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Abstract

Adult economic status is positively correlated with parental economic status in every society for which we have data, but no democratic society is entirely comfortable with this fact. As a result, all democratic societies have adopted policies aimed at reducing the effect of family background on life chances, and most left-of-center political parties think that governments should do even more. This paper makes two main arguments. First, equal opportunity does not imply eliminating *all* sources of economic resemblance between parents and children. Specifically, equal opportunity does not require that society eliminate the effects of all inherited differences in ability. Nor does it require that society prevent parents from transmitting different values to their children regarding the importance of economic success relative to other goals. Second, the size of the correlation between the economic status of parents and their children is not a good indicator of how close a society has come to equalizing opportunity. Measuring equality of opportunity requires data on *why* successful parents tend to have successful children. In particular, it requires data on the degree to which a society has minimized obstacles to economic success that we know how to alter, such as parental neglect and ineptitude, inequitable distribution of effective teachers, and labor market practices that favor the well-born.

Would Equal Opportunity Mean More Mobility?

By Christopher Jencks and Laura Tach¹

Adults' economic status is positively correlated with their parents' economic status in every society for which we have data, but no democratic society is entirely comfortable with this fact. As a result, all democratic societies have adopted policies aimed at reducing the effect of family background on life chances, and almost all parties of the left think governments should do even more. A poll of social scientists who study intergenerational mobility would probably find that they, too, want governments to be more aggressive about removing obstacles to economic mobility. Those who study the effects of family background on life chances are, after all, often drawn to the subject because they feel that the inheritance of economic disadvantages is unjust.

This chapter will make two main arguments. First, equal opportunity does not imply eliminating *all* sources of economic resemblance between parents and children. Specifically, equal opportunity does not require that society eliminate the effects of either inherited differences in ability, broadly defined, or inherited values regarding the importance of economic success relative to other goals. Our second main point, which follows directly from the first, is that the intergenerational correlation of economic status (which we denote as r_I) is not a good indicator of how close a society has come to equalizing opportunity. Measuring equality of opportunity requires data on *why* successful parents tend to have successful children. In particular, we must separate the contributions of genes and values from the contributions of child-rearing arrangements, educational institutions, and labor market practices that favor the well-born.

In the interest of brevity we will denote the intergenerational correlation of adult earnings as r_{Iy} and the intergenerational correlation of occupational rank as r_{Isei} . These two measures are not good substitutes for one another. Reported values of r_{Iy} are higher in the United States than

in most other rich countries, while reported values of r_{Isei} are lower in the United States than in most other rich countries (Corak, 2004; Björklund and Jäntti, 2000). In addition, we often cite data from papers by economists, who report the elasticity of children's earnings with respect to their parents' earnings rather than the correlation. We denote this elasticity as β_{Iy} . When earnings inequality is the same for fathers and sons, r_{Iy} will equal β_{Iy} , but that is not always the case.²

Section 1 presents evidence that most citizens of rich western democracies reject absolute economic equality and think that intelligence and skill should be rewarded, regardless of their source. But the citizens of rich democracies also think that economic inequality is fair only if there is equal opportunity. At least in America, we argue, legislators have resolved these potentially conflicting ideals by assuming that equal opportunity requires both meritocratic labor markets and an opportunity for everyone to acquire whatever traits the labor market rewards.

Section 2 briefly reviews what we know about how individual traits that affect economic success contribute to r_I . Section 3 discusses evidence about the role of genes, while Section 4 discusses the role of values. These sections tentatively conclude that the combined effect of inherited genes and values explains something like half of r_{Iy} .

Section 5 argues that meritocracy should be understood as a system in which “productivity enhancing traits” are the only source of earnings inequality, and that earnings inequality would be substantially lower in such a society than in most existing societies. Since r_I depends on the ratio of inherited inequality to total inequality, and since equal opportunity would reduce both, its effect on r_I is unpredictable. It follows that changes in r_I are an unreliable indicator of whether opportunity has become more or less equal. Section 6 concludes by suggesting that the best way to measure changes in equal opportunity is to track the effects of

specific sources of intergenerational economic resemblance that offend our sense of justice, such as the effect of parental income on college enrollment rates or children’s chances of seeing a doctor when they are sick.

1. When is the inheritance of privilege unjust?

Table 1 shows how adults in Japan, West Germany, Great Britain, and the United States responded to five questions about the acceptability of economic inequality in 1991. The data were collected by the International Social Justice Project (ISJP). The table shows the percentage of adults who agreed with each statement minus the percentage who disagreed. The negative numbers in the top two rows indicate that most respondents disagreed with these statements. The positive numbers in the bottom three rows indicate that almost all respondents agreed with these statements. Three points deserve attention:

- Most adults in rich democracies reject the proposition that distributing income equally is the fairest option. This is especially true in the United States.
- Most adults in rich democracies feel that workers deserve to earn more if they are “more intelligent or more skillful than others.” This is especially true in the United States and Britain.³
- Almost all adults in West Germany, Britain, and the United States agree that economic inequality is fair, “but only if there are equal opportunities.”

The ISJP also surveyed a number of East European nations in 1991, obtaining results remarkably similar to those in Table 1.

Table 1 about here

Table 1 does not tell us what the citizens of rich democracies mean by “equal opportunities.” In America, however, the legislative record from 1960 to 2000 suggests that lawmakers thought equal opportunity had two components. First, employers should not reward workers for characteristics that do not affect their job performance. This ideal, which we call “meritocracy,” led to legislation forbidding discrimination in hiring, promotions, and pay on the basis of race, ethnicity, gender, and many disabilities.⁴ The second component of equal opportunity was the idea that every child should have an equal chance to develop the traits that employers value. This ideal is usually called “equal educational opportunity.”

Although equal educational opportunity is a widely accepted ideal, there is no consensus about what it means.⁵ Sometimes the term refers only to the opportunities that schools provide, but sometimes it subsumes the full range of opportunities available to children, including those provided by families and communities. This broader definition implies that society must either make families and communities more alike or find ways to offset the adverse effect of growing up in the wrong family or community. To avoid confusion we refer to this broader ideal as equal developmental opportunity rather than equal educational opportunity.

Discussions of equal educational opportunity also tend to be vague about whose opportunities must be equalized. The term almost always implies that children from different racial and ethnic groups should have the same chance of acquiring valuable habits and skills, and most scholars assume that this also applies to children from different socioeconomic backgrounds. But while race and class are *not* acceptable sources of inequality, it is less clear what sources of inequality *are* acceptable in a world of equal opportunity. If equal educational opportunity means “no child left behind,” it requires equal outcomes. But Table 1 shows that hardly anyone favors that goal.

A more common interpretation of equal educational opportunity is that all children should have the same opportunities to develop their innate abilities. This chapter will adopt that definition, but with an important caveat. When people talk about differences in “innate ability” they usually have in mind genetic differences that have roughly the same additive effect in good and bad environments. If every child attends a better school, mean outcomes will rise but neither the dispersion of outcomes nor their correlation with children’s genetic endowment will change much. But genes do not always work this way. If some children have an inherited tendency to myopia and some do not, giving all children eye glasses will only help those who are myopic. Mean visual acuity will rise, and the dispersion will fall. Likewise, if a society stops marginalizing individuals on the basis of their physical appearance, mean earnings should rise and inequality should fall.

