

DUBIOUS DATA: The Thernstroms on Race in America

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ABSTRACT: *America in Black and White by Stephen and Abigail Thernstrom has become one of the bibles of the movement to abolish affirmative action. The book, however, seriously misinterprets and misuses evidence to make its case. This critical analysis focuses on the Thernstroms' errors in their discussion of housing, education, higher education, politics, crime, and employment.*

In the raging debate over affirmative action in the United States, Abigail and Stephen Thernstrom have become leading spokespeople for those who would do away with all race-conscious government policies. Invited to meet with the President to discuss his race initiative and sought after by journalists, the Thernstroms are the authors of the recently published *America in Black and White: One Nation Indivisible: Race in Modern America* (1997a),¹ which has become something of a bible for opponents of affirmative action.

The Thernstroms assert (1997b) that their book does not seek "to score points" but "to explore difficult issues in all their complexity." Nothing could be further from the truth. *America in Black and White* is a polemic, but it is a polemic with many footnotes. "One of the most distinctive features of the book," claim the Thernstroms (1997b), "is that it provides the reader with no fewer than 76 tables that include often unpublished or difficult-to-find economic, social, and demographic data. Even our most severe critics should find them extremely useful." Their data, however, are frequently misleading, incomplete, or inaccurate. But because their book is being so widely quoted, the Thernstroms' arguments and their data need to be carefully scrutinized.

In what follows, the Thernstroms' discussion of housing, education, higher education, politics, crime, and employment will be examined.

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HOUSING

In 1968 the Kerner Commission declared that the United States was moving toward "two societies, one black, one white—separate and unequal." But the Kerner Commission, claim the Thernstroms, could not have been more wrong. "However you measure it," declare the Thernstroms, "residential segregation has been declining in the United States for the past quarter century" (p. 219), and a few pages later they proclaim that "[b]y every possible measure—and we have examined several—the residential separation of blacks and whites in the United States has diminished substantially over the past three decades" (p. 224). The truth is that by some measures segregation has been declining and by some it has been getting worse, but the Thernstroms confine themselves to measures that paint an optimistic picture.

Massey and Denton (1989) proposed that since residential segregation has many dimensions, it makes sense to use multiple measures, each capturing a different aspect of what segregation entails. The index of dissimilarity tells us how evenly spread out members of a group are among the population. The index of isolation indicates the degree to which the typical member of a group comes into contact only with members of the same group. Other indexes measure clustering, concentration, and centralization. Massey and Denton found that in 1980 African Americans in sixteen large metropolitan areas were "hypersegregated"—highly segregated on at least four of these five dimensions; 35 percent of all U.S. blacks lived in these hypersegregated areas (Massey and Denton, 1993, pp. 76–77). By 1990 the number of hypersegregated areas had increased to 21, encompassing 39 percent of U.S. blacks.²

While there has been some decline over time in the dissimilarity indexes within metropolitan areas—though not as much as the Thernstroms claim³—the dissimilarity has been increasing within county and city boundaries (Massey and Hajnal, 1995). Because resources are typically allocated along these county or municipal lines, the fact that blacks and whites are increasingly living in separate jurisdictions augurs badly for the future.

There are data—not presented or discussed by the Thernstroms—on the black isolation index in large metropolitan areas in 1980 and 1990. Because of changes in boundary lines not all the metro areas are strictly comparable, but for the 55 with reasonably comparable data, black isolation *increased* over the decade, whether one uses a simple average, or, more appropriately, an average weighted by the black population (Massey and Denton, 1989, pp. 378–379; U.S. Bureau of the Census, 1997a). One might note that it is the level of black isolation, not the index of dissimilarity, that correlates strongly with such ills as urban violence (Shihadeh and Flynn, 1996).

The Thernstroms provide a table showing the proportion of African Americans living in block groups (a census category averaging about 600 people) that were at least 90 percent black in selected metropolises in 1980 and 1990. If we use a block that is 90 percent black or more as a definition of a ghetto, say the Thernstroms, then "by 1990 a substantial majority of the black residents of these metropolises no longer lived in ghettos" (p. 216).⁴ The Thernstroms state that "[o]ver the course of the 1980s, segregation by this measure did not increase in a single one of these metropolises, and it dropped in all but two of them (Cleveland and Philadelphia)." But the table in the original source shows *four* metropolitan areas in which ghettoization *increased* (leaving aside the two increases of less than 0.5 which the Thernstroms rounded to zero). Two of the metro areas with

increases—Buffalo and Cincinnati—the Thernstroms omitted from their table. And two of the increases disappeared as the Thernstroms simply reversed the sign of the change, switching an increase into a decrease.⁵

Nationwide, in 1990 30 percent of all African Americans lived in block groups that were at least 90 percent African American. In 1980, the figure had been 34 percent—“almost identical,” in the words of one of the authors of the original study (Doig, 1991). Some metropolitan areas did show substantial decreases in the ghettoization measure, but, as Doig (1991) points out, some of the “improvement” may “not be real,” reflecting rather an influx of poor Hispanic immigrants, who appear in the study as white, into once predominantly black neighborhoods. For example, a study of the New York City region (Alba et al., 1995) found that while there have been a growing number of census tracts with multiple racial/ethnic groups, there has also been an increase in the number of tracts that were either all black or mixed black and Hispanic, with one third of all African Americans in the New York area living in such neighborhoods. These mixed black-Latino tracts are not better off than the all-black ones; in fact their per capita income is only half as high as in the all-black areas. Thus, rather than suggesting that the United States has gotten over its color problem, the growth of these neighborhoods shows that impoverished Latinos are joining impoverished African Americans in urban ghettos.

The Thernstroms are correct that there has been growth in black suburbanization. Some of this represents the spread of the ghetto across the city line and some reflects the growth of more integrated middle-class suburbs. The implication of this development for the country’s urban areas, however, confirms the Kerner Commission’s grim prognosis: the “accelerating segregation of low-income, disadvantaged Negroes within the ghettos of the largest American cities.” Economic segregation for blacks and Latinos has been increasing, sharply so in the 1980s, and especially among blacks (Jargowsky, 1996). A recent report by the Department of Housing and Urban Development (1997, pp. 33–34) noted that “[m]ore than 10 percent of all city residents live in neighborhoods where . . . 40 percent or more of the households are living below the poverty line, doubling the concentration in 1970.” In 1970, “only 6 percent of all census tracts in central cities were high poverty tracts; by 1990, this had more than doubled to 14 percent.” And who lives in these areas of concentrated poverty?

Tracts with high poverty rates are almost exclusively inhabited by minorities. Indeed, minority families are far more likely than whites to live in high-poverty neighborhoods even if they are not poor themselves. Almost one in four black and Hispanic residents of central cities live in census tracts where more than 40 percent of their neighbors are poor, while just 3 percent of white urban families live in such areas.

The Kerner Commission was not so far off.

Remarkably, after their selective effort to minimize the reality of residential segregation, the Thernstroms acknowledge that “it cannot be denied that residential patterns in the United States today remain closely connected to race” (p. 219). Could this be related to discrimination?

Audit studies—where matched black and white testers pretend to be seeking a new place to live—suggest substantial housing discrimination. The Thernstroms summarize the results of a 1989 study, but they dismiss the figures, arguing that they “do not seem

high enough to support the claim that patterns of exclusion are the norm rather than the exception" (p. 225). But this objection is bizarre. The issue is not whether discrimination is "the norm" or occurs more than half the time, but whether it might have a substantial impact on residential patterns. It hardly seems irrelevant that 6 percent of the time that a qualified black homebuyer goes to a realtor, he or she is told that nothing is available—even though the same realtor showed at least one house to a matched white buyer, or that 11 percent of the time a qualified black renter is similarly excluded, or that on average black testers were shown a quarter fewer units than their white counterparts (Yinger, 1995, pp. 34–36). It is true that in real life black and white housing customers are not equivalent (whites are richer), but it is also true that many housing units are never even given to realtors, precisely as a way to facilitate discrimination (Yinger, 1995, pp. 27–28). And there are many other sources of discrimination in the housing market that are not discussed at all by the Thernstroms: bias in mortgage lending, redlining, zoning ordinances used to keep low income (read minority) people out of white suburbs, discriminatory referral services, and violence—odd omissions for a book purporting to be a comprehensive analysis of the state of race relations.

Instead of considering any of these issues, the Thernstroms attribute residential segregation to the preferences of private individuals, black and white. Both groups favor integrated neighborhoods, but, note the Thernstroms, by integration whites mean situations where blacks make up a small portion of the population, and blacks increasingly mean neighborhoods that are at least half black. Thus, people simply follow their preferences and the result is segregated housing. Even if blacks had first choice, say the Thernstroms, the result would be substantial segregation. If blacks were twenty percent of a metropolis and settled themselves in neighborhoods so that they were fifty percent of the local population, the index of dissimilarity would be very high, higher than that of Washington, DC (pp. 226–229). There are four things wrong with this analysis, however.

First, the Thernstroms derived black preferences from 1976 and 1992 surveys in Detroit that asked respondents to indicate their first and second choices of five different hypothetical neighborhoods: those where the black proportion was 0, 14, 50, 72, or 100 percent of the total. If one preferred a mixed neighborhood, the first choice would be 50-50, but the next most-balanced pattern would be 72-28. In other words, the options are not symmetrical and encouraged more respondents to choose the majority black neighborhood over the majority white neighborhood.

Second, three out of five Detroit-area blacks lived at the time in neighborhoods that were over 90 percent African American (Gillmor and Doig, 1992, p. 50), so both the 50-50 and the 72-28 choices indicated a preference for less segregation.

Third, the Thernstroms treat black preferences as closed-minded, rather than as a rational response to racism. In the 1976 survey, those African Americans who said they had reservations about living in largely white neighborhoods were asked why, and nine out of ten said whites would be unfriendly, make them feel out of place, or even use violence. The Thernstroms discount these responses, saying that since whites were friendlier towards integration in 1992, black fears should have been less, suggesting that such worries could not have been the main deterrent to integration (p. 228). But many of Detroit's blacks may have concluded that if whites still weren't ready to accept them more than two decades after the Open Housing Act, it wasn't worth the struggle.

