utilization patterns in service delivery by Daschle, Cohen, and Rice (p. 267). In discussing unnecessary or unwise health services utilization, they suggested comparing such users with those "[on] the other end of the curve—those whose outcomes are better" in order to receive instruction on more economical service utilization. These authors are apparently misinformed on the distribution curve nature of utilization of health services.

Most research bearing on health and mental health delivery systems has noted the presence of a negatively accelerating, declining "decay" curve with known empirical–statistical parameters (Phillips, 1985, 1987, 1988; Walberg, Strykowski, Rovai, & Hung, 1984). Walberg et al. have noted similar curves in biology, psychology, and sociology, where the so-called decay curves "right or [have noted] positive skew distributions in which low or null occurrences are most frequent and high performance is rare" (p. 87). In psychotherapy and medicine, respectively, the decay curve describes attrition from treatment or noncompliance with aspects of treatment. Walberg et al. (1984) observed that "the 'normal' distribution often will be a very poor model of reality leading to misguided educational theories, inferences, policies, and practices" (p. 88).

The bell-shaped, normal curve has two asymptotes; the decay or positive skew curve has one. This curve has been elaborated into a total "service delivery system" curve, following from attritional and noncompliance data on a large variety of health-related services (see Walberg et al., 1984; Phillips, 1985, 1987, 1988).

Presumably improved planning for and utilization of all health-related services will be better served if the correct nature of their distributions and resultant curves are fully recognized and taken into account from the inception of all data collection and all outcome evaluation studies.

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No More Than Skin Deep

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Helms's (September 1992) article claiming that racial differences in IQ performance are culturally biased postulated all kinds of cultural differences between American Whites and Blacks. She cited Heath (1989) to suggest that Blacks are "socialized in Black communities to develop spontaneous, creative, interactive, and expansive thinking [and that]. . . . it is difficult for them to reconcile the contrasting socially oriented worldviews of their communities with the ascetic Eurocentric view that presumably underlies test construction" (p. 1097). Despite a greater familiarity of Black children with MTV and baseball's world series than with Senegalese or Kenyan customs, Helms imagined that these children possess all kinds of "African-centered" values (e.g., "immaterial forces over linear thinking, personal conduct through movement, time measured by customs" [p. 1096]). One is left with an impression that people of black skin are culturally, cognitively, and socially different from people of white skin.

Nevertheless, IQ tests appear to measure the same psychological attribute in American Whites and Blacks (Jensen, 1980). The statistical regression lines relating IQ with later college grades or job performance, according to many studies, are nearly identical across racial groups (Cole, 1981; Jensen, 1980; Schmidt & Hunter, 1981). In cases of slightly unequal IQ regression lines (in large samples), the White line usually overpredicts Black performance on outcome criteria to a small degree. Thus, if an IQ of 90 predicts a course grade of C-, then this predicted grade would apply for an American Black or White child, regardless of whether one child was raised in poverty in a city and the other in affluence in a middle-class suburb. Furthermore, IQ almost never underestimates (at least over the short term) the intellectual performance of a Black child (Cole, 1981). Other psychometric findings also support the "same attribute" conclusion (e.g., equal rank orders of IQ item difficulties across racial groups; Jensen, 1980).

In her article, Helms failed to come to grips with these observations: If Black intelligence is somehow different from White intelligence, how could these data patterns be obtained? If IQ measures Eurocentric and not Afrocentric "g," then how does it hold the same network of correlates in Black and White groups? Just because in America in the 1990s fewer Blacks than Whites score above IQ 100 does not mean that Black and White thought processes are dissimilar (nor does it imply that IQ tests measure the sum total of intelligent behavior in either Blacks or Whites).

Basic similarity of thought can also be seen in more molecular analyses of behavior. All children advance through the early Piagetian stages of thought at approximately the same rate. Black children, like White ones, discover that objects are permanent and that quantities are conserved. Consider that *African* Black children and American White children learn mental addition in elementary schools. When they do, they develop similar problem-solving strategies and make similar mental errors (Ginsburg, Posner, & Russell, 1981). Would we really expect the serial position effect in memory to be reversed in one ethnic or racial group—so that items in the middle of a list are recalled more easily than early or late items? Dissimilarity of skin color is no guide to how people reason and think.

Racial and ethnic similarity may extend to domains of psychological development beyond IQ, such as to the development of behavioral deviance. The IQ regression results mentioned above show that 2 x 2 covariance matrices (say IQ, grades) are equal in Blacks and Whites. With two colleagues I found that much larger covariance matrices (approximately 10 x 10) consisting of psychosocial influence and outcome variables were also equal across ethnic and racial groups (Blacks, Hispanics, Asians, and Whites; Rowe, Vazsonyi, & Flannery, in press). Our findings imply that many psychosocial influences operate in different ethnic and racial populations in quantitatively the same way.