The idea that society should worry more about environmental than genetic differences also reflects the mistaken assumption that it is easier to overcome environmental than genetic disadvantages. In reality, however, environmental damage is often just as irreversible as genetic damage, and when that is the case the victims need compassionate help, not “equal opportunity.” Indeed, we may eventually find it easier to treat many genetic disadvantages than to reverse the effects of childhood diseases or parental ineptitude. For now, however, we think it important to ask how much of r_I derives from genetic resemblance between parents and children, even though we recognize that the normative implications of such estimates are far from clear.

Another crucial question about equal opportunity is whether meritocracy requires that all individuals receive “equal pay for equal work” or whether this rule applies only to members of under-represented minorities like blacks and women. In this chapter we assume that meritocracy

requires equal pay for all equally competent workers, even if they are not members of any identifiable minority. To see why we take this position, two examples are helpful.

In 1953 the wages of unionized Boston-area truck drivers varied by a factor of two, depending on what was in the driver's truck (Dunlop, 1957). Delivering magazines was worth more than delivering beer, which in turn was worth more than delivering coal. Laundry and scrap metal were the least rewarding cargoes. Dunlop attributed these differences partly to variation in the wages of other workers in a driver's industry and partly to historical accidents. Although these wage differentials were not related in obvious way to skill requirements, their effects were probably not random. Boston unions that controlled access to jobs usually favored applicants with relatives in the union. As a result, high-wage drivers were probably in a position to help their sons get similar jobs, raising r_I . Unions of this kind have declined since the 1950s, but employers still rely heavily on their current employees to fill vacancies. As a result, seemingly random variation in the wages paid to workers with the same skills will in practice benefit children with well-connected parents more than children without such connections.

Our second example involves lotteries. Lotteries make incomes more unequal, but in a random way. In recent decades Americans have invested a growing fraction of their income in lotteries. Suppose that by 2050 households were spending a quarter of their income on lottery tickets. This would make post-lottery income far more unequal than pre-lottery income without increasing the covariance between parental income and children's adult income, so r_I would fall. Some would argue that opportunity was also more equal, precisely because r_I had fallen and "anyone now has a chance to get rich." Others would argue that equal opportunity requires rewards based on merit, not luck. This chapter makes the second assumption, so we see lotteries

as simultaneously lowering r_I and making opportunity less equal by making it less dependent on merit.

Despite these ambiguities, we think r_I would remain positive if economic success depended solely on job performance and all children had the same opportunity to develop their talents. This would be true partly because the citizens of an equal opportunity society would still get half their genes from their mother and half their genes from their father. As we shall see, genes currently influence workers' cognitive skills, personality traits, educational attainment, and labor market success. And as the second question in Table 1 indicates, most citizens of rich democracies seem to think that intelligence and skills should be rewarded even if they are due to "luck."⁶ These beliefs would probably change if we knew how to neutralize the effects of disadvantageous genes and failed to do so, but that is not yet a common problem.

A second reason for believing that r_I would remain positive under equal opportunity is that even the weakest versions of democratic pluralism allow parents to pass along their distinctive values to their children (or at least to try). If parents value spending time with their families more than they value consumer goods or a big bank balance, their children will probably tend to have the same priorities. The same will be true of parents who value saving souls or artistic expression more than they value economic status. Such patterns would keep r_I positive even if genetic variation became unimportant.⁷

2. Proximate causes of r_I

Many influential sociologists and economists have argued that parents pass along their advantages to their children primarily by helping their children acquire more education (see, for example, Becker, 1964 and 1991, Blau and Duncan, 1967). Some have argued that

noncognitive traits play an equally important role in determining earnings and that such traits may also help explain r_I (Bowles and Gintis, 1976; Mueser, 1979; Heckman, 2000). Recent evidence suggests that disparities in physical and mental health also contribute to r_I .

Education and cognitive skills. Surprisingly few surveys have good measures of both respondents' cognitive skills and their parents' income. The best source of such data is probably the Wisconsin Longitudinal Survey (WLS), which has been following a representative sample of that state's 1957 high school graduates. The WLS extracted data on parental income from Wisconsin's tax records for 1957 through 1960 and obtained children's eleventh grade scores on the Henmon-Nelson group IQ test from school records. Because the WLS was limited to high school graduates, it underestimates the role of low educational attainment in explaining adults' economic problems. Because Wisconsin was relatively prosperous and overwhelmingly white in 1957, the WLS also underrepresents respondents from extremely disadvantaged backgrounds. Our aim, however, is not to estimate r_I for the United States but to estimate the degree to which r_I can be explained by children's test scores and educational attainment.

Table 2 about here.

Table 2 shows three measures of the association between parents' economic status in 1957 and their children's economic status in 1991:

1. The coefficient of father's occupational status (SEI) in 1957 when predicting employed children's occupational status in 1992.⁸
2. The coefficient of logged parental income for 1957-60 when predicting employed children's logged earnings in 1991.
3. The coefficient of logged parental income for 1957-60 in an equation predicting children's logged family income in 1991.

Column 1 shows the bivariate coefficients of father's occupation and parental income with no controls. Column 2 shows the coefficients after controlling causally prior parental characteristics (father's education in the case of father's occupation; both father's education and father's occupation in the case of parental income). Column 3 adds measures of the mother's education, family size, and whether there were two parents in the home, all of which could be affected by the father's economic position prior to marrying. Column 4 adds the child's eleventh grade test score, while column 5 adds years of school completed and six dichotomous measures of whether the respondent held specific degrees. Column 6 adds a measure of occupational aspirations at the end of high school, which is a determinant of educational attainment but also has a significant effect on all outcomes except a daughter's earnings even with educational attainment controlled. Column 7 adds five measures of personality traits in 1992.

Once we take account of a daughter's test scores and education, her parents' income no longer has an economically or statistically significant effect on her own earnings or family income. Her father's occupational status does have a statistically significant effect, but since the coefficient is only 0.034, it is not economically important. Among daughters, therefore, the economic benefits of having a successful father are almost all attributable to the fact that daughters of such fathers have above-average cognitive skills and educational attainment.

The human capital story does not work as well for sons. About half the bivariate association between parental income and sons' earnings or family income remains unexplained after we control IQ and education (compare equations 1 and 5 in rows 5 and 6).⁹ These controls also leave a third of the bivariate association between fathers' and sons' occupational standing unexplained.¹⁰

Noncognitive habits and skills. Bowles and Gintis (1976), Heckman (2000), and Osborne (2005) all argue that noncognitive traits explain part of r_I . While this argument seems plausible, empirical tests have not yielded impressive results.¹¹ Osborne, for example, finds that adolescent males who say that hard work counts for more than luck have above-average earnings in adulthood. But after she controls men's test scores and educational attainment, locus of control explains only four percent of r_I .

The 1992 WLS followup also included measures of what psychologists call the "Big Five" (extraversion, agreeableness, conscientiousness, neuroticism, and openness). When we included these measures in equation 7 of Table 2, more than half the thirty coefficients were significant. But these measures were so weakly correlated with father's occupation and parental income that they explained almost none of β_{Isei} or β_{Iy} .

Michael Olneck's followup of students who attended school in Kalamazoo, Michigan yielded similar results. Tenth grade home room teachers rated Kalamazoo students on cooperativeness, dependability, executive ability, emotional control, industriousness, initiative, integrity, perseverance, and appearance. The correlations between these nine ratings and men's economic status when they were between the ages of 35 and 59 averaged 0.24 for occupational SEI and 0.18 for annual earnings (Jencks et al, 1979. Tables A5.3 and A5.4). But once again Kalamazoo teachers' ratings were so weakly correlated with family background that they explained a negligible fraction of r_I (Corcoran and Jencks, 1979:75).