Fourth, the racial mix that whites are willing to accept is not an innate preference, but

a function of the mix in other neighborhoods. "If all neighborhoods were integrated, not even the most prejudiced white would have an incentive to move out of a neighborhood because a black family moved in" (Yinger, 1995, p. 120; see also Massey and Denton, 1993, p. 97). So individual preferences and behaviors will depend on societal patterns of discrimination and segregation.

The claim by the Thernstroms that black preferences alone would generate as much segregation as now exists in many major metropolises suffers one further defect. While it is true that the index of dissimilarity in their hypothetical example above is high, the other measures of segregation show a very different result. Thus, (as the Thernstroms acknowledge in a footnote [p. 589n48]) the fraction of the black population in their hypothetical metropolis living in ghettos would be *zero*, a figure rather lower than actually exists. And the isolation index would be considerably below that of New York, Los Angeles, Chicago, or Washington, DC (U.S. Bureau of the Census, 1997a).

A good test of how much black preferences explain housing segregation is provided by a study of Los Angeles residents (Zubrinisky and Bobo, 1996) which found that the black-white index of dissimilarity was higher than the Latino-white and the Asian-white indexes, not because minority preferences differed very much, but because whites were less tolerant of living near blacks than near the other groups. In other words, it was white preferences, not black, that accounted for black segregation levels.

The Thernstroms are quick to cite any survey data—no matter how dubious—that seem to suggest that white prejudice is a thing of the past. But they don't mention the survey data showing that three out of eight whites favor a law allowing homeowners to refuse to sell to blacks (NORC, 1994).⁶ Nor do they note that in a survey in Los Angeles, more than a quarter of African Americans (compared to fewer than 10 percent of both Latinos and Asians) reported that they had been discriminated against in looking for housing (Zubrinisky and Bobo, 1996, p. 352). And nor do they tell us that according to this same survey more than three quarters of *whites* stated that they believed that whites "very often" or "sometimes" won't rent or sell homes to blacks, and three out of five whites do not think it rare that real estate agents refuse to show or sell housing to blacks or that banks won't lend them money (Zubrinisky and Bobo, 1996, p. 354). One suspects that these whites know more about these matters than do the Thernstroms.

EDUCATION

There is a serious gap in academic performance between blacks and whites. What accounts for this gap and what can be done about it?

Anyone who has ever been inside a school in a typical U.S. inner city would have no trouble proposing a hypothesis for why the achievement of blacks lags behind that of whites: namely, that black and white students are not getting equal educations. The Thernstroms, however, promptly dismiss any thought that unequal school spending might be a cause. They cite a study done by the National Center for Educational Statistics that they say found that "the higher the percentage of minority students in a school district, the *higher* the level of spending, even after differences in costs of living and other variables were held constant" (p. 351). Their summary of this study, however, is quite deceptive.

The spending disparity study (NCES, 1995a) presents its findings on school spending by districts across the United States in three different ways. First, it gives actual per

student expenditures. Second, it gave per student expenditures but corrected for the different cost of living in different places and the added cost of teaching special needs and at risk students; data are provided on each of these cost adjustments separately and combined. And, third, it gives per student expenditures controlling for a host of other variables, such as poverty and urbanicity, with and without a correction for different costs. What the Thernstroms' summary of the study conveniently omits, however, is that the second of these three methods of presenting the results found the *precise opposite* conclusion from the one claimed by the Thernstroms: namely, *when considered in terms of buying power, districts with the highest percentage of minority students spend the least* (NCES, 1995a, p. 15; see also NCES, 1996b). And of the three ways to present the data, the one that shows the conclusion that the Thernstroms skip over is in fact the most meaningful.

That things cost more in certain places than in others is a truism apparent to any social analyst. That more dollars are spent per student in New York City than in rural Oklahoma tells us nothing about the actual educational resources available to the students in the two areas. As the authors of the study explain, to "allow meaningful comparison of revenues and expenditures per pupil across districts, it is important to convert . . . nominal amounts (actual dollars) into amounts that reflect real purchasing power (cost-adjusted dollars)" (NCES, 1995a, p. D-10).⁷

The spending disparity study made further adjustments to account for different levels of student need. School systems are required, for example, to provide extra services to special education students and so the study included an adjustment for the percent of special education students in the district. Another adjustment was for "children in poverty"—which might seem an inappropriate correction, but note that among the funds counted in a district's spending are those for school lunches provided to low-income students (NCES, 1995a, p. D7). If two school districts each spend \$5,000 per student, but in one of them a few hundred dollars goes to providing lunch, the *educational* resources available per student will not be equivalent. In any event, however, districts with more minorities spend less whether one adjusts for cost of living alone or cost of living plus student need (NCES, 1995a, p. A-5).⁸

The third method that the spending disparity study uses to present its results is controlling for all other variables. Such controls are appropriate if one is interested in investigating whether funding disparities are driven by race rather than poverty or urbanicity or some other variable, but they are meaningless if one wants to know whether minority students get adequate resources.

Thus, of the three modes of data presentation in the study, the only one that makes sense for our purposes is the one in terms of buying power, and this shows that minority districts spend less. Note, however, that the Thernstroms do not make a contrary argument, asserting that one of the other methods of data presentation is more meaningful. Instead they simply hide the fact that a buying power figure is even presented and that it shows results opposite from what they claim.

In addition, one should keep in mind that inter-district spending inequality is not the only source of black-white spending disparities. There is evidence that *within* districts, schools with a high proportion of minority students get less, and *within schools*, minority students get less (see, e.g., Darling-Hammond, 1998; Kozol, 1991, chap. 3). Thus, analyses of inter-district spending understate the racial disparity.

Analysts have long debated whether money is correlated with educational outcomes. The real question, though, is whether black students are getting equal educational inputs and these are not the same as dollars per student. Consider two adjacent school districts paying the same teacher salaries. The rich white district will be able to attract teachers with stronger credentials than will the poor minority district, with its higher crime rate and more troubled students. The money spent per student will be the same, but the educational input per student will be far different.

This is precisely what the data show. Thus, for example, the more poor students in a school, the less likely it is that their core subject teachers majored, minored, or were certified in the classes they are teaching (NCES, 1996a, table 57-2). In schools with the highest minority enrollments, students have less than a fifty-fifty chance of getting a math or science teacher with a license and a degree in the field in which he or she is teaching (Darling-Hammond, 1996; see also Orfield et al., 1996, pp. 67–70).

There are other potential sources of the black-white achievement gap: for example, the growing concentration of poverty for urban blacks, as mentioned above (U.S. Dept. of Housing and Urban Development, 1997, pp. 33–34), or the fact that districts with higher proportions of minorities tend to have higher student-teacher ratios (NCES, 1995a, p. A13), which have been shown to be inversely correlated with student achievement (Finn, 1998). None of these, however, get any consideration from the Thernstroms.

HIGHER EDUCATION

The Thernstroms argue that black college attendance has grown impressively, but the college *completion* rate of blacks has lagged seriously behind that of whites. The cause for this disparity, they assert, is affirmative action, which has led to unqualified African-American students going to colleges for which they are ill-prepared.

The Thernstroms report that 44.9 percent of blacks aged 25–29 in 1995 had attended college, but only 15.3 percent had obtained a bachelor's degree. For whites, the comparable numbers were 54.1 percent having attended college and 26.0 percent with bachelor's degrees. Thus, the ratio of black to white college attendance was .81, while the black to white bachelor's degree ratio was .59 (p. 391).

These figures, however, are misleading in a number of respects. The drop-off between the black college attendance rate and the black bachelor's degree rate does not represent, as the Thernstroms phrase it, “45 percent at the starting gate but only 15 percent reaching the finishing line” (p. 392). Many of those who attended college enrolled in two year institutions and got their associate's degree as intended. These individuals are not “drop-outs” in any sense of the word. While 15 percent of blacks reported that they had earned bachelor's degrees, 22 percent had earned a degree of some kind (U.S. Bureau of the Census, 1996b, Table 1). The ratio of black to white degree earning—bachelor's or associate's—was .65. Moreover, not everyone who takes a college course is seeking a degree. Some people just take a course or two with no intention of matriculating, let alone graduating.

That a 25–29 year old has some college but no degree does not, of course, mean that he or she has dropped out. Individuals often don't go straight from high school to college, or they attend part-time, and thus they may still have been enrolled at the time of the survey. Blacks are more likely than whites to delay their entry into college: of those who

were high school sophomores in 1980 who ever entered college before Spring 1992, 71 percent of whites entered immediately, while only 60 percent of blacks did so (calculated from NCES, 1996a, Table 26-1). In addition, a considerable number of students take more than six years to complete their bachelor's degree. Even though the Thernstroms assert that the number of students who have not completed their degrees within six years "cannot have been very many" (p. 636n12), of those students who graduated college in 1993, a quarter of whites and almost a third of blacks took more than six years to earn their degrees (NCES, 1996a, supplemental Table 11-1).

None of this should be taken to mean that African Americans do not have higher drop-out rates than whites, or that serious attention doesn't need to be directed to reducing these rates for both groups, but especially for blacks. But before accepting the Thernstroms' claim that affirmative action is to blame, we might consider some other possible causes. College students from poor families, regardless of race, are far less likely to earn a bachelor's degree; students who delay enrollment into postsecondary education by even a year are less likely to graduate; and, likewise, students who start at a community college, even if they intend to eventually get a bachelor's degree, have lower odds of getting such a degree within five years (NCES, 1996a, Table 10-1).

