., 1922 to 1936 and 1950 to 1972).
3. M. Yamamoto, R. Fujimori, T. Ito, K. Kamimura, G. Watanabe, *Humangenetik* **29**, 9 (1975); T. J. Hassold, A. Matsuyama, I. M. Newlands, J. S. Matsuura, P. A. Jacobs, B. Manuel, J. Tsuei, *Ann. Hum. Genet.* **41**, 443 (1978); M. R. Creasy, J. R. Crolla, E. D. Alberman, *Hum. Genet.* **31**, 177 (1976); T. Kajii, K. Ohama, N. Niikawa, A. Ferrier, S. Avirachan, *J. Hum. Genet.* **25**, 539 (1973).
4. J. G. Lauritsen, *Acta Obstet. Gynecol. Scand. Suppl.* **52** (1976).

16 May 1979

A Terminal Mesozoic Greenhouse

McLean (*1*) has postulated a significant global warming trend during late Maastrichtian time; this warming trend, he suggests, led to wide-scale extinctions of terrestrial faunas and marine plankton. We question two aspects of McLean's argument.

First, McLean (*1*) cites Voight's (*2*) data on the distribution of rudistids and other marine invertebrates as evidence for a "cool early Maestrichtian [sic] with subsequent warming in late Maestrichtian." Polšak (*3*) used $^{16}O/^{18}O$ ratios to determine water temperatures from 139 Cretaceous limestone (41) and fossil (98) samples from the Dinarids and Slovenian Alps. Paleotemperatures obtained in this work are shown in Table 1. These data show that in the Tethyian seaway, water temperatures were highest in Santonian to late Campanian time, *declined* slightly during late Campanian through middle Maastrichtian time, and *declined* by 5°C during late Maastrichtian time.

Table 1. Paleotemperature of Tethyian sea (*3*); B.P., before present.

Geologic age	Water temperature (°C)	Approximate age (10^6 years B.P.)
Late Maastrichtian	18.5	67
Late Campanian–middle Maastrichtian	23.5	77.5
Santonian–late Campanian	25	80
Coniacian	23.5	85
Late Turonian	24	
Early Turonian	16	90
Cenomanian	21	100
Albian	16	105
Aptian	14.5	118
Barremian	16.5	122
Neocomian	13.5	

Second, McLean (*1*) states, "Warming of the earth's surface would warm the oceans. As the water warms, the solubility of CO_2 decreases and it is driven from the oceans into the atmosphere."

McLean considers only one factor, the other being precipitation of calcium carbonate when the saturation value of CO_2 is exceeded in warmed waters. The exact balance between precipitation and loss to the atmosphere is not now precisely known. However, it has been shown (*4*) that degassing of CO_2 is accompanied by precipitation of carbonate in modern marine environments. Hence, precipitation of large volumes of calcium carbonate would be expected to have accompanied McLean's Maastrichtian warming. Further, these large quantities of readily available calcium carbonate would not have caused solution of the calcareous shells of marine protists. Had warming occurred, we would expect the late Maastrichtian distribution of such protists to be geographically enhanced, and that the abundance of these fossils would have been significantly increased. Polšak (*3*) also shows this relationship with reference to the stenothermal rudists.

The "widespread submarine solution of carbonates at the end of the Mesozoic" (*1*) is as well explained by reduction of water temperature, which would cause an increase in the CO_2 saturation values of seawater, leading to solution of carbonates. Thus, we believe that lowered ocean temperature during Maastrichtian time (18.5°C as compared to 23.5°C in Campanian time) is incompatible with global warming and that solution of marine carbonates was a consequence of lowered temperature, perhaps contributory to the extinction of calcareous-walled protists, the base of the marine food chain in Maastrichtian time.

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References

1. D. W. McLean, *Science* **201**, 401 (1978).
2. E. Voight, *Geol. Rundsch.* **54**, 270 (1964).
3. A. Polšak, *Jugosl. Geol. Kongres* **2**, 263 (1976).
4. J. S. Hanor, *J. Sediment. Petrol.* **48**, 489 (1978).

21 August 1978

McLean (*1*) presents arguments in support of the theory that the extinction of the dinosaurs at the termination of the Mesozoic Era was due to a significant increase in global temperatures. He devotes equal attention, however, to supporting the claim that during this same era the deep ocean waters were "triggered" into releasing "vast" amounts of CO_2 in a "chain reaction" which might be duplicated today as a result of fossil fuel combustion and of forest clearing.