Duncan, Kalil, Mayer, Tepper, and Payne (2005) used measures of math skills, shyness, self-esteem, depression, and "mastery" during childhood or adolescence to predict a woman's mean family income when she was between the ages of 30 and 34. The four income correlations with the four noncognitive measures averaged 0.12. The income correlation with adolescent

math scores was 0.25. The authors could not calculate the noncognitive traits' contribution to r_I , but traits that correlate only 0.12 with adult income cannot contribute much to r_I .

Dunifon, Duncan, and Brooks-Gunn (2001) showed that annual interviewer ratings of how clean respondents' houses were in 1968-72 predicted both respondents' hourly wages and their children's hourly wages in 1994-96. After controlling other aspects of family background, a one standard deviation increase in cleanliness was associated with a 10 to 15 percent increase in both parents' and children's future earnings.

Anti-social behavior may play a larger role in explaining r_I . Incarceration is associated with a significant reduction in future earnings, even after controlling test scores and education (Western, 2004). Incarceration is also correlated with family background, although we could not find estimates of its contribution to r_I .

Health. Parental income is positively correlated with children's health (Case, Lubotsky, and Paxson, 2002). Children are healthier if they have a regular bedtime, if they wear seatbelts, if no one in their household smokes, and if their mother is not overweight, but these indicators explain only a tenth of the correlation between parents' income and their assessment of their child's health. The fact that low-income parents tend to have unhealthy children appears to play some role in explaining r_I (Case, Fertig, and Paxson, 2004; Haas, 2004).

3. Genes and r_I .

In 1971 Richard Herrnstein published a controversial article in *The Atlantic* arguing that genes played a substantial role in explaining the transmission of economic advantages from one generation to the next. Herrnstein proposed a syllogism that we can rephrase thus:

- If genes affect IQ and if IQ affects economic success, then

- Economically successful parents will have more than their share of the genes that contribute to high IQ scores.
- Since children inherit half their genes from each parent, children of economically successful parents will also have more than their share of the genes that contribute to high IQ scores. Therefore
- Children of economically successful parents will have higher IQ scores and be more economically successful than children of less successful parents.

Although this argument is logically compelling, IQ explains only a modest fraction of the economic resemblance between parents and children. As a result, genetic influences on IQ do not explain much of the economic resemblance between parents and children (Jencks et al, 1972). But while Herrnstein exaggerated the importance of IQ, he may not have exaggerated the role of genes in economic success. We now know that genes affect a multitude of traits that can influence economic success, including not just test scores but physical and mental health, educational attainment, beauty, height, and weight. Herrnstein's argument can therefore be restated in more general form:

- If genetic variation affects any of the traits that labor markets reward, then
- Genetic variation will affect economic success.
- If the labor market still rewards the same traits a generation later and genes still affect these traits, then
- Biological children of a successful parent will still tend to have traits that the labor market rewards, even if the children have no social contact with this parent.

This section argues that genetic similarities between parents and children account for something like two-fifths of r_{ly} . But before turning to the evidence, we need to clarify what we mean when we say that genes “explain” something.

Genes can affect human skills and behavior in at least three distinct ways. First, genetic differences lead to physiological differences among individuals who grow up in the same environment, and these differences can affect both skills and behavior. Second, genetic differences affect the way members of a society treat one another. Third, genetic differences affect the environments that people choose or create for themselves. Consider math skills. Some children master math more easily than others, and some of this variation is traceable to genetic differences. The educational system accentuates these differences by assigning mathematically gifted students to more demanding classes and by encouraging them to attend college. At the same time, students who find math difficult preserve their self-respect by dismissing math as unimportant and dropping it at the first possible opportunity. Genes may also influence math skills indirectly. Plomin et al. (1990) report that preschool children’s genetic endowment influences the amount of time they spend watching television. Once children start school, those who watch more television presumably have less time for math.

Because our genes affect both the way others treat us and the choices we make for ourselves, we cannot estimate genes’ impact “holding the environment constant.” We can control measurable features of the environment, but we can never rule out the possibility that genes also influence unmeasured features of the individual’s environment. As a result, estimates of genes’ explanatory power seldom distinguish their physiological effects from their social effects. Thus while we may be able to say that genes currently explain, say, 20 percent of the variation in earnings, we cannot infer that environment explains only 80 percent. Since genes

influence people's environment, environmental variation could explain 100 percent the variation in earnings even though genes also explained 20 percent.

Genes and earnings in Sweden. The most direct way to estimate genes' role in economic inheritance is to compare the value of r_I when parents raise their own biological children (r_{I-b}) to the value when parents raise children to whom they are not biologically related (r_{I-nb}).

Björklund, Lindahl, and Plug (2004) have assembled such data for nearly 6,000 Swedish children adopted during the 1960s. Among children who grew up with their biological fathers the intergenerational earnings elasticity (β_{Iy-b}) was 0.232.¹² Among children adopted before their first birthday, the elasticity of children's earnings with respect to their adoptive father's earnings (β_{Iy-nb}) was 0.139 (± 0.033), a reduction of two-fifths. This difference between β_{Iy-nb} and β_{Iy-b} could reflect the fact that fathers and their adopted children have fewer genes in common, but it could also have other sources.¹³

Adoptions must usually be approved by social workers, who try to weed out unpromising applicants. If this form of selection succeeds, the correlation between income and parental competence should fall, lowering β_{Iy-nb} . One way to assess the magnitude of this bias is to look at the effect of biological fathers' incomes when they have no social contact with their children. Björklund and Chadwick (2003) report that if sons never lived with their biological fathers, the elasticity of a son's earnings with respect to his biological father's earnings is 0.09 (again, about two-fifths of β_{Iy-b}).¹⁴

Heritability estimates for earnings. We can also check these estimates using indirect methods. The fraction of the observed ("phenotypic") variance explained by genetic variation is commonly called "heritability" and denoted as h^2 . The simplest way to think about h^2 is to imagine a sample of monozygotic (MZ) twins reared apart. MZ twins occur when an egg splits

after fertilization, making the twins genetically identical. If MZ twins were separated immediately after their egg divided, transplanted to the wombs of randomly selected mothers, and raised by those mothers, the correlation between separated twins' characteristics (r_{MZA}) would tell us how much of the variation in each characteristic was traceable to genetic factors. In other words, r_{MZA} would equal h^2 .

MZ twins are never raised in this way, but there are other ways of approximating h^2 . One common method is to compare monozygotic and dizygotic twins. Dizygotic (DZ) twins occur when two separate eggs are fertilized simultaneously. As a result, DZ twins share only half their genes. If genes all had additive effects and there were no assortative mating, the earnings correlation between DZ twins separated at the moment of conception (r_{DZA}) would be $0.5h^2$. Assortative mating will raise r_{DZA} while non-additive genetic effects will lower it, so r_{DZA} should not be too far from 0.5. If growing up together raises the correlation between MZ and DZ pairs by the same amount (e_T^2), the correlations between MZ and DZ twins reared together will be

$$r_{MZT} = r_{MZA} + e_T^2 = h^2 + e_T^2 \quad (1)$$

$$r_{DZT} = r_{DZA} + e_T^2 \sim 0.5h^2 + e_T^2 \quad (2)$$

Subtracting equation 2 from equation 1 and rearranging we get:

$$h^2 \sim 2(r_{MZT} - r_{DZT}) \quad (3)$$

This approximation will be too high if nonadditive genetic effects are more important than assortative mating or if growing up together raises r_{MZA} more than r_{DZA} .¹⁵

Björklund, Jäntti, and Solon (2005) present data on Swedish twins' average earnings for 1987, 1990, and 1993. Using equation 3, $h^2 = 0.39$ for both male and female earnings. But for h^2 to be 0.39 the effect on twins' of growing up together must be zero or even negative. Since the correlation between adopted siblings reared together averages 0.074, the hypothesis that

growing up together does not make people's earnings more alike is implausible. When Björklund et al. drop this assumption and take account of their data on biological siblings and half-siblings reared together and apart, their best estimate of h^2 is 0.20 for men and 0.13 for women. In Sweden, therefore, the traditional "twin method" appears to overstate the heritability of earnings by a factor of about two for men and three for women. That could be because e_T^2 is larger for MZ than DZ twins or because assortative mating does not fully offset the various nonadditive effects of genes.