The Thernstroms doubt that money could have anything to do with black attrition rates because a "high proportion" of black college students "come from the greatly expanded black middle class" and anyway there's a lot of financial aid out there and blacks get a disproportionate share of it (pp. 392-393). But in 1992-93, a lower middle class family sending a child to a public four-year college *after financial aid* was expected to contribute \$5,158, and would still need another \$2,777 to fully cover costs (NCES, 1996a, Table 13-1). It is true that there is a lot of financial aid available, but it is clearly inadequate. While total Pell Grant spending increased by 10 percent between 1979-80 and 1995-96, the real value of the maximum Pell Grant award declined 35 percent over these same years (Kane, 1997, p. 337). Moreover, college costs are likely to be most burdensome to those families with few assets, and, as the Thernstroms acknowledge (pp. 197-198) but without any serious discussion of its implications, blacks are even worse off in terms of wealth accumulation than they are in terms of income. No theory of "academic mismatch" of black students can explain why only half of the top 25 percent of *all* high school graduates have earned a college degree within seven years of graduation (Dodge, 1991, p. A25). Obviously the failure to graduate on time has sources beyond affirmative action.

The Thernstroms claim that blacks are basically unprepared for college-level work. The proportion of whites with top SAT scores is about six times that of blacks (p. 399). Many educators note that exclusive reliance cannot be placed upon the SAT as a measure of a student's potential (see Sturm and Guinier, 1996). The Thernstroms argue, however, that the "racial gap in other measures of achievement and preparation is just as large as the gap in SAT scores" (p. 402). But their own data directly contradict this claim. The white advantage in SAT scores (6:1) is far greater than the white advantage in the percent of college-bound seniors with A averages (2.6:1), the percent of students in the top tenth of their class (1.9:1), the proportion of students taking year-long courses in six academic subjects (1.6:1), or the likelihood of taking an honors course (between 1.4:1 and 1.6:1) (calculated from p. 402, table 6).

The Thernstroms cite a study (p. 403) by Department of Education researchers that found that if five varied criteria are used, a black's chances of admittance to a highly

selective college is 0.4 percent, compared to 6.5 percent for a white. But this study (NCES, 1995b) does not calculate some composite index for admissions of the sort college admissions offices tend to use, where lower performance on one measure can be counter-balanced by higher performance on another. Instead, it uses rigid cutoffs for each criterion. The study's SAT criterion was attained by 2.6 percent of black students, so immediately 97.4 percent of black applicants were eliminated. Thus, this study took account of factors other than the SAT only for those students who made the SAT cutoff. This is not what those who urge using multiple measures of ability had in mind. In any event, however, this study confirms that the black-white gap is greatest for SAT scores and less for other measures. On 3 of the 5 criteria (that is, apart from SAT and GPA), white and black performance was almost identical (NCES, 1995b, Table 1).

The Thernstroms present a table (p. 408) of the SAT score gap between blacks and whites and the black-white gap in graduation rates at elite colleges. The drop-out rates, though, are not for the same students whose SAT scores are given. The Thernstroms claim this doesn't matter because at Berkeley, for which there is the best evidence, "the SAT gap and the gap in dropout rates changed very little between 1984 and 1994" (p. 408, source note to Table 9). The source they cite, however, gives no figures on dropout rates in 1984, stating only that Berkeley "officials say the differences [in black-white graduation rates] have narrowed over time" (Applebome, 1995, pp. I,22), a fact confirmed elsewhere ("Graduation Rates of African-American College Students," 1994, p. 44).

Elite universities enroll only a small proportion of all students, so the real question is what happens at the majority of institutions. The Thernstroms state (p. 409) that at "less selective institutions the overall dropout rate is higher and the black-white gap is correspondingly larger (Table 10)." When one looks at their table 10 (p. 410), however, one discovers that it shows that black graduation rates are correlated with SAT scores, but includes no information on white dropout rates and thus nothing on the black-white gap.

The source of their table, a study by Linda Datcher Loury and David Garman (1995), provides black and white data, broken down by student SAT score, for students at two kinds of colleges: those with median SATs of 900 and those with median SATs of 1,000. The top panel of Loury and Garman's table 4, given here as table 1, gives the data on graduation rates; the Thernstroms provided only the figures for blacks.

As can be seen, at the 900-median SAT institutions, black graduation rates were considerably higher than those of whites among those whose own SAT scores were over 700 and somewhat lower than those of whites for those whose SAT scores were under 700. At the 1000-median SAT institutions, black graduation rates were "slightly below" (Loury and Garman, 1995, p. 305) those of whites among individuals with SATs over 850 and sharply below among those with lower SATs.

It is crucial to note, however, that because the data are broken down by students' SAT scores, any difference in graduation rates cannot be due to admitting blacks with lower test scores than whites. Neither Loury-Garman nor the Thernstroms gives any evidence that affirmative action played any role in the admissions decisions of these institutions. On the other hand, because the Loury-Garman data refer to the high school graduating class of 1972, and because the Pell grant program which dramatically reduced the costs of college for the typical black student didn't kick in until 1974 (Kane, 1994, p. 882), it is highly likely that financial considerations were not irrelevant in explaining graduation rates.

What is the actual extent of preferential admission to college? Thomas J. Kane studied

TABLE 1
Expected Grades, Graduation Probabilities, and Earnings of Blacks and Whites by Own SAT Scores (Class of 1972)

<i>Variables</i>	<i>Attended College with Median SAT Score of 900</i>		<i>Attended College with Median SAT Score of 1,000</i>	
	<i>Whites</i>	<i>Blacks</i>	<i>Whites</i>	<i>Blacks</i>
Graduation probability:				
Own SAT \leq 700	.420	.379	.593	.259
Own SAT 701-850	.471	.560	.596	.389
Own SAT 851-1,000	.544	.769	.586	.518
Own SAT $>$ 1,000	.540	.769	.569	.518
Grade point average:				
Own SAT \leq 700	2.656	2.584	2.656	2.238
Own SAT 701-850	2.669	3.048	2.731	2.728
Own SAT 851-1,000	2.752	2.865	2.810	2.653
Own SAT $>$ 1,000	3.065	2.865	3.095	2.653
Earnings for dropouts:				
Own SAT \leq 700	\$325	\$324	\$329	\$323
Own SAT 701-850	\$314	\$355	\$320	\$356
Own SAT 851-1,000	\$317	\$339	\$322	\$350
Own SAT $>$ 1,000	\$327	\$339	\$332	\$350
Earnings for graduates:				
Own SAT \leq 700	\$343	\$422	\$347	\$420
Own SAT 701-850	\$332	\$462	\$337	\$464
Own SAT 851-1,000	\$335	\$441	\$340	\$455
Own SAT $>$ 1,000	\$345	\$441	\$350	\$455
Earnings for all workers:				
Own SAT \leq 700	\$357	\$385	\$364	\$371
Own SAT 701-850	\$346	\$442	\$354	\$424
Own SAT 851-1,000	\$351	\$445	\$367	\$431
Own SAT $>$ 1,000	\$361	\$445	\$385	\$431

Note: For blacks, the SAT score categories "851-1,000" and "over 1,000" are combined because of insufficient observations (Loury and Garman, 1995, p. 303n18).

Source: Loury and Garman, 1995, table 4, p. 304, logs of wages converted to dollars. Confidence intervals and standard errors omitted.

the high school class of 1982, ten years later than the one studied by Loury-Garman, and found that blacks and Latinos did have an admissions advantage at the top twenty percent of all four year schools, but that at non-elite schools the differences between the probability of admissions for whites, blacks, and Hispanics with equivalent credentials "were not statistically distinguishable from zero" (Kane and Dickens, 1996). Thus, the Loury-Darman data on colleges with median SATs of 900 and 1000—both non-elite—cannot tell us very much about affirmative action.⁹

That the minority admission advantage is significant only at elite institutions seriously undermines the Thernstroms' claim that African-American dropout rates are a result of affirmative action, since, as the Thernstroms acknowledge (p. 409), black graduation rates are *higher* at the elite schools. And if one looks at schools where black students have the same median SAT score as the student body as a whole—namely, at historically black

colleges and universities where blacks basically *are* the student body—one finds that the graduation rates are still quite low (with a 6-year graduation rate no better than the black graduation rate at all colleges nationally) (see “Graduation Rates at Black Colleges,” 1994, p. 45; “Black Graduation Rates at Second-Tier Public Universities,” 1994, p. 45; and “Graduation Rates of African-American College Students,” 1994, p. 44). What this means is that there must be explanations other than affirmative action—such as finances—to account for black dropout rates.

The Thernstroms use the Loury-Garman data to determine the costs of affirmative action. The Thernstroms report that “black college dropouts were earning one-quarter less than their counterparts [i.e., blacks with the same SAT scores] who went to less selective schools in the first place but managed to graduate” (p. 411). Again, the Thernstroms assume that academic failure is the only reason people drop out and that affirmative action is the chief cause of academic failure. But even putting aside these assumptions, the Loury-Garman data are too problematic to sustain this conclusion. Loury and Garman’s data are presented in a table that gives the log of the wages, and is thus hard to read. Table 1 gives the Loury-Garman table with all the wage figures converted to dollars.

The table does indicate, as the Thernstroms claim, that black dropouts from more selective schools earn one quarter less than their counterparts who graduated from less selective schools. But the table also shows that black dropouts from the more selective schools who had SAT scores above 700 had *higher* earnings than all white dropouts (from any school) and *higher* earnings than almost all white graduates. But there is more. Blacks who dropped out from the less selective schools and whose own SATs were between 701 and 850 had *higher earnings than every single category of white graduate*. Either there are serious problems with the Loury-Garman table or else the key to success in life is to drop out from a low selectivity college.¹⁰

The Loury-Garman study is the only source cited by the Thernstroms for their claim that affirmative action in higher education hurts blacks. Other studies, uncited by the Thernstroms, have found that attending (not necessarily graduating from) a better college boosts wages, more for blacks than for whites (Kane and Dickens, 1996; Daniel, Black, and Smith, 1995). Thus, the Thernstroms’ argument that one should oppose affirmative action in higher education out of concern for the best interests of African Americans is thoroughly unconvincing.

POLITICS

According to the Thernstroms, African Americans are doing fine in terms of politics. They are adequately represented in the political system and whites are willing to vote for them. The only remaining problem that the Thernstroms can discern is that blacks don’t show the same tolerance as do whites, rarely being willing to vote for someone of another race.

