CHAPTER SEVEN

Inequality of Conditions and Intergenerational Mobility: Changing Patterns of Educational Attainment in the United States

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In the 1980s and 1990s, most advanced industrialized countries experienced substantial increases in inequality, as measured by labor market earnings, total family income, and wealth (see Blau and Kahn 2002; Freeman and Katz 1995). In some countries, such as the United States, absolute levels of labor market inequality are now as high as they were prior to World War II (see Katz and Autor 1999). For the study of intergenerational mobility, these increases represent an unexpected reversal of the postwar trend toward greater equality of conditions. As such, they directly challenge a basic presupposition of the industrialization theories that predict a decline in inequality of conditions alongside a moderation in the total effects of social origins on occupational destinations. The presupposed causal variable – equality of conditions – has failed to exhibit its expected time trend, and as a result these theories appear less relevant as we move toward the study of social mobility in ostensibly postindustrial societies.[1]

Somewhat ironically, these increases in inequality of conditions evolved just as sociologists were developing their strongest case yet for the invariance of core social mobility patterns over time and across industrialized countries. At the conclusion of their definitive cross-national study, Erikson and Goldthorpe (1992: 367) wrote: "Over the years covered by our data, total

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mobility rates move in what would appear to be an essentially directionless fashion." Accordingly, the thesis of mobility-spawning industrialization was dismissed by Erikson and Goldthorpe in favor of a model of trendless fluctuation in mobility rates, a return in spirit to Pitirim Sorokin's (1927) conclusions in his pioneering study of social mobility.

It will take a decade or more to develop sufficiently deep explanations for the consequences of the recent growth in inequality of conditions, and research on changes in patterns of intergenerational mobility will be central to the endeavor (see Neckerman 2004). And herein lies the departure point of our study, one that is consistent with the closing appeal of Erikson and Goldthorpe (1992:396) that more effort be directed at evaluating and elaborating the "hypothesis that, within the class structures of industrial societies, inequality of opportunity will be the greater, the greater inequality of condition – as a derivative, that is, of the argument that members of more advantaged and powerful classes will seek to use their superior resources to preserve their own and their families' positions."

The data analyzed for the Erikson and Goldthorpe study were drawn from cross-sectional surveys between 1970 and 1978 (see Erikson and Goldthorpe 1992:50, Table 2.3). As with other classic studies of social mobility, their results captured intergenerational mobility patterns prior to the 1980s, and hence before the recent growth in inequality was evident. The primary question that motivates our chapter is therefore quite simple: Is there reason to expect a decline in intergenerational mobility that will be revealed in the decades to come, one that is attributable to the recent growth in inequality of family wealth and income? If so, it is reasonable to expect changes in patterns of educational attainment now for those birth cohorts whose relative life chances have been affected by recent changes in inequality of conditions.[2]

In this chapter, we will engage this primary question by investigating the educational attainment patterns of two recent cohorts of young adults, those between the ages of 17 and 21 in 1986 and in 1996. If we are to observe in the future substantial changes in mobility patterns that commenced with the increase in inequality in the 1980s and 1990s (perhaps using comparative retrospective data after 2010), then we should see changes in patterns of educational attainment for these two cohorts.

EDUCATIONAL ATTAINMENT AND THE COLLEGE ENROLLMENT DECISION

Social mobility research in the 1990s embraced a core implication of the results of Blau and Duncan (1967), seeking to model the educational attainment process as an intervening mechanism for intergenerational mobility (see also Sewell, Haller, and Portes 1969). Carrying the log-linear tradition into empirical work on education and invoking rational choice theory (see Breen and Goldthorpe 1997; Goldthorpe 1996; Raftery and Hout 1993), studies of educational attainment returned to the research frontier, but now more commonly with reference to continuation decisions for discrete educational transitions (Mare 1980, 1981). The collection of papers published in Shavit and Blossfeld (1993) reaffirmed the basic invariance of core mobility processes across countries, noting (with only a few exceptions) a robust pattern across national datasets of logit coefficients for the effects of social origins on progression through common educational transitions.[3]

In the United States, on which our empirical analysis will focus, a number of pointed debates emerged around specific questions on educational attainment, mostly without direct reference to the cross-national mobility literature. The customary practice of measuring family advantage with socioeconomic status (i.e., parents' education and labor force characteristics) was challenged by those who wished to focus more directly on the availability of resources. Dalton Conley (1999, 2001), for example, attempted to estimate the causal effect of family wealth on college entry (independent of effects for parental education, occupational prestige, and family income). He concluded that wealth effects are large, especially in proportion to the lack of attention that they were given in the extant literature.[4] Extending the work of Oliver and Shapiro (1995), he also stressed the power of wealth differentials to explain residual race differences in levels of educational attainment. This sociological attention to the effects of wealth on educational attainment was preceded in the economics literature by Mulligan (1997; see also citations therein and the subsequent work of Bowles and Gintis 2002). In the economics literature, however, wealth differences across families are interpreted more broadly, either as indicators of differential behavioral orientations correlated with savings behavior and lifetime success or reasons for families to pursue alternative strategies for human capital investment in their offspring.

While these arguments were being developed, labor economists also confronted an important policy-relevant issue arising from the growth in inequality. How would prospective college students respond in the 1990s to the substantial increase in labor market incentives to obtain college degrees? Most decision-theoretic models predict that gross rates of college entry should increase substantially in response to relative increases in the labor market payoff to college degrees. However, because incentive-based policies targeted at increasing college enrollments changed only modestly between the 1980s and 1990s in the United States (see Kane 1999a, 1999b), the same models predict that increases in inequality may have variable effects on different groups of prospective college students. In particular, increases in college enrollment should be smaller for prospective students from resource-poor families (or, at least, no larger), as these students' relative access to liquid funds to finance a college education has declined.