Parents and children also share half their genes, so if the effects of assortative mating offset the nonadditive effects of genes, the earnings correlation between biological parents and children who have no social contact (r_{ly-ba}) should again be roughly $0.5h^2$. Thus if h^2 is 0.20 for Swedish men, r_{ly-ba} should be roughly 0.10. This estimate appears consistent with Björklund and Chadwick's estimate that $\beta_{ly-ba} = 0.09$ and with Björklund, Lindahl, and Plug's finding that β_{ly-b} exceeds β_{ly-nb} by 0.093. Our suggestion that genetic resemblance explains about two-fifths of β_{ly} in Sweden is thus what we would expect if Björklund, Jäntti, and Solon's best estimate of h^2 were correct.

Is America like Sweden? Sacerdote (2004) estimated the intergenerational elasticity of household income for 1,413 Korean children adopted by American families during the 1970s and early 1980s and for 1,176 biological children raised in the same American families. Placement was random, but mean age at adoption was 21 months. The intergenerational household income elasticity was 0.161 (± 0.051) for biological children and -0.087 (± 0.037) for adopted children. The significant negative elasticity for adopted children is an unresolved puzzle.

We have better American data on adopted children's educational attainment than on their income, and these data suggest that the role of genes in educational attainment is similar in the

United States and Sweden. The 1992 WLS followup asked respondents about their children's educational attainment. When Plug (2004) regressed these children's educational attainment on their parents' attainment, the bivariate coefficients for mothers and fathers averaged 0.46 for the 15,871 biological children and 0.27 in for the 610 adopted children. When Björklund, Lindahl, and Plug (2004) made similar calculations for Swedish children born in roughly the same period, the coefficients averaged 0.235 for biological families and 0.120 for adoptive families. An extra year of parental education was thus twice as important in Wisconsin as in Sweden. Nonetheless, the reductions in the coefficients among adopted children were about the same in Wisconsin (41 percent) as in Sweden (49 percent). (In Sacerdote's sample of Korean adoptees the reduction was 77 percent.)

The most representative earnings data on American twins comes from Paul Taubman's (1976) survey of twins who served in the armed forces during World War II. Using equation 3, h^2 for white men's 1973 earnings is 0.48.¹⁶ If twin data overstate the heritability of men's earnings by a factor of two in the United States as well as Sweden, the true value of h^2 for white American men would have been about 0.24 and the predicted value of r_{fy} for biological sons who never lived with their fathers would have been about 0.12. Father-son elasticities for annual earnings were around 0.30 during the 1970s in the United States (Solon, 1992), and the correlations were similar to the elasticities. If r_{fy} was about 0.12 for biological sons who had no contact with their father and 0.30 for sons who grew up with their fathers, genes would explain about two-fifths of r_{fy} in the United States as well as Sweden. The uncertainties surrounding these estimates are obvious.¹⁷

How might genes influence earnings? The most direct way to assess genes' contribution to the various traits that link parents' economic status to their children's status is once again to

compare children raised by biological and adoptive parents. We have already mentioned evidence suggesting that about two-fifths of educational resemblance between biological parents and children derives from genetic resemblance. But Plug (2004) also shows that once parental education is controlled the *direct* effect of parental income on children's educational attainment is essentially identical for biological and adopted children.

Two small American samples provide analogous data on the association between parental SES and children's IQ. The Texas adoption study compared 256 adopted children with 93 biological children reared in the same families. The Minnesota adoption study compared 150 late adolescents who had been adopted before their first birthday with 237 late adolescents raised in a different set of biological families. The two Minnesota samples were recruited in rather similar ways and had similar economic characteristics. The Texas and Minnesota studies both suggest that the association between parental SES and older children's IQ is reduced by about two-fifths when children are adopted.¹⁸ Table 2 suggests, however, that IQ scores play a fairly modest role in explaining intergenerational economic inheritance. Comparing columns 2 and 4, for example, the reductions in β_{Isei} average about a third. For earnings and family income, the reductions in elasticities average 27 percent for daughters and 9 percent for sons. This pattern suggests that genes are not generating intergenerational economic resemblance primarily by influencing IQ.

Genes also influence noncognitive traits. Loehlin (2005) reports parent-child correlations for the "Big Five" averaging 0.13 in biological families, 0.10 for biological parents who did not raise their children, and 0.04 for adoptive parents and children. But this does not help us explain genes' contribution to r_{ly} , since the Big Five have little relationship to parents' economic success.

Physical and mental illnesses often have a significant genetic component, so when they affect earnings they can contribute to r_I . Case, Lubotsky, and Paxson (2002) found no evidence that the association between parental income and children's health was weaker when children were not biologically related to their parents, but their sample of nonbiological children was relatively small and many diseases with a genetic component only manifest themselves in adulthood, so the last word has not been written on this issue.

This survey leads us to two conclusions about genes and earnings. First, our best guess is that genes account for about two-fifths of r_I in Sweden. We are inclined to think that the figure is similar in America, but the data are far less satisfactory. Second, the normative implications of such estimates depend on whether genes exert their influence through traits like "ability" that are widely seen as legitimate sources of economic inequality, or through traits like beauty and skin color that are not seen this way. We cannot yet answer that question.

4. Values and r_I .

Individuals whose top priority is economic success tend to do better economically than individuals whose top priority is spending time with their children or shooting ducks. We have no data on the weight that adults assign to occupational standing or earnings relative to their other goals in life, but we do have data on a few elements of such a measure.

Time. Working reduces the time available for most other activities. Since economic success usually requires unusually long hours, it leaves even less time for everything else. Altonji and Dunn (2000) have investigated the intergenerational correlation of hours worked. After taking account of the positive relationship between wages and hours worked, which should control both the effect of wages on motivation to work and the effect of skills on the availability

of work, they attribute the remaining variation in hours to individual preferences. Preferred hours account for 15 percent of the variation in annual earnings for sons and 56 percent for daughters. The effect of a father's preferences on his son's preferences explains only 4 percent of the covariance between fathers' and sons' annual earnings, but the effect of a mother's preferences explains 37 percent of the covariance between mothers' and daughters' earnings.

Location. Many people also want to live in the community where they grew up or in a nearby community with similar characteristics. Page and Solon (2003) have used the Panel Study of Income Dynamics (PSID) to compare the places where parents lived in 1968 to the places where their grown sons lived in 1987-91. Among sons raised in nonmetropolitan areas, 54 percent still lived in such an area as adults. Only 14 percent had moved to a major metropolitan area (more than a million residents). Among sons raised in major metropolitan areas, in contrast, 83 percent still lived in such an area, and only 6 percent had moved to a nonmetropolitan area. Sons raised in large metropolitan areas earned 46 percent more than sons raised in nonmetropolitan areas.