It might seem that African-American political representation leaves much to be desired. Though they make up more than 11 percent of the voting-age population, African Americans are only 2 percent of U.S. Senators, 8.5 percent of the members of the House of Representatives,¹¹ 7.1 percent of state legislators,¹² and 1.5 percent of all elected officials (U.S. Bureau of the Census, 1995, p. 285, table 452). The Thernstroms reasonably point out that the latter figure somewhat understates black representation, given that blacks tend to live in political jurisdictions with fewer elected officials per person (p. 290).

But the Thernstroms raise another consideration that is far less justified. The “real question,” they argue, is not the black proportion of elected officials but the black proportion of political offices “held by Democrats” (p. 289). By focusing on the Democrats, “the party to which almost all blacks belong”—say the Thernstroms—one finds that black representation in the House of Representatives has been “strong” and at the state level “notable” (pp. 289–290). But surely when judging any other country’s treatment of an historically oppressed ethnic group, we would look at the group’s representation overall, not how it was doing within a single party. The same logic ought to apply to the United States.

The Thernstroms can’t quite decide whether the black share of Democratic office holders ought to be compared to the black share of the voting-age population or to the black share of votes for Democrats. For their House of Representatives comparison they use the latter, stating that blacks contributed 14 percent of the votes in the 1992 Democratic presidential primaries and were 14.4 percent of the Democratic congress-people elected in that year (p. 289). It’s not clear why they used the primaries rather than the presidential *election*, where blacks were 15.4 percent of those who voted for the Democratic candidate (“Portrait of the Electorate,” 1992, p. B9)—or perhaps it is clear—but in any event consider the logic of the Thernstroms’ position. If the Democrats were to lose 50 seats in some election, including those of 5 blacks Democrats, we would conclude that the representation of blacks had improved, since their share of Democratic seats would have increased.

For the state legislatures, the Thernstroms compare the black proportion of Democratic legislators with the black share of the voting-age population. The logic of this approach leads to even more bizarre results. Say there is a state where blacks are twenty percent of the population. Assume that out of 100 legislators, 10 are black Democrats, 30 are white Democrats, and 60 are white Republicans. While to the average observer it might appear that blacks were under-represented (one tenth of the legislators but one fifth of the population), the Thernstroms would point to the fact that 25 percent of the Democrats are black, which is higher than their proportion of the population, and we are therefore supposed to conclude that blacks are over-represented. If we look at whites, however, we find that by any calculation they are over-represented as well: they are 80 percent of the voting population but 100% of the Republicans and 90% of the representatives as a whole. In Lake Wobegon, it is said that all the children are above average. In the fantasy world of the Thernstroms, all the voters are over-represented.

Even with this slight of hand, the Thernstroms still can’t get black representation in the state legislatures up to proportionality: nationwide blacks are 10 percent of the Democrats in the lower houses and 8.1 percent in the upper houses (p. 290). But by excluding states without sizable black populations, the Thernstroms are finally able to achieve their spurious proportionality (p. 290)—but it is easy to see why African Americans may not be so reassured.

The Thernstroms are correct that groups of voters might be under-represented for reasons having nothing to do with discrimination. Under a winner-take-all system it will often be the case that minorities—referring not to race or ethnicity but simply to those who are not the political majority—are under-represented. This might not matter much in a society where majorities and minorities were constantly shifting; but where there is considerable polarization the winner-take-all system does a bad job of representing the

minority. When the minority happens to be a group that has been the victim of historic discrimination and that lags behind the majority in most indicators of well-being, then the winner-take-all system seems especially problematic. Some have tried to address this problem by redrawing district lines to create districts where minorities are the majority. This approach is not without problems of its own (weakening minority influence in other districts or reducing minority voter turn-out), and so others, such as Lani Guinier and Cynthia McKinney, have proposed various changes in the winner-take-all system (see, e.g., Guinier, 1994). The Thornstroms mention this in a footnote (p. 651n20), but while categorically rejecting racial redistricting they offer no alternative method for dealing with the problem of black under-representation.

Supporters of racial redistricting note that the newly constructed districts where minorities are the majority are in fact among the *least* segregated in the nation. The Thornstroms respond that though these districts contained significant numbers of whites, “they were politically 100 percent black . . . A 65 percent black district is a sure seat for an African-American candidate; additional black voters are superfluous” (p. 480). But compare the Thornstroms’ solicitousness about wasted white votes with their attitude toward blacks in majority white districts, where they approvingly cite the view that “blacks were electing a candidate of their choice when they cast their ballots for the winner in a white-on-white contest” (p. 490).

Although they wanted to look only at the Democratic party for assessing black representation, the Thornstroms strangely ignore political party preference when discussing why blacks and whites vote as they do. They claim that “only a very small percentage of blacks fail to vote for black candidates” (p. 294), while whites, on the other hand, are increasingly willing to support black candidates. For example, in the 1989 New York City mayoral contest, Rudolph Giuliani “lost an estimated 25 to 30 percent of the white vote to his black opponent, David Dinkins” (p. 295). But rather than showing the tolerance of white voters, this case shows much the opposite. Almost all New York blacks are Democrats and they voted for the Democratic candidate—nothing odd there. But among white *Democrats*, three out of five voted for the Republican candidate in 1989 and two thirds did so in 1993 (“A Portrait of New York City Voters,” 1993, p. B6).

That almost all blacks voted for Dinkins, the Democrat, hardly shows black unwillingness to vote for a white or evidence that “color had clearly been the first consideration among African-American voters,” as the Thornstroms assert (p. 307). To fairly assess whether African Americans are willing to support white candidates over black ones, we need to look at contests that do not pit a white Republican against a black Democrat. And the Thornstroms themselves give examples that undermine their claim that “black candidates can usually count on almost every black vote” (p. 295). Edward Rendell, a white, garnered 20 percent of the black vote against three black opponents in Philadelphia’s 1991 Democratic primary (p. 305). White Zell Miller received 20 percent of the black vote in defeating Andrew Young in the 1990 Democratic primary in Georgia (pp. 307–308). Richard M. Daley, another white, took 26 percent of the black vote in Chicago in 1991, though he was opposed by an African-American former judge running on the “Harold Washington Party” line (p. 305).

The number of blacks in the U.S. House of Representatives grew sharply between 1990 and 1992. This was not because of any dramatic change in white attitudes, but because the redistricting based on the 1990 census resulted in the creation of many new districts in

which blacks or blacks and Latinos were the majority. The Thernstroms are fervently opposed to race-conscious redistricting, but it is remarkable how much of the black presence in the House of which they are so proud is attributable to districts where minorities are a majority. In 1991, there were 25 blacks in the House. Only four did not come from districts where either blacks or blacks and Latinos were a majority of the voting-age population (Swain, 1995, p. 194).¹³ In the 1992 election there were 16 new black members of the House. Three of them replaced black incumbents; all thirteen others were elected from districts created to have a majority of minority voters (Swain, 1995, p. 228). In 1994 of the 38 black members of the House, again only four were from white-majority districts (Swain, 1995, p. 230).

In 1995, the Supreme Court struck down five of the newly-created black-majority districts. In 1996, black incumbents ran for re-election in four of these districts and won. The Thernstroms take this as showing that the era of blacks being unable to get elected in white majority districts is over (pp. 485–486), but the advantage of incumbency is quite powerful and the conditions that led to these African Americans representing white districts (first gain incumbency in a minority district and then run for re-election when the boundaries are changed) are not likely to occur very often in the future. One of the five redistricted representatives, Cleo Fields of Louisiana, chose not to run and the Thernstroms grumble that “[b]lack candidates cannot win elections in which they do not run” (485). True enough, but the decision to run is not just a personal one; the views of funders and political allies matter too, and the newly created district in Louisiana was what David Duke called “tailor made” for his candidacy (Karlan, 1995).

As mentioned above, there are good reasons to think that creating black-majority districts cannot solve the problem of black political representation in the United States. Only some new form of voting system can properly represent minorities. But the status quo has certainly not been providing African Americans adequate political voice.

CRIME

Though they are only an eighth of the population, African Americans make up more than half of the country's prison population (Mumola and Beck, 1997, p. 9). To what extent is this grim situation the result of discrimination?

Blacks are disproportionately imprisoned, say the Thernstroms, because they disproportionately commit crimes. The Thernstroms report that the “most recent review of the literature” by a liberal author concludes that “black incarceration rates are substantially higher than those for whites . . . [because] black crime rates for imprisonable crimes are substantially higher than those for whites” (p. 272, citing Tonry, 1995, p. 79). Tonry does not actually say that higher black crime rates are *the* reason for higher black incarceration rates, but that they are (in words the Thernstroms omit from Tonry's sentence) “the main reason” (Tonry, 1995, p. 79).¹⁴ But in any event, Tonry's analysis has its problems.

Tonry (1995, p. 73) provides a graph of black arrest rates for robbery and aggravated assault from 1980–1991 plotted along with the percent of black offenders for these crimes as reported in victim surveys. The black offending rates are uniformly below the black arrest rates for the respective crimes, and more than 10 percent lower than the arrest rates for assault in every year. But Tonry then minimizes this finding by noting problems with

the data. The data indeed have many problems, but there are no better data supporting the opposite conclusion.

Tonry next reports a 1985 study by Patrick A. Langan (an analyst at the Justice Department's Bureau of Justice Statistics [BJS]) comparing victimization data with state prison admissions for five crimes. For two of the crimes, the imprisonment rates were pretty close to the rate of offending, but black incarceration was considerably higher than the black rate of offending for the other three crimes, and the disparity in the aggravated assault category was statistically significant in each of the three years studied. Nevertheless, Langan concluded that his results "generally support" the hypothesis that black imprisonment corresponds to black offending (quoted in Tonry, 1995, p. 77).

Tonry notes that Langan's analysis (as did other studies) left some 20 percent of the racial disproportion unexplained—that is, unaccounted for by differing patterns of offending. Langan, and apparently Tonry, consider this disparity of no great consequence. But if blacks had been sentenced to prison at the same rate relative to their offending as whites, more than eight percent of imprisoned African Americans would not be incarcerated (calculated from Tonry, 1995, p. 77)—hardly a minor matter.