Mayer (2001) developed evidence for both hypotheses, using aggregate state-level data to identify the total effects of inequality on patterns of educational attainment. Ellwood and Kane (2000) offered similar results, using NELS and HS&B data (even though these data have rather coarse information on family income). But, Cameron and Heckman (1999) and Carneiro and Heckman (2002) challenged their interpretations, arguing that long-run deprivation is a much more important determinant of college entry and completion than short-run credit constraints, which is an argument consistent with the classic status attainment literature in sociology (e.g., Sewell et al. 1969 and Hauser, Tsai, and Sewell 1983; see Morgan 2005 for a review). Heckman and his colleagues argue that few students are credit-constrained (i.e., considerably less than 10 percent). Furthermore, the evidence that adolescents from high income families are more likely to have responded to the greater incentives to acquire college degrees is not necessarily supportive of the credit-constraint hypothesis (see Kosters 1999 and Heckman and Krueger 2003 for further debate).

No consensus has since emerged in the empirical literature in labor economics on the size or meaning of the effect of family income on college enrollment and completion, and understanding this effect seems necessary before an estimate of the incentive-effect of recent increasing returns can be constructed (see also Mayer 1997). As sociologists have long contemplated the consequences and meaning of long-run social disadvantage, perhaps at the cost of ignoring other factors that also explain patterns of social mobility, it is nonetheless somewhat heartening for sociology to see long-run deprivation effects at the core of some of the best recent work in the economics of education. To this convergent literature, we offer the following empirical analysis.

EMPIRICAL ANALYSIS

In order to investigate changes in patterns of educational attainment, we need a dataset that includes good measures of family income and wealth, spanning some portion of the time period in which inequality of income and wealth has increased. The standard datasets on which models of educational attainment are usually estimated are not ideal. The NLS-72, HS&B, and NELS data have large samples of high school students, but they have coarse family income measures and no direct wealth measures. The NLSY data have better income and wealth measures, but they represent cohorts of students who contemplated college enrollment primarily in the early to mid-1980s before much of the increase in inequality of conditions unfolded. Instead, we will analyze the 1986 and 1996 rounds of the Survey of Income and Program Participation (SIPP; see U.S. Department of Commerce, Bureau of the Census 2001), focusing on those age 17 to 21 in the spring of 1986 and 1996. We describe the construction of our analytic sample in a supplementary appendix (available by request and on the website for the volume: http://www.inequality.com/publications/symposia_books.shtml).

The primary strength of the SIPP data is the carefully and consistently defined income and wealth variables for two separate cohorts of students. And yet, as we detail in the supplementary appendix, the data are not without limitations, which may explain why we have been unable to find any other research reports using these data for the modeling of college entry patterns. The SIPP design, because of its focus on a nationally representative sample of households, yields a relatively small sample of college-age students. This limitation makes detailed subgroup comparisons nearly impossible because of sampling noise. Furthermore, there are substantial limitations in the available data for both college-age students and their parents. The SIPP provides no measures of cognitive skills, and hence we cannot enter the vigorous debate on the relationships between mental ability, measures of cognitive skill, and educational attainment (see Epstein and Winship, Chapter 10 of this volume).[5]

Finally, the sampling design rendered some 17- to 21-year-olds in the SIPP as members of households other than those of their parents, although the problem is not as severe as one might fear. Consider the procedures by which interviews are initiated for the SIPP. When a SIPP interviewer approaches a sampled household to develop a roster of all household members, individuals who are not currently living in the household but enrolled in college are retained on the household roster. Likewise, when a SIPP interviewer approaches a household of college students living together, students are eliminated from the household roster for that household if they could be listed as permanent members of their parents' households (which could thereby reduce the number of individuals in the sampled household to zero, thereby ending the interview). These two procedures ensure that the vast majority of enrolled students between the ages of 17 and 21 are listed as members of their parents' households for the SIPP.

Patterns are not as clean for nonstudents. In particular, 17- to 21-year-olds not enrolled in school and living in households without their parents are considered independent members of their own households for the SIPP. And thus, because we are interested primarily in the relationship between parental resources and college enrollments, we had to develop an imputation scheme for the parents' characteristics of these nondependent, nonenrolled, 17- to 21-year-olds, which we detail in the supplementary appendix. As we describe there, it is quite likely that our imputation scheme is too conservative, yielding estimated parental resources for nondependent, nonenrolled, 17- to 21-year-olds that are too close on average to the levels of resources typical of nonenrolled 17- to 21-year-olds still living with their parents. Mindful that our imputation scheme was necessarily limited, we carried on to analysis because we judged that this limitation, like others, does not vary meaningfully across the 1986 and 1996 surveys, thereby allowing for reliable analysis of the cohort comparisons that are our central focus.

Evolving Wealth and Income Differentials

We begin our empirical analysis by documenting the substantial increase in inequality between 1986 and 1996 using the SIPP data. Table 7.1 presents selected measures of the distribution of wealth and income for the entire SIPP sample, including (for now) all households with and without college-age students. Table 7.2 presents descriptions of the component SIPP variables used for the composite wealth and income variables analyzed for Table 7.1.