When Bonggeum Kim (personal communication, 3/17/04) controlled the size of the community and the region of the country in which a son was raised, the father-son earnings elasticity in the PSID fell from 0.447 to 0.315. The wage gap between large and small communities was partly attributable to cost-of-living differences, but this adjustment only lowered the father-son elasticity from 0.447 to 0.402 (Kim, 2002). Kim (2004) argues that the remaining wage gap between large and small communities arises because skilled men move to larger places where their skills are more valuable. But skill differences between men raised in large and small communities are also partly endogenous. If children growing up in a small

community want to stay there, they have little incentive to develop skills that would be valuable only if they moved.

Nonmonetary job characteristics. Workers also face tradeoffs between maximizing their hourly wages and job characteristics like safety, job security, interesting work, and proximity to home. Jencks, Perman, and Rainwater (1988) constructed an index of jobs' nonmonetary rewards based on the way workers with different job characteristics rated their own job relative to an average job. A job's score on their index of nonmonetary characteristics correlated 0.50 with its pay. Workers' ratings of their own job correlated 0.48 with their pay and 0.62 with the value that the average worker assigned to the job's nonmonetary characteristics.

We have not found data on whether parents and children make similar tradeoffs between jobs' monetary and nonmonetary rewards, but we would expect some resemblance. Fathers who are self-employed tend to have sons who are self-employed, even when the father and son work in different occupations (Klatzky and Hodge, 1971). Parents who work in occupations that require a lot of education relative to their pay also tend to have children who do the same (Hauser and Warren, 1997). Conversely, if parents always choose the best paid job they can find, their children are likely to have somewhat similar priorities.

What can we say about the importance of values to r_I based on currently available data? Altonji and Dunn find that preferences about hours vary more for daughters than for sons, and that daughters' priorities are more influenced by their mother's priorities than their father's. If we take account of inherited locational preferences, however, values probably explain at least a tenth of β_{Iy} , even for sons. If that is the case, and if two-fifths of β_{Iy} in biological families is explained by genetic resemblance, the combined effects of genes and values would account for something like half of β_{Iy} among both sons and daughters. As with all guesses, however, caveat emptor.

5. Estimating r_I when opportunity is equal

Most thoughtful advocates of equal opportunity concede that r_I would probably be positive even if labor markets were completely meritocratic and developmental opportunities were completely equal. Nonetheless, most still assume that equal opportunity would make r_I substantially smaller than it is now.¹⁹ While that is possible, it is far from certain. Everything depends on the “denominator problem.”

The easiest way to illustrate the denominator problem is to decompose the variance of economic success ($\text{Var}Y$) into the variance explained by a parent’s success ($\text{Var}\hat{Y}$) and the variance not explained by the parent’s success ($\text{Var}Y_e$). Since $r_{Iy}^2 = \text{Var}\hat{Y} / \text{Var}Y$, decomposing $\text{Var}Y$ yields:

$$r_{Iy}^2 = \text{Var}\hat{Y} / (\text{Var}\hat{Y} + \text{Var}Y_e) \quad (4)$$

If $\text{Var}\hat{Y}$ falls while $\text{Var}Y_e$ remains constant, r_{Iy} must fall. This is the scenario that most advocates of equal opportunity envision. The “denominator problem” arises because equal opportunity can also affect $\text{Var}Y_e$. Indeed, if equal opportunity means that all workers with comparable performance-related attributes get equally desirable jobs, $\text{Var}Y_e$ is likely to fall substantially. If $\text{Var}Y_e$ falls proportionately more than $\text{Var}\hat{Y}$, then r_{Iy} will actually rise.

Consider Sweden between 1956 and 1970, when large employers and labor unions sought to eliminate wage differences between similar jobs in different industries, communities, and workplaces. Hibbs and Locking (2000) report that the squared coefficient of variation for blue-collar industrial wages fell by 33 percent between 1962 and 1970. If these data were available back to 1956, the decline would presumably be even larger. We have no data on how this decline was allocated between $\text{Var}\hat{Y}$ and $\text{Var}Y_e$. Before 1956 blue-collar fathers in high-wage

workplaces were presumably helping their sons get jobs in the same place, so reducing wage differences between workplaces presumably lowered $\text{Var } \hat{Y}$. Blue-collar sons also tended to live near their fathers, so reducing geographic wage differences presumably lowered $\text{Var } \hat{Y}$ too. Nonetheless, it is hard to imagine that such changes reduced $\text{Var } \hat{Y}$ by 33 percent. Thus if all else had remained equal, r_{Iy} would probably have risen among blue-collar fathers and sons.

One common response to this argument is that it illustrates the advantages of using β_I rather than r_I to measure inequality of opportunity. If workers cared only about absolute differences, this argument might be correct. But workers also care about their rank in the economic hierarchy.²⁰ Suppose, for example, that Sweden had cut β_I from 0.50 in 1950 to 0.25 in 1980 and had eliminated *all* other sources of variation in earnings. This change would have had two effects. First, the children of Sweden's best and worst paid workers in 1950 would have enjoyed more equal living standards in 1980. Second, the children of the best paid workers in 1950 would all, without exception, have been the best paid in 1980. Conversely, the worst paid workers in 1980 would all have been children of the worst paid workers in 1950. We doubt that many people would view such a change as a step towards more equal opportunity.

The American labor market is larger and more chaotic than the Swedish labor market, so an "equal pay for equal work" rule could have even more dramatic effects here than in Sweden. American workers' measured characteristics explained less than half the variation in their occupational rank and less than a third of the variation in their annual earnings during the 1960s and 1970s (Jencks et al, 1979), and the same is probably true today. Some of the remaining variation is traceable to unmeasured skills and personality traits, but earnings vary a lot even when we follow the same individual over time. Gottschalk and Moffitt (1992) found that a third of the variance in American workers' annual earnings was caused by year-to-year fluctuations

around a worker's average earnings over the course of a decade. Some of these fluctuations presumably reflected changes in workers' preferred job characteristics, but some reflected the fact that the American labor market is not governed by the law of one price. Year-to-year fluctuations cannot be correlated with family background, which is fixed. Thus if meritocracy reduced fluctuations of this kind, r_{ly} would rise. Indeed, Solon's (1992) upward revision of r_{ly} flowed directly from his argument that economists should be concerned only with fathers' and sons' lifetime earnings and should treat year-to-year fluctuations like measurement error.

Eliminating variation in children's opportunities to develop their talents would also have unpredictable effects on r_l . If all children had the same opportunity to develop their innate abilities, some of the genes that now influence earnings would no longer do so. But *some* genes would still affect earnings by affecting performance. If we use G to denote the maximum potential earnings of individuals with a given set of productivity-related genes and use V to denote the fraction of potential earnings that individuals take in monetary form (rather than leisure, safety, or intrinsic interest), equal opportunity would mean that an individual's earnings were:

$$Y_i = V_i G_i. \quad (5a)$$

Note the absence of an error term. Taking logarithms:

$$\ln Y_i = \ln V_i + \ln G_i \quad (5b)$$

In equilibrium, the variance of logged earnings would be the same for parents and children, so:

$$\text{Var}(\ln Y) = \text{Var}(\ln V) + \text{Var}(\ln G) + 2\text{Cov}(\ln V, \ln G) \quad (6)$$

Using the subscripts p and c to denote a parent and a child, the covariance of a parent's earnings with a child's earnings is:

$$\text{Cov}(\ln Y_p, \ln Y_c) = \text{Cov}(\ln V_p, \ln V_c) + \text{Cov}(\ln V_p, \ln G_c) + \text{Cov}(\ln V_c, \ln G_p) + \text{Cov}(\ln G_p, \ln G_c) \quad (7)$$