The Thornstroms report that a major study by the BJS in 1993 confirms the lack of discrimination in the criminal justice system after the point of arrest. Their footnote (p. 602n97) reads as follows:

U.S. Department of Justice, *Felony Defendants in Large Urban Counties, 1990* (Washington, DC: Bureau of Justice Statistics, 1993). For a brief summary, see Patrick A. Langan, "No Racism in the Justice System," *Public Interest* 117 (Fall 1994), pp. 48–51.

According to this study, report the Thornstroms (p. 273), a larger percentage of white felony defendants than blacks were prosecuted after the filing of charges (69 versus 66) and a larger percentage of whites than blacks were convicted (78 versus 73).

The BJS study (Smith, 1993) cited by the Thornstroms, however, nowhere reports the findings that the Thornstroms allege. One suspects that the Thornstroms read Langan's 1994 article which makes an ambiguous comment referring readers to the Smith (1993) study for details on the survey from which his data came (Langan, 1994, p. 49), and never bothered to actually check it.

Langan's article—which is not a BJS study—reports that 38 percent of whites who were convicted of felonies in 75 large urban counties in 1990 were sentenced to prison, compared to 51 percent of blacks. But Langan concludes that there is no racism in the criminal justice system because three factors, rather than bias, account for the differential treatment. First, he notes that blacks were over-represented among those convicted of more violent crimes. Second, black defendants were more likely to have prior criminal records. Both of these factors—the severity of the crime and the defendant's criminal record—are legitimately taken into account by judges passing sentence. Langan's third factor is that blacks tend to be concentrated in jurisdictions that have tougher sentences, not just for blacks but for whites as well. But why do African Americans happen to be concentrated in tougher jurisdictions? Did they move there because they were attracted to the no-nonsense approach to law and order? Or, more likely, were the laws made tougher in these jurisdictions *because* there were a lot of blacks there? Is it a coincidence, for

example, that the leading death penalty states are disproportionately states with high black populations and a history of slavery and Jim Crow? Langan (1994, p. 51) states that "[t]hese three differences explain why 51 percent of convicted blacks but only 38 percent of convicted whites were sent to prison." But he gives no indication that he actually carried out a statistical analysis to correct for any of these variables.

Langan's study and many other studies of discrimination in the criminal justice system suffer from a more serious problem. By comparing blacks to whites regardless of whether they are Hispanic, the actual disparities between blacks and non-Hispanic whites are greatly minimized. Most people of Hispanic ancestry categorize themselves as racially white. Latinos make up about 11 percent of the white population, but when it comes to crime, they often make up more than half the offenders or prisoners classified as white. Thus, for crime statistics, comparisons of African Americans with all whites are actually comparing two groups, both of which might include many individuals victimized by discrimination. Perhaps Latinos are not discriminated against compared to whites, but this ought to be a subject for investigation, not assumed away as implicitly occurs when Hispanics are folded into the white category.

When comparing things like conviction rates by race, there is always the problem that factors having nothing to do with race may explain why one defendant is let off and another not. So an adequate analysis of the question of discrimination in the criminal justice system needs to take into account a whole host of factors rarely available to the analyst. And data on race and ethnicity that would allow the separating out of Hispanics are also infrequently available, given that each state has its own approach to categorizing people in terms of race and ethnicity.

In 1993, the Justice Dept. sponsored and published a study that was able to take account of all factors legally relevant to sentencing decisions. The study, "Sentencing in the Federal Courts: Does Race Matter?" is not cited by the Thernstroms despite its obvious relevance to their concerns. It found that blacks received harsher treatment than non-Hispanic whites for a variety of offenses (McDonald and Carlson, 1993a; McDonald and Carlson, 1993b; see also Mustard, 1998).

The most substantial disparity was for drug offenses. The main source of this disparity is that federal legislation specifies that the penalties for crack cocaine—the form of cocaine for which blacks are disproportionately arrested—are to be much more severe than for powdered cocaine. The Thernstroms argue that the different treatment of the two forms of cocaine could not be discriminatory because the Congressional Black Caucus originally backed the law (p. 278). But whatever the intentions of lawmakers in 1986, when all the facts about crack cocaine and the disparate impact of the law might not have been known, equal treatment seems much harder to credit in 1995. In that year, Congress had before it a report from the U.S. Sentencing Commission, prepared at Congress's request, documenting the faulty science on which the law was based and its racially disparate impact. Despite this information, the House of Representatives voted four to one to reject the Sentencing Commission's recommendation to revise the differential treatment, while the Congressional Black Caucus voted almost unanimously (one member didn't vote) to accept the recommendation (Congressional Quarterly, 1996, pp. H208–H209).

The Thernstroms argue that critics who "depict the War on Drugs as 'an unmitigated disaster for young blacks' typically exaggerate its impact on black incarceration rates" (p.

278). The facts, however, are that in 1980, before the War on Drugs, 1.7 out of every hundred adult black males were in federal or state prison; by 1994, it was almost 5 out every 100. If we include jail inmates, the 1994 figure rises to almost 7 out of 100. And if we add in probation and parole, we find that almost one out of six African-American adult males is under the supervision of the criminal justice system (Brown et al., 1996, pp. 5, 9, tables 1.2, 1.9).¹⁵ This seems pretty disastrous.

The black prison population, claim the Thornstroms, “would be smaller, but not much smaller, if the drug laws were different—if, for example, crack and powder cocaine were treated identically” (p. 279). They then report a calculation that they claim shows that if in 1992 “the percentage of black men serving drug sentences had been reduced to the figure for white men, the black proportion of the total would have fallen from 50 percent to 46 percent.” This, they say, is “not a trivial difference, but hardly a monumental one” (p. 279). Their calculation, however, is totally bogus.

Table 2 presents an extract from the table on which they based their calculation. The Thornstroms state that blacks are 50 percent of newly admitted prisoners. Presumably, they got this by dividing the number of black admissions into the “all new court commitments” figure. But if one adds up the admissions by race, one sees that the total is 263,646, eight percent less than the “all new court commitments” figure. The reason is that race data are often missing. So blacks in fact make up 54 percent of new prison admittees whose race is known, not 50 percent.

The Thornstroms are not very clear regarding how they went about their calculation, but apparently they observed that the figure for drug offenses as a percentage of all offenses for blacks was 36.3 percent, compared to 21.6 percent for whites. If blacks had the same 21.6 percent figure as whites, the black total would be reduced by 14.7 percent, and so they reduced the black total figure by 14.7 percent. But this makes no sense. To say that blacks and whites are treated the same for drug offenses doesn’t mean that drug offenses should be the same fraction of each group’s total offenses. Rather, it means that drug offenders of each race should be sent to prison with equal likelihood. The Thornstroms report that blacks make up about three eighths of those arrested for drug offenses (p. 264). If the races were treated equally (leaving aside for the moment whether the arrest rates themselves might reflect unequal treatment) then blacks ought to be three eighths of those sentenced to prison for drug offenses. From Table 2, we see that about 25,000 whites

TABLE 2
New Court Commitments to Prisons in 38 States, by Offense and Race, 1992

Most Serious Offense	All New Court Commitments	Race		
		White	Black	Other
Number of Admissions	286,164	117,936	143,168	2,542
All Offenses	100%	100%	100%	100%
Robbery	9.9%	6.5%	12.6%	10.7%
Assault	7.5%	7.1%	7.7%	12.0%
Burglary	13.3%	16.6%	10.8%	14.8%
Larceny-Theft	8.1%	9.1%	7.9%	6.8%
Drug Offenses	30.4%	21.6%	36.3%	15.3%

Source: BJS, 1995, p. 553, table 6.32.

(21.6% of 117,936) were admitted to prison for drug offenses from the reporting states in 1992; therefore, there should have been about 15,000 imprisoned blacks (since 15,000 is $3/8$ of the sum of 25,000 and 15,000), instead of the 52,000 actually recorded. This means that there would have been 37,000 fewer blacks sent to prison out of the 143,000 committed in 1992, a reduction of about twenty-five percent. Since these data are just for reporting states, the actual number of blacks who would not have been imprisoned in 1992 if there were equal treatment in terms of the drug laws is 45,000 (25% of black state prison admissions as given in Brown et al., 1996, p. 15, Table 1.16). This, of course, is just one year's excess black imprisonment; if we assume similar numbers for each year since the drug laws of 1986 and 1988, we are approaching half a million blacks who have been incarcerated unfairly.

Of course, the unfairness of the drug laws means not simply that blacks are sent to prison more often than whites, but that their sentences are longer as well. Thus the total number of blacks in prison (rather than the number of new admissions) will be even more disproportionate.

The fact that blacks constitute three eighths of those arrested for drug offenses may itself reflect discrimination or unfair treatment. We cannot check victimization reports for drug offenses. There are survey data, however, on drug usage rates, and black arrest rates for drug offenses are disproportionate compared to their use rates (Tonry, 1995, pp. 108–112).¹⁶

Nowhere in their discussion of blacks and the criminal justice system do the Thernstroms mention racial profiling or police brutality. Nor do they cite poll data showing that, in 1995, 45 percent of *whites* said that blacks were treated more harshly by the criminal justice system than whites (*Gallup Poll Monthly*, Aug. 1995, p. 29). The Thernstroms conclude their chapter on crime by commenting that "If the African-American crime rate suddenly dropped to the current level of the white crime rate, we would eliminate a major force that is driving blacks and whites apart and is destroying the fabric of black urban life" (p. 285). True enough. And if everyone suddenly lost all the burdens imposed by race, gender, and class, life would be grand. As a guide to public policy, however, the Thernstroms' remark is rather empty.

EMPLOYMENT

The Thernstroms begin their assessment of the extent of employment discrimination by presenting a table giving the black-white earnings ratio broken down by sex and by level of education. The table, say the Thernstroms, shows that "black women are pretty much on a par with white women who have had roughly equivalent schooling." Black women who have not completed high school have lower incomes, but those with more education earn somewhat more than white women with the same education.