TABLE 7.1

Changes in Family Income and Household Wealth by Racial Group

		1986	1996	Change
Family Income (Monthly)				
Median	All	2,493.12	2,619.00	5.1%
	White	2,700.77	2,845.00	5.3%
	Black	1,522.27	1,791.00	17.6%
	B/W ratio	.56	.63	
Mean	All	3,189.91	3,545.10	11.1%
	White	3,384.50	3,775.03	11.5%
	Black	1,988.11	2,484.51	25.0%
	B/W ratio	.59	.66	
95th percentile	All	8,006.05	9,156.00	14.4%
	White	8,275.58	9,591.00	15.9%
	Black	5,167.76	6,722.00	30.1%
	B/W ratio	.62	.70	
Gini coefficient	All	.43	.45	
	White	.41	.45	
	Black	.44	.47	
Net worth				
Median	All	50,364.41	43,560.00	−13.5%
	White	64,286.30	61,632.00	−4.1%
	Black	4,290.43	5,667.00	32.1%
	B/W ratio	.07	.09	
Mean	All	111,786.30	147,575.10	32.0%
	White	126,808.00	175,300.20	38.0%
	Black	28,885.68	29,329.08	1.5%
	B/W ratio	.23	.17	
90th percentile	All	270,627.10	305,092.00	12.7%
	White	293,184.80	353,460.00	20.6%
	Black	87,733.77	83,500.00	−4.8%
	B/W ratio	.30	.24	
Gini coefficient	All	.63	.71	
	White	.61	.69	
	Black	.65	.65	

NOTES: Nominal dollars have been converted to 1996 dollars using the PCED deflator. The 1986 panel includes 30,577 respondents from 11,454 households; 5.6% of the heads of households did not provide answers to the wealth questions. The resulting N for this table equals 10,139 households. The 1996 panel includes 95,141 respondents from 36,730 households; 11.5% of the heads of households did not provide answers to the wealth questions. The resulting N for this table equals 32,519.

TABLE 7.2
Components of the Composite Income and Wealth Variables

Composite Variable	*Component Raw Variables*	*Level*
Monthly family income	Total family earned income Total family property income Total family means-tested cash transfers Total family "other" income	Family
Total net worth	Total wealth − total unsecured debt	Household
Total wealth	Home equity Net equity in vehicles Business equity Interest earning assets held at banking institutions Interest earning assets held at other institutions Equity in stock and mutual funds shares, real estate Other assets IRA and KEOGH accounts	Household
Total home equity	Market value of the resident property − total debt owed on home	Household

To enable direct cohort comparisons, nominal dollars in 1986 were converted to inflation-adjusted 1996 dollars using the personal consumption expenditures deflator of the Bureau of Labor Statistics.

The first panel of Table 7.1 presents patterns of income inequality for all SIPP households and then separately for those with white and black household heads. For economy of space, but also in recognition of the careful focus usually given to black-white differences in educational attainment (e.g., Conley 1999; Hallinan 2001), we do not present separate tabulations for Hispanics and Asians. However, these racial groups (and a catchall "other" category) are included in the full sample results and in our subsequent models of educational attainment.

As shown in Table 7.1, income inequality increased, which can be seen most clearly in an examination of comparable quantiles of the income distribution. For example, median family income increased by 5.1 percent while the 95th percentile of family income increased by a much larger 14.4 percent. Alongside this overall increase in income inequality, the black-white gap narrowed. Although black family income remained low, the black-to-white ratio for the mean, median, and 95th percentile of family income increased substantially between 1986 and 1996.

The second panel of Table 7.1 presents similar findings for the net worth of SIPP families. Consistent with other research using similar data (e.g., Wolff 1998), median household net worth fell slightly between the mid-1980s and mid-1990s while at the same time mean household net worth increased. This growth in the inequality of wealth is evident in the Gini coefficient for net worth, which increased from 0.63 to 0.71. Similar to race differences in family income, the black-white gap in wealth is large. But, the magnitude of the racial difference is much more dramatic for wealth, with the black-white ratio of median net worth less than 0.1 in both 1986 and 1996. Moreover, there were few signs of improvement in these differences. Whereas the black-to-white ratio for the median of net worth increased from 0.07 to 0.09, the same ratio for the mean of net worth decreased from 0.23 to 0.17. In tandem, the growth in wealth inequality disproportionately benefited whites relative to blacks.

Table 7.3 presents the same measures of wealth and income as Table 7.1, but now only for families with young adults in our restricted college-entry sample. For these results, we drop more than 85 percent of the SIPP sample and then recalculate the same measures of family and household resources. The general pattern matches the results for the full sample, as reported earlier in Table 7.1. To the extent that resource differentials of wealth and income represent the crucial dimensions of the inequality of conditions that are relevant for entry into postsecondary education, we conclude that a comparison of the 1986 and 1996 SIPP panels is well suited to an examination of changes in the relationship between inequality of conditions and educational attainment.

Income and Wealth as Predictors of College Enrollment

To test for variation in the associations between family resources and college enrollment, many modeling strategies can be adopted. Before directly examining college enrollment rates for separate social classes, in Tables 7.4 and 7.5 we present coefficients from five variants of a basic specification of resource and demographic variables. For these models, the probability of college enrollment in November of each year is predicted using a logit model, with adjustments for associations with gender, race, age, and prior enrollment status in March. In models I through V, alternative combinations of family resource variables are specified.[6]

TABLE 7.3

Changes in Family Income and Household Wealth by Racial Group, Restricted to Families with College Eligible Children Between the Ages of 17 and 21