This latter conclusion is contradicted by his evidence (with references) that the end of the Mesozoic Era was characterized by widespread solution of carbonates. As he correctly points out, this would indicate that carbon dioxide was being injected into the oceans rather than being released.

For conclusions with respect to modern times, the article draws on Manabe and Wetherald's (*2*) model which predicts that the temperature increases resulting from a doubling of CO_2 in the atmosphere could be about 3°C at latitudes less than 60° and that as you proceed poleward these temperature increases should reach levels three to five times greater than those predicted for medium and low latitudes. McLean postulates that the surface water from the more greatly heated polar areas will transport heat into the deep oceans through downwelling. He proposes that this will overturn the density stratification of the oceans, causing the deep waters to release their excess CO_2.

But a process that is at least equally important in establishing the vertical density stratification of the ocean is the absorption of heat in the upper mixed layer (*3*). It is usually agreed (*4*) that atmospheric warming would increase the vertical density stratification of the oceans. This would certainly be true at latitudes less than 60° where significant downwelling does not occur. Although temperature increases at these latitudes would only be one-third to one-fifth that at higher latitudes, the area below 60° contains more than 90 percent of the ocean's surface and an even greater fraction of the ocean's volume. The proposed destabilizing effect, therefore, appears doubtful and, if true, would be so small as to require millennia to exert its effect.

The only remaining claims relate to a decrease in the solubility of CO_2 as the surface temperature of the ocean increases. The article suggests that a 1°C increase in ocean temperature would produce a 4 percent increase in atmospheric CO_2. This effect is not large compared to the 30 to 40 percent increase needed to raise the atmospheric temperature 1°C.

The documented global increase in CO_2 (currently referred to as the "greenhouse" effect) deserves serious scientific attention and a sense of urgency is justified in carrying out investigations to establish its cause, eventual magnitude, and implications. The objections in this comment are motivated by the feeling that highly speculative papers are counterproductive in that they arouse unjustified distrust of the evidence that high priority research on the greenhouse effect is needed.

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References

1. D. McLean, *Science* **201**, 401 (1978).
2. S. Manabe and R. T. Wetherald, *J. Atmos. Sci.* **24**, 786 (1967).
3. *Understanding Climatic Change: A Program for Action* (National Academy of Sciences, Washington, D.C., 1975).
4. *Energy and Climate* (National Academy of Sciences, Washington, D.C., 1977), p. 8.

2 October 1978

Black's (*1*) statement "highly speculative papers are counterproductive in that they arouse unjustified distrust of the evidence that high priority research on the greenhouse effect is needed" requires comment.

My works on the terminal Mesozoic reptilian (*2*), and late Pleistocene (15,000 to 9,000 years ago) mammalian extinctions (*3*), are geared to ascertaining the impact on life if there were, as postulated by the National Research Council (*4*) 6°C rise in global temperatures in the next two centuries. Cases may be made in linking the extinctions to global warming by heat-induced reproductive failure. Among mammals, females as well as males may be adversely affected by even normally hot summer temperatures, and any warming potentially outpacing the capacity of animals to evolve with it must be considered serious until proved otherwise. Humans are not immune; a 10-year study on human fertility (*5*) showed a decline in hot months. Others [see (*6*)] have noted a continuation of the decline in human male sperm density in industrial nations first noted 30 years ago; sperm counts have dropped from 90×10^6 per milliliter of semen in 1929 to 60×10^6 in 1974 (20×10^6 is functional sterility). Reason suggests that we examine any phenomenon that can disrupt mammalian reproduction and especially one of global magnitude.

I did not "correctly point out" (*1*, paragraph 2) that CO_2 was injected into the oceans. My mechanism [also for Cornell and LeMone (*7*), paragraphs 3 and 4] for the terminal Mesozoic $CaCO_3$ solution event, and initial "greenhouse" conditions is: In the oceans, organic particulates settle gravitationally, "pumping" CO_2 into the deep oceans and thereby maintaining relatively low atmospheric levels; failure of the pump today would increase atmospheric CO_2 by severalfold (*4*). About 500,000 years before the end of the Mesozoic terrestrial extinctions (*8*), extinctions among the abundant marine algae (coccolithophorids) would have severely disrupted the "pump." Carbon dioxide formerly taken from surficial waters and the atmosphere by photosynthesis would have accumulated there, triggering $CaCO_3$ solution and initial greenhouse conditions. Greenhouse heating of the oceans would, by reducing CO_2 solubility, have driven more into the atmosphere in a "chain reaction" of increasing oceanic surficial heating and CO_2 expulsion. The Maastrichtian oceans were seemingly less stratified than modern [Douglas and Savin (*9*) indicated that the top to bottom temperature gradient in low latitudes was about one-third its present value]; hence my suggestion that the deeper waters may have come into play, expelling some of their CO_2 and contributing to the solution and greenhouse phenomena.