The Thernstroms comment that their table does not prove that employers discriminate against white women compared to black women. The small difference, they say, could be the result of white women working slightly shorter hours or being slightly more concentrated in rural areas (pp. 444–445). But the major factor—which the Thernstroms don't mention—is that their table compares the incomes not of black and white full-time, year-round workers, but of all blacks and whites with income, whether they worked full-time or part-time. Among women, whites are more likely than blacks to be part-time

workers when they have a high school diploma or more, precisely the groups for which the Thernstroms' table suggests African-Americans have an income advantage. (Of course, it would make sense to factor in part-time workers if white women were more likely than black women to be working less than they wanted to, but the opposite is the case.¹⁷) Looking only at full-time, year-round workers (U.S. Bureau of the Census, 1997b, Tables 12a and 12b), we find that after controlling for years of schooling the income of black women lags behind that of their white counterparts. The only education category for which black women come close to the incomes of white women is those without high school degrees, where the minimum wage places a lower bound on earnings. In every other category black women earn at least ten percent less than white women (and at least 35 percent less than white men).

For men the wage gap between blacks and whites is larger than for women, even correcting for education. But the Thernstroms argue that years of schooling is an inadequate measure of educational achievement. What we really need to correct for, they assert, is the level of cognitive skill workers bring to the job as demonstrated on standardized tests (pp. 445–446). The Thernstroms report that according to one study that controlled for cognitive skill, black men earned 9 percent *more* than white men and that “other research suggests much the same thing” (p. 446). So the Thernstroms would apparently have us believe that not only is there no discrimination, but that black men are somehow earning more than they deserve. Their evidence, however, is extremely weak.

The 9 percent figure comes from an article by George Farkas and Keven Vicknair (1996), critiquing another study that did not include cognitive skill as a control variable in its regression equation. Farkas and Vicknair calculated the same regression equation applied to a different data set but adding in the average score on the Armed Forces Qualification Test (AFQT), a test of cognitive skill. But as Farkas and Vicknair acknowledged in a footnote (p. 557n1), there are two ways of performing this calculation—one can compute either what black incomes would be using the white slopes or what white incomes would be using black slopes. In other work, Farkas and his colleagues (1997, p. 929) state that “the choice between these two [approaches] is somewhat arbitrary” and therefore they present two estimates, one based on each group's slope, “thus providing a range.” In the study cited by the Thernstroms, Farkas and Vicknair (1996) give the estimate using the black slopes in their text, but provide the other estimate in their footnote (p. 557n1): blacks earn 20 percent less than whites, after correcting for many variables including cognitive skill. Thus, we have a range of estimates—from 9 percent more to 20 percent less—which together do not suggest an absence of discrimination. If one were to average the two, for example, black males would earn over 5 percent less than white males.¹⁸

In a footnote (p. 646n85), the Thernstroms back off somewhat from their claim that “other research” supports their contention that black males are overpaid once cognitive skill is factored in, noting that “not all” of the recent studies they cite show an earnings advantage for black males. In fact, however, *none* of them do. One by Ronald Ferguson (1995) shows that after controlling for skill and other variables, black males still earn 5% less, and Ferguson acknowledges the “continuing importance of discrimination” (p. 67). The Thernstroms assert that a study by Neal and Johnson (1996) found only a 27¢ an hour pay difference between black and white males, but the Thernstroms misread the study. The dependent variable in these sorts of regression equations is usually the log of the

individual's wage (because it is expected that the relationship will be curvilinear rather than linear). Neal and Johnson too used the log wage, but in passing mention that if one uses the unlogged wage the gap is 27¢; this by no means represents their main finding, which was that after correcting for cognitive skill a wage gap of about 7% remained; they do not give a dollar equivalent for this, but (if we use wage figures from Farkas et al., 1997, p. 920) it's probably on the order of 80¢ an hour—which is \$1,600 a year. Their results, they note (p. 892), “provide some evidence of current labor market discrimination.”

The study providing the strongest support for the Thernstroms—but which still does not show an earning advantage for black males—is by O'Neill (1990). The Thernstroms report that when O'Neill controlled for region, years of schooling, and test scores, the black-white earnings differential shrank by three quarters to 4.5 percent, and that when years of work experience were controlled, the gap virtually disappeared. O'Neill does indeed say this, but as she specifies in her text (p. 40) and is clear from her Table 5 (p. 41), she is referring to one of the two ways she presents her data: that is, using the white slopes; in the very same table she also presents the data using the black slopes and then blacks make 90 percent of white earnings (controlling for test score, region, schooling, and potential work experience) and 95 percent of white earnings (controlling for these same variables plus actual work experience). No combination of variables used by O'Neill resulted in a black advantage when averaging the black-slope and white-slope equations.¹⁹

Thus, none of the recent studies cited by the Thernstroms show an earnings advantage for black males. The Thernstroms also cite one twenty-year-old study by Hanushek (1978), based on 1970 census data, which they claim found that “black males were actually better paid than white males with comparable education” (p. 646n85). In fact, however, Hanushek found no such thing. What he did find was that in some census regions and at some education levels, the percentage increase in income per year of quality-adjusted schooling was greater for blacks than for whites. The Thernstroms seem to think that higher returns to education means the same thing as blacks being paid more after correcting for education. It does not, however, as the following example from Hanushek's data shows. Among those with no more than a high school education in the West North Central region, white males earned \$94 and blacks \$65 (Hanushek, 1978, table 7). In this region blacks overall had 4.5 years less schooling than whites, adjusting for the quality of the schooling (Hanushek, 1978, table 8).²⁰ Each year of quality-adjusted schooling increased the earnings of blacks by 5.1 percent (which was higher than the white return of 5.0 percent; Hanushek, 1978, table 13). So if blacks had as much quality-adjusted education as whites, their incomes would rise \$15 ($\$65 \times .051 \times 4.5$), leaving them still 15 percent below white incomes. Similar calculations using other regions show a black earning deficit ranging from 8 percent in the Mid-Atlantic to 40 percent in the Northeast.

Although none of the econometric studies showed an earnings advantage for black males, the gap in many of them was quite small. But these studies are likely to understate the extent of discrimination for four reasons. First, the control variables used in the regression models (especially work experience) are often themselves affected by current labor market discrimination. Second, the studies tend to look at a relatively young subset of all workers, for whom labor market discrimination has been correspondingly brief and thus the cumulative effects of discrimination have not yet shown up. Third, the studies usually don't take into account that labor market discrimination manifests itself both in

lower wages and higher unemployment. Real data suggest that 10–20 percent of black wage gains from the mid-1960s to the mid-1970s was accounted for by decreased black labor force participation rates (cited in Ferguson, 1995, p. 69n6). Neal and Johnson do a rough correction for this by assuming that the unemployed have wages of zero and then recomputing their regression equations, controlling for cognitive skill. Their black-white wage gap then increases from 7 percent to 13 percent (Neal and Johnson, 1994, p. 883).

The fourth reason that the econometric studies understate discrimination relates to the validity of the instrument used to measure cognitive skill. There is considerable debate about whether the AFQT is racially biased (for critiques, see Rodgers and Spriggs, 1996; Cawley et al., 1996; Mason, 1998). The Thernstroms ridicule those who claim the test is biased by saying “[e]mployers are apparently guilty of class and racial bias if they want employees to be able to read a training manual . . .” (p. 645n84). But this has nothing to do with the concept of test bias. A racially-biased test is one that predicts wages or job performance differently for people of different races. For example, on the AFQT, the higher that African-American women score on the verbal component the higher their wages, but there is no such wage impact for white women; conversely, math scores are correlated with higher wages for white women, but not for black (Rodgers and Spriggs, 1996, p. 23). Racial differences on AFQT scores tend to be larger than on direct measures of actual job performance (Wigdor and Green, 1991, vol. 1, p. 179). Some scholars have tried adjusting the AFQT to eliminate its racial bias and then running the wage regressions; they find that the resulting black-white wage gap is substantial (Maume et al., 1996; Rodgers and Spriggs, 1996). Whether these adjustments are appropriate remains a complex issue, but in any event if group differences on the test are wider than on job performance, and if non-discriminatory wages ought to correspond to job performance, then using the test scores in the regression equations instead of some more direct measure of job performance will understate the extent of discrimination.

Using the AFQT to correct for job ability involves a related problem. To see this, consider a hypothetical example. Say that in country X females were dominant and employers routinely discriminated against males. A discriminatory employer might look for some measure on which men scored worse and then use this as the criterion for selecting people for high-paying jobs. Since women typically score higher on standardized writing tests, by using such a test employers could assure that women would receive higher earnings. Here is clear discriminatory intent, but yet the discrimination would be difficult to detect. Many would assert that surely the ability to write is a valuable skill and there would be authors who would caustically comment that employers are apparently guilty of gender bias if they want employees to be able to write a coherent memorandum. More importantly, econometric studies that put the writing test score measure into their regression equations would conclude that there was no discrimination (the test score accounted for all the earnings gap), even though there clearly was discrimination.

This example might be objected to on the grounds that while writing is one useful skill it is not the only one; in the real world one’s writing test score does not predict earnings very well. But the same is true of the AFQT. In 1993 the AFQT explained only about 10 percent of the differences in young people’s earnings (Dickens, Kane, and Schultz, 1995). In the famous Griggs case (1971), the Duke Power Company had been openly discriminating against blacks; when such discrimination was made illegal, the company introduced for the first time test and educational job requirements. The Supreme Court ruled,

sensibly enough, that a job requirement is permissible if it is related to job performance, but not otherwise. Without such a ruling, an employer who wanted to discriminate need only find some measure on which a disfavored group performed poorly. To keep out men, use a writing test. To keep out women, use a test of quantitative skill. For some jobs, the AFQT may be a good predictor of performance, but for many it will not be, and so to use it in earnings equations may hide ongoing discrimination.

After discussing the econometric studies, the Thernstroms turn their attention to another method of documenting discrimination: employment audit studies. These studies involve selecting and training pairs of matched testers, whose characteristics and job qualifications are made as close to equivalent as possible except that the members of each pair differ on some characteristic such as race, ethnicity, gender, or age. The testers are then sent out to apply for jobs and to the extent that they fare differently, the difference can be attributed to discrimination.