Dividing equation 7 by equation 6 the intergenerational earnings correlation in a society with equal opportunity (r_{ly-EO}) is:

$$r_{ly-EO} = \frac{\text{Cov}(\ln V_p \ln V_c) + \text{Cov}(\ln V_p \ln G_c) + \text{Cov}(\ln V_c \ln G_p) + \text{Cov}(\ln G_p \ln G_c)}{\text{Var}(\ln V) + \text{Var}(\ln G) + 2\text{Cov}(\ln V \ln G)} \quad (8)$$

We cannot estimate equation 8 from existing data. We know, for example, that individuals currently make tradeoffs between earnings and jobs' nonmonetary characteristics (including hours of work). Otherwise there would be no missionaries and few actors. But we do not know how much of the variance in annual earnings is currently attributable to these tradeoffs, so we cannot estimate $\text{Var}(\ln V)$. We also know that genetic differences currently explain some of the variation in hourly wages, but we do not know how much of this variation derives from differences in job performance, so we cannot estimate $\text{Var}(\ln G)$. The intergenerational correlation of $\ln V$ is likely to be positive, but we have no evidence on its magnitude. Nor do we know the correlations between $\ln V$ and $\ln G$ either within or across generations. Nor do we know how any of these parameters would change if labor markets were completely meritocratic and developmental opportunities were completely equal.

We can, however, get some sense of how large r_{ly-EO} might be with a few simplifying assumptions. First, let $\text{Var}(\ln G) = M^2 \text{Var}(\ln V)$. Normalizing $\text{Var}(\ln V)$ to 1.00 then makes $\text{Var}(\ln G) = M^2$. Second, assume that while high potential wages induce people to work more hours, working more hours also makes people put more weight on their job's nonmonetary characteristics, so that the individual level correlation of $\ln V$ with $\ln G$ is zero. Third, assume that $\ln V$ is also uncorrelated with $\ln G$ across generations. Fourth, assume that the intergenerational correlation of G is 0.50 and denote the intergenerational correlation of V as r_{IV} . Then equation 8 reduces to:

$$r_{ly-EO} = (r_{IV} + 0.5M^2)/(1 + M^2) \quad (8a)$$

If there are *no* genetic limitations on potential earnings, $M = 0$ and $r_{Iy-EO} = r_{IV}$. Now assume that r_{IV} is positive but small (say 0.2). If $M = 0.5$ rather than zero, r_{Iy-EO} will rise from 0.20 to 0.26. If productivity-related genes generate as much variance in earnings as values, $M = 1$ and r_{Iy-EO} rises to 0.35. If $M = 2$, r_{Iy-EO} rises to 0.44. If r_{IV} exceeds 0.20, r_{Iy-EO} will be correspondingly larger.

Corak (2004) suggests that intergenerational earnings elasticities currently exceed 0.4 in the United States, Britain, and France.²¹ His estimates for Germany and Sweden are close to 0.3, and his estimates for Canada, Denmark, Finland, and Norway are less than 0.2. In countries like Canada, Denmark, Finland, and Norway, therefore, making labor markets more meritocratic and developmental opportunity more equal could easily raise r_I rather than lowering it. Whether such a change would make people feel that they lived in a more just society is unclear.

Our earlier argument suggested that positive values of r_I are not in themselves evidence that opportunities are unequal. This section suggests two further conclusions:

- The value of r_I can fall for either of two reasons: because parental success has less impact on children's success or because other sources of variation in economic success have become more important. Conversely, r_I can rise either because parental success is having more impact or because other sources of variation are becoming less important.
- Unless r_I is very high, declines in r_I do not necessarily mean that opportunity is becoming more equal, and increases in r_I do not necessarily mean that opportunity is becoming less equal.

Unlike our earlier conclusion, however, these conclusions rest on the potentially controversial assumption equal opportunity should reduce random variation in the earnings of individuals with the same skills and preferences.

6. How *can* we measure unequal opportunity?

If we want to know whether opportunity is becoming more or less equal, we need to track the effects of the specific intergenerational linkages that violate our norms regarding meritocracy and developmental opportunity. In the case of the labor market, these mechanisms could include:

1. Random variation in the wages or working conditions of equivalent workers.
2. Employers who treat socioeconomic background as evidence of potential productivity.
3. Economically successful parents using their social networks to help their grown children find the best available job for someone with their abilities.

In the case of developmental opportunity, three other mechanisms are potentially relevant:

4. Economically successful adults making better use of the time they spend with their children to develop traits that the labor market rewards.
5. Economically successful parents spending more on goods and services that enhance their children's labor market prospects.
6. Economically successful parents living in places that provide more public good to children.

This section uses the last two mechanisms to suggest the kinds of measures we think potentially useful for assessing trends inequality of opportunity.

Effects of family income. Parental income is positively correlated with preschool children's school test scores and health status. Among older children parental income is also correlated with high school grades, high school graduation, college graduation, post-educational employment, staying out of prison, and marrying before having a baby. Such correlations may

indicate that affluent parents use their money to buy goods and services that improve their children's life chances, but they may also reflect the influence of genetic endowment or parenting practices.

Susan Mayer's 1997 book, *What Money Can't Buy*, is the most comprehensive effort to separate the effects of parental income from other sources of r_I . Mayer investigated the effects of parental income on children's test performance, behavior problems, high school graduation rates, college attendance, and post-educational employment. She also looked at the effect of income on teenage daughters' fertility and their chances of having a child out of wedlock. Her findings for test performance illustrate her general approach. She found that a child's test performance between the ages of five and seven was fairly strongly correlated with the parents' average income since the child's birth. But she also found that three-fifths of this correlation was explained by the fact that more affluent parents had higher test scores themselves, were better educated, had fewer children, had their children when they were older, and were more likely to be white.

Mayer also investigated whether other unmeasured factors could be influencing both parental income and children's test scores by comparing the correlation of children's test scores with their parents' average income before and after the test was given. If income were only a proxy for stable but unobserved parental characteristics that also influenced test performance, pre-test and post-test incomes should be equally good predictors of test performance. If the entire correlation between pre-test income and test performance were causal, there should be no correlation between post-test income and test performance once pre-test income was controlled. Based on these comparisons Mayer concluded that three-quarters of the association between parental income and children's test performance was explained by other stable determinants of

both test scores and income. The remaining quarter appeared to be a true effect of pre-test parental income on children's scores.²²

Mayer found broadly similar patterns when she investigated the impact of parental income on behavioral problems, high school graduation, having a child as a teenager, and becoming a single mother. Note that if parental income also had a small impact on many other outcomes, these small effects could ultimately explain a nontrivial fraction of r_{ly} . Mayer also found one important exception to the rule of small effects. Children's chances of attending college were far more strongly related to their parents' income when the children were in high school than to parental income when children were in their twenties. This pattern suggests that parental income during high school really does affect college attendance.²³

The fact that parental income affects college attendance should come as no surprise. Unlike learning math, finishing high school, or avoiding pregnancy, attending college usually requires someone to write a series of sizable checks. The fact that parental income during adolescence affects college attendance also rebuts the most common objection to Mayer's "before and after" method. Economists often argue that households adjust their current consumption in light of their expectations about their future income. If this were common, the "before and after" method would be misleading. But if parents who expected their income to rise often borrowed to help their children, financing higher education should be their top priority. The fact that family income during adolescence appears to matter more than subsequent income for college attendance suggests either that parents' expectations are seldom accurate or that such expectations have little influence on parents' willingness to defray their children's college bills.