		1986	*1996*	*Change*
Family Income (Monthly)				
Median	All	3,411.72	3,519.00	3%
	White	3,926.02	4,151.00	6%
	Black	2,013.20	2,209.00	10%
	B/W ratio	.51	.53	
Mean	All	4,130.13	4,408.45	7%
	White	4,564.83	5,040.27	10%
	Black	2,404.01	2,973.05	24%
	B/W ratio	.53	.59	
90th percentile	All	7,660.89	8,464.00	10%
	White	8,102.00	9,130.00	13%
	Black	4,165.00	6,021.00	45%
	B/W ratio	.58	.65	
Gini coefficient	All	.40	.44	
	White	.37	.41	
	Black	.45	.45	
Net worth				
Median	All	55,294.28	36,541,00	−34%
	White	74,049.78	62,400.00	−15%
	Black	5,088.01	5,847.00	15%
	B/W ratio	.07	.09	
Mean	All	116,996.70	129,899.60	11%
	White	142,159.70	170,919.80	20%
	Black	27,933.36	31,087.63	11%
	B/W ratio	.20	.18	
90th percentile	All	282,563.30	274,288.00	3%
	White	320,123.80	339,375.00	6%
	Black	89,183.10	85,039.50	−5%
	B/W ratio	.28	.25	
Gini coefficient	All	.61	.71	
	White	.59	.68	
	Black	.61	.69	

NOTES: Nominal dollars have been converted to 1996 dollars using the PCED deflator.

TABLE 7.4

Estimated Logit Coefficients for the Effects of Income and Wealth on College Enrollment in November of 1986 and 1996 for SIPP Respondents Ages 17 to 21

	I		II		III	
	1986	*1996*	*1986*	*1996*	*1986*	*1996*
Female	.214	.248	.260	.233	.276	.239
	(.136)	(.086)	(.140)	(.086)	(.139)	(.086)
Black	−.658	−.109	−.402	−.054	−.538	−.132
	(.224)	(.130)	(.228)	(.132)	(.224)	(.130)
Hispanic	−.309	−.413	−.089	−.379	−.230	−.476
	(.273)	(.137)	(.273)	(.138)	(.271)	(.137)
Asian	−.031	.608	−.005	.649	.090	.656
	(.429)	(.220)	(.427)	(.222)	(.439)	(.225)
Income	.121	.141				
	(.031)	(.018)				
Zero income	.889	<−.001				
	(.728)	(.539)				
Net worth			.039	.035		
			(.007)	(.004)		
Zero net worth			−.968	−.567		
			(.370)	(.290)		
Negative net worth			−.888	.325		
			(.286)	(.152)		
Home equity					.087	.075
					(.016)	(.011)
Zero home equity					−.132	.014
					(.202)	(.125)
Negative home equity					−.327	1.160
					(.434)	(.291)
Covariates:						
Other race	✓	✓	✓	✓	✓	✓
Age	✓	✓	✓	✓	✓	✓
March enrollment	✓	✓	✓	✓	✓	✓
N	1,900	4,994	1,900	4,994	1,900	4,994

TABLE 7.5

Estimated Logit Coefficients for the Effects of Income and Wealth on College Enrollment in November of 1986 and 1996 for SIPP Respondents Ages 17 to 21 with Covariates for Parental Education

	IV		V	
	1986	*1996*	*1986*	*1996*
Female	.261	.255	.274	.266
	(.141)	(.088)	(.141)	(.088)
Black	−.262	.079	−.377	.073
	(.237)	(.136)	(.234)	(.135)
Hispanic	.175	.202	.032	.193
	(.285)	(.152)	(.284)	(.153)
Asian	−.022	.815	.067	.845
	(.427)	(.227)	(.441)	(.230)
Father's education	.040	.126	.033	.128
	(.033)	(.028)	(.033)	(.028)
Mother's education	.107	.103	.109	.109
	(.040)	(.029)	(.040)	(.029)
Income	.009	.049	.017	.046
	(.036)	(.020)	(.035)	(.020)
Zero income	.034	.043	.271	.073
	(.713)	(.552)	(.699)	(.549)
Net worth	.032	.018		
	(.008)	(.005)		
Zero net worth	−1.069	−.346		
	(.375)	(.295)		
Negative net worth	−.872	.225		
	(.290)	(.156)		
Home equity			.076	.051
			(.016)	(.011)
Zero home equity			−.124	.048
			(.205)	(.128)
Negative home equity			−.205	.852
			(.430)	(.302)
Covariates:				
Other race	✓	✓	✓	✓
Age	✓	✓	✓	✓
March enrollment	✓	✓	✓	✓
N	1,900	4,994	1,900	4,994

For model I, which is estimated separately by cohort, college enrollment is predicted from family income and a dummy variable for zero income (and the other covariates listed earlier).[7] For both 1986 and 1996, the logit coefficients are similar. In a pooled model, the increase of 0.020 has a standard error of 0.035, a ratio that suggests the cohort difference in coefficients is consistent with the fluctuation produced by sampling error. In magnitude, the coefficients imply that an increase of $500 in monthly family income is associated with an increased probability of enrolling in college of between 0.006 and 0.010 (depending on the values at which other variables are set). Were we prepared to regard this coefficient as a warranted causal effect (see the discussion section for an explanation of why we will not), this small but substantial association would suggest that giving the average family $6,000 per year in family income would increase the college enrollment rate among their adolescents by an additional two-thirds to 1 full percent.

The size of this association is somewhat artificial, because the estimate is conditional on the prior spring enrollment coefficients parameterized with four dummy variables for (1) not enrolled in school, (2) enrolled in college, (3) enrolled as a high school junior, and (4) enrolled as a high school freshman or sophomore (and thereby leaving high school seniors as the reference category). Models removing these dummy variables for types of spring enrollment yield larger logit coefficients for family income (i.e., from 0.121 and 0.141 to 0.161 and 0.194, respectively), but no pattern of alternative cohort differences is revealed in such models.