Large scale employment audits have been conducted by the Urban Institute (UI), by Franklin James and Steven W. DelCastillo of the University of Colorado, and by the Fair Employment Council in Washington, DC. The Thernstroms present a table (Table 2, p. 448) summarizing two UI audits (in the Washington, DC, and Chicago metropolitan areas) and the James-DelCastillo audit (in the Denver area), where being preferred means being offered a job when the auditor of the other race was not. The Thernstroms indicate that the data in their table come from tables 5.1 and 5.3 of an article by Heckman and Siegelman (1993). However, the data from the Denver audit presented in Heckman and Siegelman's table 5.3 article are incorrect—they do not correspond to Heckman and Siegelman's table 5.10 which is supposed to be a disaggregation of the same data nor to the figures given in the actual report of the Denver study (James and DelCastillo, 1991, Table 1; James and DelCastillo, 1992, p. 42, Table 2). The left half of table 3 presents the data from the Thernstroms' Table 2 (p. 448) corrected to show the actual Denver study figures.

The Thernstroms make much of the fact that 86 percent of the time black and white testers in Chicago were treated equally (p. 448), but most of the cases where there was no difference in getting a job were cases where neither applicant was hired. Rather than considering that these cases show the absence of discrimination, they are more properly regarded as situations where no test for discrimination could be conducted. Presumably the employers in these cases hired someone else (since they did advertise a job opening), but unless we know the race of the individual hired, and how that person compared to the rejected auditors, we have no basis for either confirming or disconfirming any level of discrimination. In the Chicago study, three quarters of all the audits resulted in neither applicant being offered a job. In the UI Washington study, the corresponding figure was 59 percent and in the Denver study it was 71 percent (Heckman and Siegelman, 1993, table 5.1; James and DelCastillo, 1992, p. 42, table 2; p. 46). If we consider only those audits where at least one tester was offered a job, as shown in the right half of Table 3, we see that whites got offered jobs 70–86 percent of the time while blacks received offers 54–62 percent of the time. In the Chicago area, where the Thernstroms think that discrimination can “barely be detected” (p. 448), whites got jobs when blacks didn't more than twice as often as the reverse; altogether whites got a third more jobs in the Chicago area than did blacks.

The Thernstroms concede that the figures for Washington show a “sizable, though not a huge, difference,” but they discount these because the audit only looked at private

TABLE 3
Job Offers to Matched Pairs of Testers in Three Metropolitan Areas, 1990

Metropolitan Area	Org. Conducting Audit	All Audits			Audits With At Least One Job Offer					
		Number of Audits	No Difference ¹	White Only ²	Black Only ³	Number of Audits	Both ⁴	White Only ²	Black Only ³	White Total ⁵
Chicago	UI	197	85.8	9.6	4.5	50	44	38	18	82
Washington	UI	241	75.1	19.1	5.8	100	40	46	14	86
Denver	JD	145	77.9	12.4	9.7	47	32	38	30	70
										Black Total ⁶
										62
										54
										62

Sources: Rows 1 and 2 calculated from Heckman and Siegelman, 1993, table 5.1; row 3 calculated from James and DelCastillo, 1992, p. 42, table 2, and p. 46. UI = Urban Institute; JD = James and DelCastillo, University of Colorado;

¹ Either both were offered job or both were turned down;

² Only white tester was offered job;

³ Only black tester was offered job;

⁴ Both testers were offered job;

⁵ Sum of White Only and Both; and

⁶ Sum of Black Only and Both.

employers in the Washington area, excluding government jobs where, they argue, the prevalence of affirmative action would lead to different findings (p. 448). But the Urban Institute, not unreasonably, chose to do a study of private sector discrimination—results that might be more generalizable than one focused on conditions unique to Washington.

The Denver data show very little difference by race and that difference is statistically insignificant.²¹ What accounts for the different findings between the Denver study and those in Chicago and Washington? In part they may reflect actual differences between the labor market conditions in these metropolitan areas. Perhaps Denver's extremely small black population faces less discrimination because there simply aren't enough of them to be perceived as a threat by the white majority (see discussion in Mincy, 1993, pp. 176–177; James and DelCastillo, 1992, pp. 37–38).

Alternatively, or as well, the different findings in Denver compared to Chicago and Washington may reflect methodological differences in the studies. The studies used different methods for recruiting and hiring their testers (UI hired a tester only if a match could be found for him; in the Denver study, the matching was done after the hiring), provided different levels of supervision (UI monitored its testers on a daily basis, the Denver researchers did so twice a week), and had different remuneration schemes (the UI studies paid testers a flat fee, while the Denver study paid testers bonuses based on how successful either member of each audit pair was in his job search) (see James and DelCastillo, 1992, pp. 38–41, 50n4, n6, n7; James and DelCastillo, 1991, pp. 6, 9–10; Turner et al., 1991, pp. 24–26; Zimmermann, 1993, pp. 408–410; Mincy, 1993, pp. 170–171, 175–176). One can debate the pros and cons of each of these methodological choices, but the crucial matter is how well were the pairs matched? In the UI studies, each pair of testers had equivalent education; in the Denver study, there were nine pairs of auditors testing black-white and Hispanic-Anglo discrimination; in two pairs the minority applicant had some college while the white/Anglo did not, and in one pair the minority had a high school diploma while the white/Anglo had some college (James and DelCastillo, 1992, p. 50n4). In terms of their behavior, the black testers in Denver were more ambitious than their white counterparts, making twice as many follow-up phone calls after submitting a written application (James and DelCastillo, 1992, p. 51n8). James and DelCastillo (1992, p. 51n8; 1991, p. 12 and Table 3) claim that the greater efforts of minority auditors to contact employers did not affect the results of their study, but acknowledge that the behavioral differences “may have contributed to the success of minority job applicants” (1991, p. 12). The differences at least suggest problems in the matching, training, or supervision of the Denver testers.

Although the Denver study reported no significant difference in number of job offers for blacks and whites overall, the researchers found an interesting pattern in terms of which jobs each group obtained:

Employers offering menial jobs appear to have been more likely to offer the job to minority applicants. In the Black-Anglo test for example, three black testers were offered jobs as dishwashers. No Anglo testers were. Similarly, two Anglos were offered jobs in administrative or clerical positions; no black testers were (James and DelCastillo, 1992, p. 47).

Given that all the applicants were claiming to be high school graduates, the fact that minorities were given preference in hiring as dishwashers seems a rather mixed blessing. The Franklin and James report does not provide enough detail to know how many other menial jobs were involved.

Are the differences between blacks and whites in success at getting jobs statistically significant in Chicago and Washington? For Washington, the answer is a clear "yes." For Chicago, matters are more complicated. Heckman and Siegelman (1993) provide an extensive discussion of different statistical tests and the rationale for using them. With the one they ultimately prefer, they conclude that the Chicago audits fall just short of statistical significance ($\chi^2 = 3.1$, with one degree of freedom; the cutoffs for the .05 and .10 significance levels are 3.8 and 2.7, respectively).²²

Heckman and Siegelman (1993, pp. 215–216) and others have pointed out that the audit methodology is susceptible to "experimenter effects": that is, the results can be influenced by the conscious or unconscious desire of the testers to find evidence of discrimination. The Thornstroms consider this the decisive refutation of the audit studies. It is hard, however, to see how a tester could be recruited and trained without figuring out the purpose of the audits. Testers, after all, do not carry out a single audit, but conduct dozens of audits each. And why else would someone be paying you to apply for jobs under a fictitious identity but never to accept them? Experimenter effects are a serious concern, but these can be reduced by careful training and supervision of the testers (see Zimmermann, 1993, for a discussion of supervision in the UI studies).

Even though experimenter effects might cause the audits to overstate the extent of discrimination, there are at least two reasons why the audits are likely to understate discrimination.

First, the audits only tested jobs that were advertised in newspapers (James and DelCastillo, 1992, p. 38; Turner et al., 1991, pp. 20–21). But most entry-level jobs are not filled this way (see Holzer, 1987; Holzer, 1988; Korenman and Turner, 1996), and employers who don't want to hire minorities are far more likely to avoid newspaper ads, relying instead on personal contacts, word of mouth, drop-ins (particularly if located in a suburb), and employment agencies (Neckerman and Kirschenman, 1991).

Second, the UI study had its black testers avoid stereotypically black clothing, mannerisms, and speech (Turner et al., 1991, p. 25). The logic of this decision by UI is clear: they wanted to avert a debate over whether an employer who used any of these characteristics as the basis for a hiring decision was actually discriminating (for jobs requiring customer contact, for example, are black speech or dress a bona fide disqualification?). But even if the use of these factors in hiring decisions is not always discriminatory, it will certainly sometimes be so, and the audits would miss this sort of discrimination.²³

Interestingly, some of the arguments that the Thornstroms give against the validity of the audit studies in fact acknowledge discrimination. Were the auditors really matched for all relevant characteristics, ask the Thornstroms, including cognitive skill? But the auditors were not given any job or skill test by the employers. Maybe the employers could tell anyway that an applicant was low in cognitive skill? But given that the UI auditors had more education than they pretended to have, it would have to be a pretty discerning employer who could detect a relevant cognitive skill gap. More likely, if cognitive skill played any role it was in terms of employer assumptions about the cognitive skills of

people of different races—which is statistical discrimination and illegal (see Turner et al., 1991, pp. 23, 25–26).

The Thernstroms argue that the special character of Washington, DC could have skewed the results because employers would likely be “suspicious of black job applicants who looked attractive, reasoning that ‘if they’re as good as they seem, why aren’t they applying for a better-paid, more secure post in the Department of Transportation?’” (p. 647n90). This is dubious: if anything, employers in the Washington area would be likely to assume that equal opportunity employment laws would be enforced more vigorously there than elsewhere. But consider the logic of the Thernstroms’ point. They imply that it is reasonable for an employer to be suspicious of a qualified black, but not equally suspicious of a qualified white. No employers checked up on the credentials of any of the audit applicants (Turner et al., 1991, p. 23), so apparently the Thernstroms consider it reasonable that when in doubt, don’t hire the African American. This too is discrimination.