Helms also presented misleading genetic information. She advocated the controversial idea that the ancient Egyptians were racially the same as sub-Saharan Africans. Her view is rejected by many physical anthropologists and historians. She confused the concept of genetic recessiveness with that of Genotype x Environment Interaction. Furthermore, she failed to appreciate the degree of racial *similarity* of genetic composition. According to Lewontin (1982), approximately 85% of genetic variability is found "between people within a nation or tribe" (p. 123), whereas approximately 6% is "between races" (p. 123). True, if some genes affecting behavior have racial prevalence differences, such as genes affecting skin color, then they could contribute to *average* behavioral differences among racial groups—but they could not redesign pan-*Homo sapiens*'s emotional and cognitive adaptations represented in the functioning of the nervous system.

The psychological similarity of Americans of different ethnic and racial groups is supported by (a) sharing many features of a common American culture, to which all ethnic and racial groups have contributed, and (b) sharing our species' genetic blueprint. Nonetheless, I endorse Helms's and Betancourt and Lopez's (1993) calls for adequately defining culture and attempting to quantify the amount of relevant culture to which Black and White individuals are exposed. More generally, by using measured biological, psychosocial, and cultural variables in research, it may be possible to identify the sources of ethnic and racial (to the extent they exist) differences. We may find basic developmental similarities, even when mean level differences exist among ethnic or racial groups. For example, the regression of maternal drug use on low birth weights may be similar in different ethnic groups. If so, average group differences may arise from different prevalences of drug abuse (see Vega, Called, Hwang, & Noble, 1993). As implied by my title, ethnic and racial differences may be "no more than skin deep" because physical dissimilarities lead to false attributions of psychological difference, when in reality people of different ethnic and racial backgrounds are broadly psychologically alike.

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Sample Description, Reporting, and Analysis of Sex in Psychological Research: A Look at APA and APA Division Journals in 1990

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Sexually biased language has been virtually eliminated from American Psychological Association (APA) publications, but how is psychology doing with regard to the deeper issues of sex fairness in research? Are both men and women appropriately sampled in research design, and is the sexual composition of samples fully reported? How is sex of participants dealt with descriptively, analytically, and in discussion? A recent study by Gannon, Luchetta, Rhodes, Pardie, and Segrist (1992) examined the sex-of-participants reporting practices of eight psychology journals in the 20-year period 1970–1990. In 1970, 8% to 25% of studies in these journals did not report sex of participants; in 1990 nonreporting ranged from 0% to 34%. For most journals, the percentage of male-only studies declined over time. No difference in the sexual composition of samples associated with sex of first author was found. Gannon et al. concluded that significant improvements had occurred but that discriminatory practices continue. Our study extends the work of Gannon et al. by sampling from all APA journals publishing original human studies research in 1990 and by sampling all APA division journals publishing such research.

We examined two issues of each APA and division journal published in 1990. Each original research report using human participants was reviewed; theoretical articles, review articles, and animal research articles were excluded. This approach yielded a sample of 506 articles from 26 journals. Data were recorded individually for each study or experiment reported in an article; the number of studies reported per article ranged from one to nine. When the unit of study in an article was a family (at least one parent and at least one child), data were recorded separately for the parent(s) and child(ren), yielding two "cases" per study. Twenty-four such studies were identified. The 506 articles yielded a total of 801 studies. Deborah N. Ader evaluated all studies; in addition, Suzanne Bennett Johnson evaluated 25 randomly selected articles to obtain an estimate of the reliability of this procedure. Interrater reliability was assessed using Cohen's kappa; coefficients ranged from .68 to 1.0.

For the full sample, 79 cases clearly indicated using participants of only one sex (9.86%), 440 (54.93%) cases used both male and female participants and reported sex fully, and 44 cases (5.49%) indicated using participants of each sex without reporting exact numbers. Two hundred thirty-eight (29.71%) did not report sex of participants. For purposes of all further analyses, both incomplete reporting and nonreporting categories were combined into one group representing studies failing to quantitatively report sex of participants.

Table 1 provides reporting rates for all journals sampled. For APA journals, the lowest reporting rate was found in the *Journal of Experimental Psychology: Learning, Memory, and Cognition* (only 29% of 96 studies reported sex); the highest reporting rate was 100%, in the *Journal of Counseling Psychology*. Among division journals, the lowest reporting rate was found in *Psychotherapy* (33% of 6 studies reported sex); reporting rates of 100% were found in the *Psychology of Women Quarterly* and the *American Journal of Community Psychology*.

In studies reporting the number (or percentage) of participants of each sex represented, the average sample was 52.5% female and 47.5% male. Sex was reported most often when participants were elderly (89.7%), children (82.1%), or adolescents (80.8%), and least often when participants were undergraduates (47.8%). Descriptive data by sex were reported in only 12.6%, and sex analyzed in 27.3%, of cases that were not reported as single-sex studies. Descriptive data by sex were more likely to be reported for adolescent and least likely to be reported for undergraduate research participants; analyses by sex were most likely to be reported for undergraduates and nonelderly adults. Sex of participants as a factor was treated in the discussion section in only 100 cases (14% overall, 35% of cases reporting analysis of sex).

Of the 79 reported single-sex cases, 26 (32.9%) used male participants only; 53 (67.1%) used female participants only. Sex of participants was clear from the title in only 40.5% of these studies and clear from the abstract in 54.2%. A rationale for using the sex studied was lacking in 47.4% of cases. The limitation of generalizability to the sex not studied was mentioned in only 11.1%. A pattern emerged when these data were analyzed by sex of participants stud-