Mayer did not investigate the effect of parental income on children's health, and we have not seen any natural experiments that throw light on this question. Based on Mayer's findings

about college attendance, we would expect parental income to have its largest health effects through medical care. We would also expect these effects to be much larger among the uninsured than the insured. Roughly a fifth of children in families with incomes less than twice the poverty line were uninsured in 2002. A third of these children had no usual source of care, and a sixth had not seen a doctor in the past year. Almost all insured children had a usual source of care, regardless of income.²⁴

The role of government. Advocates of equal opportunity level two kinds of charges at governments: that they distribute public resources in ways that favor the rich over the poor, and that even when they give more to the poor they do not do as much as equal opportunity requires. Again, test scores are a good place to begin. The most popular explanation for test score differences between richer and poorer children is that school districts serving affluent children spend more per pupil. If this were the explanation, however, low-income children should learn more in districts with high per pupil expenditure. Three decades of research have failed to find such a relationship (Hanushek, 2003).²⁵ In addition, if the link between parental income and children's school achievement reflected disparities in school spending, California's decision to equalize expenditure on rich and poor children should have made achievement scores more equal. That did not happen.²⁶

The weak relationship between expenditures and test performance probably reflects the fact that effective teachers gravitate to schools that serve more advantaged (and more white) students regardless of whether these schools pay more or have smaller classes (Hanushek, Kain, and Rivkin, 2004; Hanushek, Kain, O'Brien, and Rivkin, 2004). As a result, schools that enroll children of successful parents can get their pick of teachers even if they spend no more than schools that enroll less privileged children. The only way to eliminate the correlation between

parental income and teacher quality would be for schools serving disadvantaged students to pay far more than their competitors. That is not politically feasible in America's decentralized system.

Reducing the effect of parental income on college enrollment would be easier than reducing the effect on teacher quality. Kane (1999) reports that when states raise tuition at their public colleges, the fraction of state residents entering college tends to fall, especially among students from low-income families. Since the 1960s, however, states have raised public college prices faster than most residents' incomes. A sixth of all children lived in family with money incomes below the federal poverty line in both 1967 and 2001. In 1967 tuition, room, and board at public colleges averaged 29 percent of poverty line. By 2001 the ratio had risen to 46 percent.²⁷ Most of this increase was driven by tuition charges, not room and board. Such increases presumably help explain why Ellwood and Kane (2000) found that the link between parental income and college entrance tightened between 1982 and 1992.

The legislators who raised state college tuition often defended their votes by arguing that affluent families could afford to pay more, and that poorer families could get federal financial aid. But the Pell Grant program, which has provided federal scholarships for low-income students since 1972, did not reduce the association between parental income and college attendance (Hansen, 1983), perhaps because potential recipients could not know how much help they would get -- or even whether they would qualify -- until after they had been admitted to a college. The Social Security Administration's financial aid program for children of deceased, disabled, or retired beneficiaries, in contrast, told children how much help they could expect as soon as they became eligible. Perhaps for this reason, the Reagan Administration's decision to abolish the program led to a sharp decline in college attendance among children of widows

(Dynarski, 2003). Evidence of this kind suggests that reducing the effect of parental income on college attendance would probably be much easier than reducing the effect of parental income on access to the most effective elementary and secondary school teachers. All we would need to do is cut prices in a predictable and transparent way.

What about health care? Most nations provide almost all of their citizens with low-cost health care. In the United States, a sixth of the population lacked insurance in 2002, up from a seventh in 1984. The effects of insurance on health are hard to estimate, because the causal connections run both ways. The cost of care deters the uninsured from seeking care when they are sick, but this effect can be masked by the fact that people with health problems are more likely to seek coverage.²⁸ The best way to identify the effect of coverage is therefore to ask what happens when more people become eligible for free care. Currie and Gruber (1996), for example, studied the effect of expanding Medicaid eligibility between 1984 and 1992. Children's chances of seeing a doctor increased faster in states where eligibility expanded more. Childhood deaths also fell faster in these states, suggesting that free care really saved lives.²⁹ Levy and Meltzer (2004) find that expanding insurance coverage usually improves adult health as well.

This brief review suggests that while parental income has a direct effect on many characteristics of children, its effects on outcomes that do not require significant cash expenditures are relatively small. But when significant cash expenditures *are* required, as they are for both college attendance and medical care for the uninsured, the effects of parental income are important. College graduation is probably the outcome with the strongest effect on children's economic prospects. Children's health and survival is probably the outcome that parents care most about. The fact that these outcomes depend on parental income has two

important implications. First, the best currently available evidence suggests that more generous public programs could reduce the impact of parental income on both children's access to higher education and their access to medical care. The fact that the United States has done less than most rich democracies to reduce this relationship suggests that we should be skeptical about the claim that America has an unusually strong commitment to equal opportunity. Second, if we want to know whether a country is doing all that it can to equalize opportunity, changes in the effects of parental income on cash-dependent outcomes provide a better measure than changes in *r*.

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Table 1: Percentage of adults who agreed minus the percentage who disagreed with five statements about distributive justice: Japan, West Germany, Great Britain, and the United States in 1991.

Question	Japan	West Germany	Great Britain	United States
“The fairest way of distributing wealth and income would be to give everyone equal shares.”	-39	-37	-32	-51
“It is just luck if some people are more intelligent or more skillful than others, so they don’t deserve to earn more money.”	-37	-27	-56	-66
“People who work hard deserve more money than those who do not.”	86	89	93	89
“People are entitled to keep what they have earned even if this means some people will be wealthier than others.”	56	83	72	88
“It’s fair if people have more money and wealth, but only if there are equal opportunities.”	40	72	71	74

Source: International Social Justice Project, as reported in Marshall, Swift, and Roberts (1997: 246).

Table 2: Coefficients of father's occupation and family income in 1957 when predicting children's occupation, earnings, and family income in 1991-92 using various controls in the Wisconsin Longitudinal Survey

Dependent and independent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	N
1. Daughter's occupational SEI score in 1992 on father's occupational SEI score in 1957	.171 (.016)	.119 (.018)	.102 (.018)	.076 (.018)	.034 (.017)	.034 (.017)	.029 (.017)	2,961
2. Daughter's logged earnings in 1991 on logged family income in 1957	.095 (.034)	.057 (.039)	.049 (.040)	.039 (.039)	.022 (.039)	.022 (.039)	.015 (.039)	2,264
3. Daughter's logged family income in 1991 on logged family income in 1957	.152 (.028)	.094 (.031)	.082 (.032)	.073 (.031)	.061 (.031)	.058 (.031)	.053 (.031)	2,790
4. Son's occupational SEI score in 1992 on father's occupational SEI score in 1957	.309 (.019)	.232 (.022)	.212 (.023)	.153 (.021)	.092 (.019)	.070 (.019)	.069 (.019)	2,737
5. Son's logged earnings in 1991 on logged family income in 1957	.207 (.023)	.125 (.026)	.125 (.026)	.114 (.026)	.094 (.025)	.093 (.025)	.095 (.025)	2,353
6. Son's logged family income in 1991 on logged family income in 1957	.235 (.024)	.156 (.027)	.153 (.027)	.143 (.027)	.122 (.026)	.120 (.026)	.121 (.026)	2,576
Controls:								
Father's education and (in rows 2, 3, 5, and 6) father's occupation		yes	yes	yes	yes	yes	yes	
Mother's education, number of siblings			yes	yes	yes	yes	yes	
Eleventh grade IQ score				yes	yes	yes	yes	
Educational attainment (see text)					yes	yes	yes	
Occupational aspirations in 1957						yes	yes	
"Big Five" personality traits in 1992							yes	

Source: Authors' tabulations using the Wisconsin Longitudinal Survey. Coefficients in bold are twice their standard error.