The Thernstroms summarize their review of the evidence of employment discrimination by stating that “[w]e are left with the conclusion suggested by the several studies examining racial differences in income controlled for cognitive skill”—namely, that the income differences are “primarily” the result of cognitive skill differences (p. 449). But this is a non sequitur. The issue isn’t whether cognitive skill differences might not account for more of the income gap than other factors. Rather it is whether discrimination remains an important obstacle to equality for African Americans. And even if one doesn’t like the audit studies, there is a wealth of other evidence documenting the continuing significance of discrimination, but the Thernstroms consider none of it. In a book purporting to present a full picture on the status of blacks in the United States, the authors offer no discussion of the studies that interviewed employers and found discrimination to be frequent (Kirschenman and Neckerman, 1991; Holzer, 1996; Turner, 1997); no analysis of cases before the EEOC or the courts (Darity and Mason, 1998); and no discussion of the implications of polling data which they mention in passing in another context (p. 506) that shows that about a third of whites—who of course have incentive to understate the reality—believe that blacks have “less opportunity” than whites to live a middle class life.

RACISM AND AFFIRMATIVE ACTION

Survey data, say the Thernstroms, show a remarkable reduction in white racism. Survey data also show, they claim, citing the work of Sniderman and Piazza (1993), that white opposition to affirmative action is not motivated by racism (pp. 500–501). But then they also tell us that Sniderman and Piazza found that some whites dislike affirmative action so much that it has caused them to dislike blacks as a consequence (p. 312). If disagreement over the appropriate policy for remedying the country’s long history of racism causes some whites to dislike blacks, however, then one suspects that whites were not as free of racism as the Thernstroms suggest.

Other scholars have shown that Sniderman and Piazza are probably wrong in their claim that much opposition to affirmative action is not motivated by racism (Sears et al., 1997), but Amy Gutmann (1996, p. 150) has pointed out one other interesting finding of the Sniderman and Piazza study—a finding naturally ignored by the Thernstroms: opposition to affirmative action is lessened when respondents are given arguments for and against. This ought to give heart to supporters of affirmative action, for it means that there is still

a critical role for making the case to the American people. And refuting the distortions of the Thernstroms is an important part of that case.

NOTES

1. All subsequent references to this book are given simply as page numbers in parentheses.
2. Data from U.S. Bureau of the Census, 1997a, with cutoffs of $>.6$ following Massey and Denton, 1993, pp. 75–77. Massey and Denton (1989) selected the 1980 hypersegregated metropolitan areas from among 60 large metropolitan areas; for comparability, the 1990 hypersegregated areas were selected from the 60 metropolitan areas with the largest black population in 1990. If instead all metropolitan areas are examined 29 are hypersegregated, accounting for 40 percent of all U.S. blacks.
3. The Thernstroms present a table of dissimilarity indexes for 15 metropolitan areas from 1970–1990 (p. 215, Table 5) that they say shows an increase in New York, no change in Detroit, and “modest declines” in Chicago and Cleveland; “[e]verywhere else the level of residential segregation of blacks from whites fell sharply” (p. 214). In fact, the declines shown in their table for Philadelphia and New Orleans were smaller than those in Chicago or Cleveland. The Thernstroms state that the 15 metropolitan areas they included were those “with the largest black populations in 1990” (p. 214; also Table 5, p. 215), but they have confused metropolitan statistical areas and consolidated metropolitan statistical areas. The average 1970–1990 decline in the dissimilarity index for the 15 metropolitan areas that were actually the largest in 1990 is 8 points, rather than the 10 calculated by the Thernstroms.
The Thernstroms cagily note that the black-white dissimilarity index in Boston in 1970 was the same as the Italian-Hungarian index for that year (p. 230). But this statistic comes from an analysis of a mere 533 people of Hungarian stock in a city of 641,056 (U.S. Bureau of the Census, 1973, Table 81; see Kantrowitz, 1979, esp. pp. 44n11 and 50, table 1, note c). Researchers consider the index of dissimilarity unreliable if one of the groups has a population less than 10 times the number of census tracts (see Langberg and Farley, 1985, p. 71), a condition not met in this case.
4. It should be noted that the Thernstroms don’t tell us about the urban ghettos discussed by the Kerner Commission, but about the larger metropolitan areas. So, for example, in *metropolitan* Philadelphia from 1980–1990, the percentage of the black population living in ghettos was basically unchanged, but within the *city proper*, the percentage increased from 67 to 72 (Borowski and Dubin, 1991, p. B1; see also Gillmor and Doig, 1992, p. 51).
5. The Thernstroms’ table (p. 216) also lists two metropolises, Los Angeles and Dallas, with data for 1990 but not for 1980. These figures do not appear in their source: Gillmor and Doig, 1992; Los Angeles figures for both years do appear in a newspaper article by Doig (1991), but in any event by including these least ghettoized areas in 1990 but not 1980, the average improvement looks better than it was. Correcting the reversed signs and omitting Los Angeles and Dallas for both years yields a 1980–1990 decline in ghettoization from 51 to 47, instead of from 52 to 44.
6. The Thernstroms do give historical data on the proportion of whites who say it would not make any difference to them if “a Negro with the same income and education as you moved into your block”—in fact, they give these data twice, with different numbers each time (p. 141, Table 2; p. 221, Table 9). That 85 percent answer that it would not make any difference to them (in 1972), while 3 out of 8 still believe that housing discrimination should be permissible, shows the limitations of this question for discerning white attitudes.
7. The locational cost adjustments in this study are probably understated. Compare the cost figures in NCES, 1995a, pp. D11–D14, with those in Chambers, 1996, Table 1, and NCES, 1998a, pp. 19–20.
8. The relationship between cost-adjusted expenditures and minority enrollment is not mono-

tonic decreasing, and the relationship between actual dollars of expenditures and minority enrollments is not monotonic increasing. NCES, 1995a, p. A-5.

9. Note that the variable in the Loury-Garman analysis called "grade point average" (Table 1, p. 296) is the individual's *college*, not *high school*, grade point average (p. 295) and therefore their study cannot, as Kane's could, judge the extent of admissions preference shown blacks.

10. Further problems with the table can be seen by examining the figures for "earnings for all workers." These ought to equal the earnings of graduates times the fraction of those who graduate plus the earnings of dropouts times the fraction of those who do not graduate. However, they are substantially different in every case.

I wrote to Profs. Loury and Garman on Feb. 8, 1998 requesting clarification of their table. On Feb. 26, Prof. Garman wrote back that Prof. Loury would respond to my questions. I followed up on Mar. 22. On Apr. 1, 1998, Prof. Loury apologized for the delay and said she would get to it in the next week. I followed up again on May 26, but have not yet gotten a reply.

11. The Thernstroms claim there were 39 black representatives in the House in 1996 (p. 288), but they include two non-voting delegates, as they do for at least some of the years shown in their Table 1, p. 289. Incidentally, as part of their consideration of how African Americans are doing politically, the Thernstroms might have included some discussion of majority-black Washington, DC, the only place in the country subject to taxation without representation, a matter that at one time aroused some concern.

12. Calculated by dividing the number of black state legislators into the total number of legislators given in U.S. Bureau of the Census, 1995, p. 284, Table 450, adding in vacancies and seats held by third party candidates. Note that the figure for black state legislators in 1993 given in the Thernstroms' Table 1 (p. 289) is incorrect; U.S. Bureau of the Census, 1996a, p. 284, cited by them, lists 561 as the total for state *and* U.S. legislators, not for state legislators alone.

13. One was William Clay, whose district had been historically black through many years of Clay's incumbency, dipping to slightly below half black in terms of voting-age population, but still more than half black in terms of total population (see Swain, 1995, pp. 117, 194, 202-203; Barone and Ujifusa, 1997).

14. As the Thernstroms acknowledge (p. 602n95), Tonry makes an exception for drug offenses.

15. Figures "per 100,000 residents" were converted to "per 100,000 adult residents" using the age by race data in U.S. Bureau of the Census, 1995, p. 21, table 21. Figures for black males estimated by taking the overall male-female ratio and applying it to blacks.

16. There are no data on arrests for drug possession as opposed to for all drug offenses, but sentencing for drug possession shows more of a black disproportion than does sentencing for drug trafficking. See BJS, 1995, p. 553, table 6.32.

17. Data on unemployment and underemployment (people working less than full-time but wanting more work) among educated women show that whites have lower rates than blacks. See the data available at the web site of the Economic Policy Institute, <http://epinet.org/>.

18. Farkas et al., 1997, using their own regression equation, find that after correcting for cognitive skill and many other variables black males make between 78% and 103% of what white males do. Averaging 78% and 103% gives a black pay gap of 9%.

19. In addition to the other problems with O'Neill's study discussed below, Neal and Johnson (1994, p. 877) note that she apparently included in her analysis a supplementary sample of economically disadvantaged whites, which would have the effect of reducing the black-white wage gap and thus understating labor market discrimination.

20. This figure is for all blacks, not just those with no more than a high school education; presumably the gap for the latter would be smaller, but Hanushek doesn't give this figure. Thus, this calculation overstates the narrowing of the wage gap.

21. The Thernstroms coyly remark that the Denver study is much less well known than the UI

studies (p. 449), but their citations for the Denver study are to an unpublished report which it seems (given their exclusive reliance on the data in Heckman and Siegelman) they never saw (p. 647n89).

22. Heckman and Siegelman refer readers to their unpublished companion paper for further details; however, it turns out that the companion paper has not yet been written. Communication from Peter Siegelman, 31 Jan. 1998.

23. It is unclear how the Denver researchers handled this. On the one hand, they mention as an apparent difference that UI excluded "recognizably black mannerisms, speech, and dress"; on the other hand, in Denver they made careful efforts to ensure that the tester pairs had "equivalent . . . dress and appearance" (James and DelCastillo, 1992, p. 40). If the black Denver testers did have recognizably black speech, then there is a different reason that the study might understate discrimination. An audit only counted if both team members got to contact employers in person. Sometimes an advertisement told applicants to call first (James and DelCastillo, 1991, pp. 8, 11). If an employer suspected that a black was calling and for that reason did not invite the applicant to apply in person, this would not be counted as an audit and thus the discrimination would go undetected.

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