I did not state (*1*, paragraph 3) that the modern oceans would "overturn." Quoting myself (*2*) [from Turekian (*10*) and Revelle (*11*), respectively] "the structure of the oceans can be maintained only if there is continuous supply of each type of water to give continuing definition to the water masses; the structure would be lost by the process of random mixing," and "if only 5 percent of the deep oceanic waters were to release their excess CO_2. . . ." Considering the slow deep circulation, one might assume release of potentially large amounts of CO_2 to involve centuries and not decades; however, how deep waters release CO_2 to the atmosphere is not completely understood.

The oceanic warm water lid (1, paragraphs 3 and 4), which is scheduled, according to theorists, to develop with global warming, is a surface phenomenon different from stratification induced from influence of cold polar regions. With warming of the climate, high latitudinal regions will theoretically warm more rapidly than low, reducing wind speeds and thus wind stress on surficial waters (12), possibly decreasing upwelling and vertical convection; it may also (13) raise the bottom of the lighter oceanic water making it more accessible to mixing, and affect levels from which upwelled waters are drawn. This scenario is controversial.

The paleotemperature data and discussion by Cornell and LeMone (7, paragraphs 2 and 5) do not focus on the time of the extinctions. Synthesis of marine isotopic temperatures by Margolis *et al.* (14) indicate "the Tertiary/Cretaceous boundary appears actually to fall during a time of significant global warming of bottom and surface waters, which lasted until the Early Eocene." Savin (15), on temperature histories of the South Atlantic and North Pacific, noted "isotopic temperatures recorded by both planktic and benthic fossils dropped by a few degrees in early to middle Maastrichtian time, and then recovered. . . ." "The drop occurred prior to the end of the Maastrichtian and hence earlier than the great extinctions of the Cretaceous-Tertiary boundary." However, Savin is careful to cite sampling problems associated with the boundary interval.

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References and Notes

1. J. F. Black, *Science* **206**, 1429 (1979).
2. D. M. McLean, *ibid.* **201**, 401 (1978).
3. _____, *Geol. Soc. Am.* (abstract, Southeastern sectional meeting, 1979), p. 205.
4. *Energy and Climate* (National Academy of Sciences, Washington, D.C., 1977).
5. N. J. K. Lamar and R. Rodgers, *Anat. Rec.* **87**, 25 (1943).
6. R. C. Dougherty, address to American Chemical Society, Washington, D.C., 9 to 14 September 1979; _____, M. J. Whitaker, S. Tang, R. Bottcher, M. Keller, D. W. Kuehl, *Environ. Health Chem.*, in press.
7. W. C. Cornell and D. V. LeMone, *Science* **206**, 1428 (1979).
8. W. Lowrie and W. Alvarez, *Geophys. J. R. Astron. Soc.* **51**, 561 (1977).
9. R. C. Douglas and S. M. Savin, *Init. Rep. Deep Sea Drill. Proj.* **17**, 591 (1973).
10. K. K. Turekian, *Oceans* (Prentice-Hall, Englewood Cliffs, N.J., 1968), p. 80.
11. R. Revelle, in *Man's Impact on the Climate*, W. H. Matthews, W. W. Kellogg, G. D. Robinson, Eds. (MIT Press, Cambridge, Mass., 1971), p. 282.
12. R. Revelle, address to workshop on Environmental and Societal Consequences of a Possible CO_2-Induced Climate Change, Annapolis, Md., 2 to 6 April 1979.
13. R. Stewart, in *ibid.*
14. S. V. Margolis, P. M. Kroopnick, D. E. Goodney, *Mar. Geol.* **25**, 131 (1977).
15. S. M. Savin, *Annu. Rev. Earth Planet. Sci.* **5**, 319 (1977).