Other variants of this model also reproduced our basic claim of no-cohort differences. For example, when we ignored shifts in the distribution of family income, we obtained similar results. The difference in the odds of enrolling in college for those in the highest quintile in family income in comparison with those in the lowest quintile was virtually the same for both cohorts. Models using the natural logarithm of income yielded coefficients of 0.275 and 0.239.[8]

Model II substitutes household net worth for family income and model III substitutes home equity for family income, both right-censored at the 95th percentile. For model II, the cohort-specific coefficients for net worth are within sampling error of each other. And, for model III, the analogous coefficients for home equity are also similar and within sampling error.[9]

Models IV and V present slightly more elaborate specifications, in order to demonstrate that no cohort differences are masked by movements in other family resource variables. For both models, measures of parental education and family income are included along with net worth for model IV and home equity for model V. The magnitudes of the coefficients for all dimensions of resources decline, which is entirely unsurprising given the positive correlations between them and with parental education. But all of the between-cohort fluctuations in coefficients remain erratic, and small enough to be reasonably attributed to sampling error. In sum, based on the results reported in Tables 7.4 and 7.5, we conclude that (1) family resources exhibit substantial associations with college enrollment for both cohorts and that (2) the relatively stable but erratic pattern of coefficients across specifications suggests that little has changed between cohorts.[10]

Social Class as a Predictor of College Enrollment

Against this backdrop of relatively constant (though noisy) associations between family resources and college enrollment, we now ask whether social class of origin predicts college enrollment in the same pattern for both cohorts. To the extent that between-class inequality of conditions has increased, and yet the predictive power of direct measures of resources has not changed, then one might expect to see a larger social class advantage in 1996 for those at the top of the class hierarchy.

Table 7.6 presents four sets of logit models, predicting college enrollment from dummy variables for a variant of the class schema developed for Erikson and Goldthorpe (1992) and its predecessors. The reference category for the social class dummy variables is class VIIa, which is comprised of semiskilled and unskilled workers not employed in agriculture. Class effects are parameterized with reference to this class, which has the lowest mean levels of resources in income and wealth. Classes I and II are comprised primarily of higher-level and lower-level professional and managerial workers, respectively. Class III represents routine, nonmanual workers, and class IV represents self-employed small proprietors and landholding farmers. Class V primarily consists of the supervisors of manual workers and some higher-grade technicians, while class VI is composed of skilled manual workers. Finally, class VIIb is the smallest of the eight social classes, as it includes only agricultural laborers and others in primary production who are not proprietors.

The point estimates for social class in model VI suggest that adolescents whose parents are members of class VIIa are the least likely to enroll in college between the ages of 17 and 21 (with the possible exception of class VIIb

TABLE 7.6

Estimated Logit Coefficients for the Effects of Social Class on College Enrollment in November of 1986 and 1996 for SIPP Respondents Ages 17 to 21

	VI		VII		VIII		IX	
	1986	*1996*	*1986*	*1996*	*1986*	*1996*	*1986*	*1996*
Female	.294	.240	.283	.259	.282	.241	.277	.264
	(.165)	(.101)	(.166)	(.102)	(.167)	(.103)	(.170)	(.102)
Black	−.573	−.052	−.446	.069	−.468	.029	−.276	.143
	(.279)	(.153)	(.284)	(.156)	(.284)	(.157)	(.290)	(.159)
Hispanic	−.296	−.287	−.201	−.195	−.141	.158	.014	−.166
	(.360)	(.169)	(.364)	(.170)	(.371)	(.180)	(.369)	(.170)
Asian	−.231	.704	−.323	.719	−.302	.811	−.369	.807
	(.478)	(.273)	(.491)	(.273)	(.486)	(.277)	(.492)	(.275)
Class I	.876	1.353	.651	1.100	.490	.787	.625	1.144
	(.262)	(.164)	(.279)	(.173)	(.293)	(.184)	(.272)	(.168)
Class II	.815	.795	.669	.639	.462	.325	.731	.670
	(.270)	(.150)	(.276)	(.155)	(.290)	(.165)	(.277)	(.153)
Class III	.318	.486	.254	.471	.176	.290	.292	.426
	(.259)	(.159)	(.262)	(.160)	(.265)	(.163)	(.269)	(.161)

(*continued*)

TABLE 7.6
(*continued*)

	VI		VII		VIII		IX	
	1986	*1996*	*1986*	*1996*	*1986*	*1996*	*1986*	*1996*
Class IV	1.231	.812	1.109	.626	.900	.633	.950	.517
	(.611)	(.774)	(.626)	(.801)	(.646)	(.790)	(.619)	(.786)
Class V	.092	.274	−.032	.153	−.008	.158	−.033	.216
	(.357)	(.236)	(.363)	(.238)	(.360)	(.240)	(.362)	(.237)
Class VI	.243	.309	.162	.228	.240	.263	.141	.285
	(.287)	(.177)	(.291)	(.178)	(.290)	(.177)	(.295)	(.177)
Class VIIa (reference)								
Class VIIIb	−.223	.616	−.365	.547	−.195	.640	−.261	.506
	(.991)	(.435)	(1.016)	(.441)	(.995)	(.463)	(1.027)	(.444)
Covariates:								
Other race	✓	✓	✓	✓	✓	✓	✓	✓
Age	✓	✓	✓	✓	✓	✓	✓	✓
March enrollment	✓	✓	✓	✓	✓	✓	✓	✓
Income			✓	✓				
Parental education					✓	✓		
Net worth							✓	✓
N	1,331	3,563	1,331	3,563	1,331	3,563	1,331	3,563

in 1986). All other classes have positive logit coefficients, with those from classes I, II, and IV somewhat more likely than those from classes III, V, and VI to enroll in college. These results are entirely consistent with the literature, demonstrating (once again) the predictive power of this sort of class schema.