Footnotes

¹ We are grateful to the Russell Sage Foundation for financial support and to Tom Cook, Miles Corak, Gosta Esping-Anderson, Michael Hout, Jane Mansbridge, Jal Mehta, Mathias Risse, Adam Swift, and Scott Winship for invaluable criticisms that led to drastic revisions of an earlier draft. Any remaining errors are our own.

² Using y_p to denote the standard deviation of a parent's logged earnings and y_c to denote the standard deviation of a child's logged earnings, $r_I = \beta_I(\sigma_{yp}/\sigma_{yc})$. Because σ_y rises with age, σ_{yp} usually exceeds σ_{yc} and r_I usually exceeds β_I if parents were older than children when the earnings data were collected. But in countries where σ_y is rising over time, this bias may be offset if children's earnings are measured in a later period than parents' earnings.

³ If we denote the percentages in Table 1 as P_T , then if all respondents answered a given question the percentage who agreed would be $(P_T + 100)/2$. Table 1 therefore implies that about 17 percent of Americans and 37 percent of West Germans thought that individuals did not deserve to earn more just because they were more intelligent or more skillful, with the British and Japanese falling in between.

⁴ Michael Young (1958) invented the term "meritocracy" to describe a dystopia, but in America it has usually had positive rather than negative connotations.

⁵ See Jencks (1988) for a discussion the diverse meanings that an elementary school teacher might assign to "equal educational opportunity" when allocating her time between different children in her classroom.

⁶ Some of those who said they disagreed with the statement in line 2 of Table 1 could, however, have been rejecting the factual premise that "it is just luck if some people are more intelligent or more skillful than others" rather than the moral inference ("so they don't deserve more money").

⁷ Roemer (1998) makes a similar argument for slightly different reasons.

⁸ Occupational SEI scores take their name from Duncan's (1961) "Socio-Economic Index for All Occupations," and are based on the educational attainment and earnings of workers in a given occupation. Measures of r_I based on SEI scores are consistently higher than measures of r_I based on either prestige rankings (Featherman, Jones, and Hauser, 1975; Featherman and Hauser, 1976) or an occupation's mean earnings (Hauser and Warren, 1997).

⁹ Bowles, Gintis, and Osborne (2005) reach similar conclusions using other data.

¹⁰ Corcoran and Jencks (1979) present data from three surveys of men over the age of thirty suggesting that test scores and education typically explain 84 percent of the bivariate relationship between parental characteristics and grown sons' SEI scores. None of these surveys includes a measure of parental income.

¹¹ Farkas (2003) provides a good review on this literature.

¹² The elasticities of children's earnings with respect to father's earnings are 0.260 for sons and 0.203 for daughters. All elasticities are slightly higher when the mother's and father's earnings are summed.

¹³ Björklund et al investigate and reject the hypothesis that selective placement plays a significant role in β_{ly-nb} .

¹⁴ β_{ly-nb} may not be a "pure" genetic effect if these biological fathers' earnings are also correlated with the quality of their children's early environment. Also the relationship between fathers' and sons' earnings is not linear in logs, so the elasticity for adopted sons is not necessarily representative of what one would obtain for all sons.

¹⁵ Critics of the twin method often argue that identical twins are treated more alike than fraternal twins, but this is not the right test. Identical twins reared apart are also treated more alike than

random children, because their genes are more alike and genes influence the environment. The relevant test is whether the *increase* in environmental resemblance is greater when identical twins are reared together than when fraternal twins are reared together.

¹⁶ Orley Ashenfelter and his collaborators also collected data on twins' hourly wages in the 1990s. This sample overrepresents identical twins and probably overrepresents twins who remain close to one another as adults. Ashenfelter and Rouse (1998) provide details. Their analysis focuses on returns to schooling among identical twins, but Rouse (personal communication) reports that the MZ and DZ twin correlations for hourly wages were 0.63 and 0.37 respectively. Using equation 3, the implied heritability of hourly wages is $(0.63-0.37)/(1-0.5) = 0.52$. Miller, Mulvey, and Martin (1995) also present estimates of r_{MZ} and r_{DZ} for Australia, but their income estimates are based on the average earnings of workers in a given occupation.

¹⁷ For a somewhat different approach to estimating the genetic component of intergenerational economic resemblance see Bowles and Gintis (2002).

¹⁸ In Minnesota the multiple correlation of father's occupation and total parental income with adolescent children's IQ scores was 0.28 for biological children and 0.16 for adopted children (Scarr and Weinberg, 1978). In Texas, the adoptive parents' SES (based on the father's occupation and both parents' educational attainment) correlated 0.17 with the biological children's IQ scores and 0.11 with the adopted children's scores between the ages of 13 and 24 (Loehlin, Horn, and Willerman, 1989). There was some selective placement in both Minnesota and Texas, so some of the resemblance between adoptive parents and their adopted children is genetic in origin, but this bias appears to be tiny. One fairly consistent feature of comparisons between children reared by biological and adoptive parents is that the regression coefficient of a rearing parent's educational attainment in an equation predicting a child's IQ score drops more

among adopted children than does the coefficient of father's occupation or family income. This is true in Scarr and Weinberg's (1978) Minnesota study and in the older studies reviewed by Jencks et al (1972: 276). Capron and Duyme's (1989) small but carefully selected sample of French adoptive parents also suggests that their SES has a sizable effect on their adopted children's IQ scores.

¹⁹ This generalization rests on three facts. First, we have made this argument to a number of audiences. No one has ever said that they were convinced, and many have said they were not. We also received the extensive written comments on earlier drafts of this paper from thoughtful colleagues and anonymous referees. None of them were convinced either. Finally, we have never seen anyone else make this argument in print, although Swift (2004) raises some of the same issues.

²⁰ Parducci (1995) reviews the algorithms that both experimental subjects and ordinary citizens use to assess the adequacy of economic rewards.

²¹ Mazumder (2001 and 2005) reports even higher elasticities, although these may reflect differences between the standard deviations for fathers and sons rather than high values of r_I .

²² Shea (2000) reached conclusions similar to Mayer's using union membership, job loss, and industry wage premiums as instrumental variables for identifying the true effects of parental income.

²³ Children from low-income families can usually scrape together enough money to attend a local public college if they work while in college and borrow heavily. But these requirements make higher education far less attractive to poor students than to affluent students. Students who have to work will have little leisure, and students who have to borrow know that it will be a long time

before their investment raises their standard of living. An affluent parent can eliminate these costs by paying the bills, making college attendance considerably more attractive.

²⁴ The data in this paragraph are from National Center for Health Statistics (2004:254, 256, and 350). Among insured poor children, 94 percent had a usual source of care in 2001-02.

²⁵ Card and Krueger (1996) present evidence that higher school spending affect adult earnings, at least before World War II, but this claim is controversial (see the other papers collected in Burtless, 1996).

²⁶ See Downes (1992). For some contrary evidence see Card and Payne (1998).

²⁷ Tuition is the mean for public four-year institutions other than universities and come from National Center for Education Statistics (2002:359). Poverty thresholds are from US Bureau of the Census (2000 and 2002).

²⁸ This presumably explains why Case, Lubotsky, and Paxson did not find that insurance coverage explained any of the correlation between parental income and children's health.

²⁹ Conley and Springer (2001) also found that infant mortality fell faster when OECD countries increased their health spending.