line in figure 1 of (*2*). Chakraborty and Kidd refer to more than 2000 U.S. court cases that have employed DNA evidence [(*2*), reference 4]. This is misleading, as the majority are paternity cases in which the DNA types of mother, child, and putative father are usually available for comparison. Chakraborty and Kidd compare binning the variable number of tandom repeat (VNTR) sequences with the grouping of A_1 and A_2 blood type alleles. This comparison is inappropriate because the problem with binning VNTRs is not that alleles are grouped but that sometimes they are assigned to the wrong bin. Their statement that the "worst case" scenario is an equal mixture of Poles and Italians is incorrect—it is actually the best case for their argument. Their statement [(*2*), p. 1737] that the "arithmetic and underlying principles are identical" for linkage equilibrium and Hardy-Weinberg equilibrium is simply incorrect. In a quote from our article, they also did not include an important qualifier present in our original text [(*2*), reference 6].

Wills states in his letter that our arguments are based on "old" blood group data. Why Wills would disregard reliable blood group data is unclear, because even Chakraborty and Kidd concede their relevance. Wills also cites a high mutation rate among some VNTRs as the basis of a "mutational churning process" that is "a very large reason for the relative uniformity of allele frequencies from one human group to another. . . ." This hypothesis "explains" what has not yet been shown to exist, as relative uniformity of allele frequencies is precisely the point in dispute. We see no evidence of VNTR "churning" in French and Israelis or in the South American Indian tribes studied by Kidd *et al.* (*6*) [(*1*), p. 1749]. Wills also makes much of the observation that one of our subheadings (*1*) is identical to the title of a 1902 pamphlet by V. I. Lenin. This coincidence has no more relevance to DNA typing than the fact that Wills' letter makes favorable reference to Japanese automobile companies.

Austad concedes the validity of our arguments, but points out that DNA typing is certainly better than polygraph results, so why all the fuss? His argument seems to be that new sources of scientific evidence should be held to a standard of reliability no greater than methods currently in use. We would make a fundamental distinction between the intrinsic limitations of a technology and limitations imposed by the use of false assumptions, particularly when simple alternatives are available. For example, although polygraph examinations have a high intrinsic error rate, we suppose that Austad would object to a test in which an electrical short in the machine produced additional erratic readings. Our view is that erroneous assumptions about genetic uniformity among ethnic groups are no more necessary to DNA typing than electrical shorts are necessary to polygraph machines.

We would finally like to emphasize that this dispute is not about the use of DNA evidence in the courtroom. DNA typing is a very powerful procedure. We regard it as "possibly the most powerful innovation in forensics since the development of fingerprinting in the last part of the 19th century" [(*1*), p. 1746]. All we ask is a basic degree of candidness in reporting the statistical significance of a match. With databases as large as $x = 10{,}000$, why not use $1/x$ as a conservative estimate of the probability [(*1*), p. 1749]? After all, 0.0001 is already a pretty small number. Why invoke unsupported assumptions in order to obtain a still smaller probability that is exaggerated and unreliable? Perhaps it is because the organizations whose interests are served by numerical exaggeration have also been in charge of choosing the statistical procedures.

RICHARD C. LEWONTIN
Museum of Comparative Zoology, Harvard University, Cambridge, MA 02138
DANIEL L. HARTL
Department of Genetics, Washington University School of Medicine, St. Louis, MO 63110

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Erratum: In the abstract and in the text (line 35 in the middle column of page 185) of the report "Electrical resistivity and stoichiometry of K_xC_{60} films" by G. P. Kochanski *et al.* (10 Jan., p. 184), the minimum resistivity was given incorrectly as 2.2 microohm-cm. The correct value is 2.2 milliohm-cm.

Erratum: In the News & Comment article "Is homosexuality biological?" by Marcia Barinaga (30 Aug., p. 956), it was suggested incorrectly that the suprachiasmatic nucleus is not part of the hypothalamus.

Erratum: The Table of Contents for the issue of 31 January 1992 (p. 508) incorrectly listed a letter by J. Bello as appearing in the Letters section beginning on page 514. The letter appeared in the issue of 14 February on page 784.

Erratum: In figure 1 (p. 1509) of the Research Article "Radar images of Mars" by D. O. Muhleman *et al.* (27 Sept., p. 1508), the Mars longitude of the sub-Earth point was mislabeled in each of the six snapshot radar images of Mars. None of the labels should have contained a decimal point. The values of λ in the labels should have been 78, 92, 104, 120, 133, and 147, respectively.