Moreover, for model VI, the only noticeable change between cohorts is the increase in the relative odds of college enrollment for those from class I. The increase from 0.876 to 1.353 is substantial, and in a pooled model for both cohorts the difference of 0.478 has a standard error of 0.309. Although not statistically significant by conventional standards, the increase is consistent with our prior beliefs, which are grounded in the received wisdom about the increasing resources of class I and the importance of resources in explaining college enrollment. Thus, we are inclined to view the increase as genuine, even though we recognize that substantial caution is in order. The increase suggests that prospective students from the most advantaged social origins were more likely to be enrolled in college in 1996 than in 1986. This result, when paired with the apparent stability of the associations between resources and college entry, suggests that the increase in inequality of conditions between classes may be responsible for the increased odds of college entry for class I.

We evaluate this inductive conjecture in the last three models of Table 7.6, where parental education, income, and net worth are added to the model successively (and exclusively). Our conjecture that increased resources can account for the increased odds of class I is, however, disconfirmed by these results. Although adjusting for each of these additional measures of family advantage attenuates social class as a predictor of college enrollment, the increase in the odds of college enrollment for class I is only slightly altered in these models. To the extent that there has been a relative increase in the enrollment rate for those from the most advantaged social class origins, our results suggest that (1) this increased enrollment rate is attributable to a change between 1986 and 1996 other than increasing relative resources or (2) our specification of resource effects does not capture the relevant levers relating social mobility to inequality of conditions. We reserve discussion of the latter for the end of the chapter.

Summary of Empirical Conclusions

Our results from Tables 7.4 and 7.5 reveal little or no change in the associations between family resources and college enrollment. Given this apparent invariance across cohorts, it would be reasonable to expect that the particular distributional shift in resources revealed in Tables 7.1 and 7.3 (i.e., where the increases in inequality represent a relative redistribution of resources away from those in the middle toward those in the tails) to generate greater inequality of college enrollment. Adolescents from the top of the resource distribution might be expected to attend college more frequently in 1996 than in 1986, as their relative resources increased while the apparent effect of each increment of resources remained the same.

Our results from Table 7.6 are consistent with this baseline expectation. We found a small increase in the relative odds of college entry for those from the most advantaged social class I, comprised primarily of the children of professionals and higher-level managers. However, we could not attribute the relatively large logit coefficient predicting college enrollment for class I in 1996 to any of the resource or family background variables, thereby undermining the main rationale for the expectation that a greater relative enrollment rate would emerge at the top of the class hierarchy in 1996. This inconsistency represents a puzzle that awaits resolution, as we will discuss later. It could, for example, reflect a greater relative recognition among adolescents and parents from class I that college is ever more essential for labor market success.

In total, we have not found any evidence that recent increases in inequality will generate dramatic changes in patterns of social mobility. At best, quite modest changes are unfolding, with the advantages of class I escalating slightly for unknown reasons. Two qualifications to these conclusions are in order.

First, this judgment of relatively little change in the social mobility arising from inequality of conditions is based on the assumption that future changes in social mobility patterns would necessarily be revealed to some degree in patterns of educational attainment in the 1980s and 1990s (and, furthermore, rather narrowly in basic college enrollment patterns, as opposed to college graduation, and so on). We are well aware that change could result from other mechanisms relating social origins to occupational destinations, and if so, we may nonetheless see a change in patterns of social mobility that is a consequence of changes in inequality of conditions. But, were this to be the case, the consensus position of the literature – that inequality of conditions regulates levels of social mobility primarily via selection and allocation mechanisms of the educational system – would be open to revision. We doubt that the literature would be proven so far off the mark, even though we agree that investigation of change in other mechanisms is surely in order.

Second, total mobility rates could change in the future for reasons entirely unrelated to the recent increase in inequality of conditions. In particular, entry rates into class I may increase substantially in the United States in the future if the narrative of the evolving global economy is substantiated. But this increase would then be duly labeled as structural mobility, rather than that which would be attributed directly to the mechanisms regulated by inequality of conditions. Uncovering such a pattern of structural mobility would be an important contribution to the empirical literature, but it would not have much relevance for the direct question we have addressed in our empirical analysis in this chapter.

DISCUSSION

Within the social sciences, standards by which coefficients of statistical models are judged relevant for theoretical propositions and policy prescriptions are in the process of revision. Our chapter is rather old-fashioned in this regard, as we merely attempt to assess whether or not the predicted relationships suggested by theoretical propositions are realized in the available data (using the accepted model specifications that prevail in sociological research on social mobility). And yet, there are two distinct types of inquiry embedded within our analysis: (1) an assessment of the effects of family resources on college enrollment and (2) an assessment of the consequences of changes in estimated associations for subsequent patterns of social mobility and the industrialization theories that have been constructed to explain them. Whereas the first type of inquiry is carefully defined and precise answers should be expected, the latter is more deeply a matter of judgment, given the limitations of available data, the potential for entirely unforeseeable shocks to the economy, and the rather informal nature of the predictions set forth in the social mobility literature from the 1950s through the 1980s. In this section, we first draw this distinction more clearly, indicating where in the methodological and epistemological terrain we would wish our study to be placed. We then conclude by laying out a more encompassing set of issues that relate our findings to some of the core themes of this volume.