17 October 1979

Psychophysical Functions and Regression Effect

Moyer *et al.* (1) commit an error of statistical concept which, while it seems to have no important bearing upon the validity of their conclusions, seems worth pointing out in order to lessen the chance of its recurrence in other, less innocuous settings. Their error is one of overcaution, of fearing an artifactual regression effect in a situation in which none is possible and reacting to this fear by introducing an erroneous correction. Stripped to the statistical essentials, the situation they consider is one of comparing two regression lines fitted by least squares, the "perceptual" line $P_i = a_P + b_P X_i$ and the "memorial" line $M_i = a_M + b_M X_i$, the principal question being the comparison of the slope coefficients b_P and b_M. (Here, to focus upon their second experiment, P_i and M_i are, respectively, apparent perceptual and memorial magnitudes recorded in response to $i = 1,2,\ldots,48$ states with log relative areas X_i, by two independent groups of subjects.)

The investigators note that the squared correlation coefficients $r_{XM}^2 = .934$ and $r_{XP}^2 = .982$ are unequal, and fear a possible regression effect. In their reference 16 they suggest correcting for $r_{XM} \neq r_{XP}$ by comparing the slope b_M with $(s_P/s_X)r_{XM}$ instead of with $b_P = (s_P/s_X)r_{XP}$ (2). They give no further explanation for their correction, but I assume it is based upon reasoning such as the following: If the pairs (X_i,M_i) and (X_i,P_i) are considered as two sets of bivariate data, and the appropriate measures of the relationships between the variables were s_M/s_X and s_P/s_X, then it would be misleading to compare $b_M = (s_M/s_X)r_{XM}$ and $b_P = (s_P/s_X)r_{XP}$, if $r_{XM} \neq r_{XP}$. This view might be defensible if, say, the data were bivariate normal and the slopes of the major axes of the elliptical contours were the relationships of interest, but this is not the case here. The situation here is the classical regression situation (3) in which the experimenter selects the values of X, and the conditional expectations of M and P given X are the relationships of interest. In the given situation, these are estimated without bias by the least-squares lines $P = a_P + b_P X$ and $M = a_M + b_M X$, and no correction for a regression effect is needed.

In fact, a correction of the type attempted will introduce a bias. They suggest comparing b_M with $(s_P/s_X)r_{XM}$ instead of $b_P = (s_P/s_X)r_{XP}$. That this invalid can be seen by considering the extreme case where $r_{XM} = 0$ and memory serves only to destroy the signal, replacing it with noise. Their correction would suggest that perception and memory were equally unreliable, even if $r_{XP} = 1$! Rather than viewing $r_{XM} \neq r_{XP}$ as a cause for concern, it should be viewed here as reassuring. If the two least-squares regression lines fitted to the same X values fit equally well [as judged by their respective residual sums of squares (4)] and have equal correlations, then they must have identical slopes (5).

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References and Notes

1. R. S. Moyer, D. R. Bradley, M. H. Sorensen, J. C. Whiting, D. P. Mansfield, *Science* **200**, 330 (1978).
2. The notation here differs slightly from that of (1). Here $s_X^2 = (n - 1)^{-1} \Sigma(X_i - \overline{X})^2$, $s_M^2 = (n - 1)^{-1} \Sigma(M_1 - \overline{M})^2$, $s_P^2 = (n - 1)^{-1} \Sigma(P_i - \overline{P})^2$, and r_{XM} and r_{XP} are the sample correlation coefficients of the (X_i,M_i) and (X_i,P_i), respectively.
3. See, for example, K. A. Brownlee [*Statistical Theory and Methodology* (Wiley, New York, 1965), p. 409] for a discussion of the relationship between regression lines and bivariate distributions.
4. Indeed, one of the unstated assumptions underlying the validity of the t-test employed in (1) comparing b_P and b_M is that the error variances are equal for the two lines, in which case the residual sums of squares would have equal expected values, the sample sizes being equal.
5. The residual sums of squares are just $(n - 1)\, s_P^2 (1 - r_{XP}^2)$ and $(n - 1)\, s_M^2 (1 - r_{XM}^2)$. If these are equal and $r_{XP} = r_{XM}$, we must have $s_P^2 = s_M^2$. Then $b_P = (s_P/s_X) r_{XP}$ and $b_M = (s_M/s_X) r_{XM}$ are equal, too.

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We thank Stigler for his comment on our reference 16, and wish here simply to echo his statement that his observation has no bearing on the validity of our conclusions.

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