Causal Inference for the Effects of Family Resources

Focus on a single dimension of family resources – such as family income – and suppose that (1) our primary subject of investigation is the causal effect of family income on college enrollment and (2) our secondary subject of investigation is whether this causal effect has changed between 1986 and 1996. In this scenario, we are interested in more than just the degree to which observed family income predicts the observed odds of college enrollment, as we are interested in the underlying causal effect. And, our position is that one cannot be interested in this causal effect without wishing to know the answer to counterfactual questions, such as "What would the college enrollment rate of students with family income equal to *X* have been if instead these same students had family income equal to *Y*?" Indeed, ideally we would wish to know the equation:

$$\Pr\,(ENROLL)_i = f_i\,(FAMINC) + e_i \tag{1}$$

where *FAMINC* is a deterministic "what if" family income, $f_i(\cdot)$ is an individually varying function of potential family income, and the final term is a random shock.[11]

Our logit coefficients for family income, such as for model I in Table 7.4, do not reveal very much about the fundamental causal relationship that Equation 1 represents. Thus, we make no claims that our logit coefficients are informative about the true counterfactual causal effects of family resources on college enrollment.[12] In fact, as causal effect estimates, the literature suggests that our coefficients for family resources are almost certainly too large because of the absence of covariates such as cognitive skill (see Heckman and Krueger 2003 along with Cunha, Heckman, and Navarro, Chapter 11 of this volume and also Epstein and Winship, Chapter 10 of this volume). Given our recognition of this limitation of our models, why were they worth estimating?

Before answering this question, we should make clear that we do not share the position of others that counterfactual causality is an improper conceptual foundation for the methodology of sociological research, even if one's results fall far short of its standards.[13] We most certainly should attempt to generate results that can warrant counterfactual conditional statements (i.e., if we wish our causal models to be portable across time and space to at least some degree, if we wish our conclusions to have some relevance to the design of policy interventions, and so forth). Accordingly, we would very much encourage efforts to find natural experiments that produce more direct information about Equation 7.1, including the pursuit of the sort of social experiments advocated by Hanushek (2003). And, net of the causal effect of income on college enrollment, it would surely be fruitful to learn whether wealth does indeed have a meaningful causal effect on educational attainment. Part and parcel of pursuing such a conclusion would be the determination of whether wealth has as an effect by eliminating credit constraints on college entry or instead by enabling the purchase of an environment early in a child's life that is conducive to learning and more general features of child development.[14]

Given that we cannot warrant strong counterfactual conditionals, why then are our models useful? Note that the existence of meaningful omitted variables such as cognitive skill does not necessarily invalidate our "no-change in the association" conclusions for family resources. In order for the absence of a measure of cognitive skills to have suppressed a genuine cohort difference in the effects of social class or family resources, the relationship between these variables and the cognitive skills of adolescents would have to have changed between 1986 and 1996. Although some scholars have claimed that there are trends in these relationships, none of these claims has been substantiated in the literature (see Devlin, Fienberg, Resnick, and Roeder 1997). Thus, even though we are willing to concede that we cannot offer reasonable estimates of causal effects in the potential outcome framework for income, wealth, or social class, we still regard our models as worthwhile, for they succeed in addressing some of the key change-over-time propositions at the heart of sociological writing on social mobility, as we discuss next.

Implications for the Mobility and Education Literature

As discussed in the introduction, in the 1980s and 1990s the simple narrative of equality-spawning industrial development fell apart. Along with the evolution of an economy often characterized as postindustrial, we have seen an increase in inequality of conditions. Our contribution to this volume is motivated by our prediction that a new wave of studies will be forthcoming on the connections between the logic of postindustrialism and rates of social mobility. We have offered results bearing on a classic proposition of the social mobility literature—inequality of conditions regulates the level of social mobility in a society. And we have used the customary model specifications employed in sociological research on mobility. Our results lead us to the conclusion that post-1980 trends in educational attainment are less supportive of this classic proposition than one might have expected.

Our findings are also relevant (in an analogous way) to the conjecture of American exceptionalism in mobility patterns. Commencing with Tocqueville, and carried forward by Parsons, Duncan, Treiman, and others, many scholars have put forth the conjecture that rates of social mobility are comparatively high in the United States because of (1) its unique historical legacy as a new state without a titled nobility, (2) its supposed status as the vanguard nation of industrialization, and (3) its greater reliance on universalistic criteria for educational and occupational placements. An equally impressive group of scholars has sought to disconfirm the same conjecture, most prominently Lipset and Bendix (1959) and Erikson and Goldthorpe (1985).

The conjecture of American exceptionalism survives because a modest amount of empirical evidence seems to support it. Indeed, Erikson and Goldthorpe (1992) demonstrate the endurance of the conjecture. Their primary goal was to parsimoniously account for the pattern of intergenerational mobility characteristic of the industrialized nations in Europe, which they fulfilled admirably with their model of core social fluidity for nine European nations. When separate attention was then given to the United States, they concede that some evidence for higher rates of social mobility in the United States was found (even though it is less, in their estimation, than an exceptional amount).[15]

How do our results relate to the thesis of American exceptionalism? Erikson and Goldthorpe (1992:369) noted, with reference to Treiman and Yip (1989), that industrialization theories had identified declining inequality of conditions as the primary proximate cause of increasing rates of mobility in countries such as the United States. If we had seen the influence of social origins strengthening between the two cohorts and could have attributed this increase to widening resource differentials, then this result would have been consistent with a particular narrative of American exceptionalism, wherein the United States is seen as the vanguard nation of industrialization and, by virtue of this status, has levels of social mobility that respond to changes in levels of inequality of conditions more immediately and consequentially than other nations. That is, one could have argued that the moderation of differences in initial conditions generated openness of entry at the top of the occupational structure in the United States between 1950 and 1980, but then that the accentuation of inequality has led to (or will lead to in the next two decades) a general decline in exchange mobility. But, we did not find this pattern, and as a result we are left with alternative implications for the thesis of American exceptionalism. Our findings may suggest that the discounted ideas of historical contingency and cultural differences will yet be revealed as more than just ideas that serve the "cause of national mythologies" (Erikson and Goldthorpe 1992: 372).[16]

Finally, while we await the discovery of natural experiments that can help tease out the genuine causal effects of family resources (and government transfers to meet resource shortfalls), and as we await descriptive patterns relevant to a new engagement of postindustrialization theories of mobility and the thesis of American exceptionalism, we should, in the meantime, attempt to build a better model of educational attainment. As discussed in the introduction, a promising mixture of perspectives is emerging in sociology and economics, one that seeks to link these two traditions together. For example, in sociology, the first author (see Morgan 2002, 2005) has used this convergent literature to develop a stochastic decision tree model for determining intermediate levels of preparatory commitment and then the educational attainment that follows from it. If this type of modeling more effectively captures the genuine process of educational attainment, then the increased odds of enrollment for those from class I would reflect (1) the more certain recognition of those from class I that higher education pays off and (2) that this differential recognition causes more adolescents from class I to prepare themselves for college entry by engaging more deeply in the pursuit of academic success while still in high school.

New theoretical work will only have a substantial payoff if it generates a fruitful agenda for new empirical research. And, in this regard, there already has been some progress by other sociologists, as the results of Breen and Yaish in this volume build directly on the work of Breen and Goldthorpe (1997) and then Breen (1999). The next goal for those following in this tradition (that is, if our finding of increased relative odds of college enrollment for class I begins to appear elsewhere) would be to show that students from class I are responding differently to changing incentives as well, with perhaps a deeper change in the decision rules on which their continuation choices are based.

Notes

1. As eloquently discussed by Erikson and Goldthorpe (1992), these "logic of industrialism" theories were championed by Talcott Parsons (although they may have originated in the convergence theory of Clark Kerr) and were then further developed by Blau and Duncan (1967) and Treiman (1970). For the last of these, see Treiman's (1970) proposition I.A.6 of increases in equality of income, which he argues is supported by evidence available at the time of his writing. Then, see his predictions I.B.1 through I.B.7 on increases in mobility, which he argues follow from proposition I.A.6 (and other propositions). As indicated by Erikson and Goldthorpe (1992) with reference to Treiman and Yip (1989), the link between increasing equality of conditions and slightly increasing mobility remains the most robust piece of the thesis.

2. As we discuss later, this expectation rests on assumptions about the existence of a relatively invariant model for educational attainment and the continued importance of educational attainment for mobility patterns, following on the classic argument of Blau and Duncan (1967) and as elaborated and qualified by Raftery and Hout (1993).

3. Cameron and Heckman (1998) challenge the interpretation of these coefficients across transitions but do not challenge the claim that the same pattern prevails across countries. If the common pattern is a result of modeling assumptions encoded in sequential logits, then some of the similarity may reflect common methodology across national studies rather than common substantive findings. The jury is still out on this larger issue.

4. This is not necessarily a fair interpretation of the evidence. Conley (1999), for example, gives rather little consideration to the impact of basic multicollinearity. He relies on statistical significance as his measure of "strength" and "predictive importance" but ignores how hard it is to precisely estimate the unique contribution of family income to college enrollment and completion in the presence of other variables for parental education, occupational prestige, and levels of wealth (see especially Conley 1999, Tables 3.2 and A3.2). See also Orr (2003) for a similar set of interpretations.

5. Even constructing a college entry measure was challenging. As discussed later (and in the supplementary appendix), we pooled those aged 17 to 21 and then estimated the logit models conditional on dummies for age and spring enrollment status. Both sets of dummies partial out common age and grade-of-origin effects across cohorts, enabling comparisons of the adjusted coefficients. The modeling strategy represents our attempt to get as close as possible to the usual practice of estimating college entry rates for graduating high school seniors. Of course, our coefficients represent unknowable mixtures of enrollment probabilities across our mixed-age samples, further conditioned on prior spring enrollments. Our assumption is that these unknowable mixtures are invariant across the two cohorts, thereby enabling meaningful comparisons of coefficients across cohorts.

6. As we hint throughout, many other variants on this basic specification were estimated, and no substantively important differences were detected. In particular, we used alternative specifications of family and household resources (both quantile dummy specifications and monotonic transformations). We estimated the models ignoring March enrollment status. And, we estimated the models on the subset of respondents (approximately 86 percent of respondents) who were still dependent on their parents (i.e., excluding those "living alone," who by SIPP design were, by definition, independent adults). Results for these alternative models are available from the authors, but we assure the reader that they differ little from what we present in this chapter. See also the details in the appendix.

7. As is shown in the appendix, and discussed later in the text, we chose a specification of resource variables that matches the sort of censored resource variables available in other datasets on which models of educational attainment are usually estimated (such as the NELS or HS&B). We therefore censored the original income and wealth variables, coding all those above the 95th percentile as if they were at the 95th percentile. For monthly income, those above the 95th percentiles were recoded to values of 9,526 and 10,698 for 1986 and 1996, respectively. For total net worth, the equivalent values are 392,808 and 425,926, and for home equity they are 198,019 and 170,000. This specification also makes sense to us for deeper theoretical reasons: (1) With regard to a coarse college enrollment variable, we do not expect differences between the very rich and the very, very rich; (2) other transformation of resources which would shrink the very, very rich toward the very rich (such as the log of resources) transform the entire resource scale, which seems inappropriate to us. Nonetheless, we did estimate these alternative models, and the same non-trends were evident. For the income variable in model I, the numbers in the table – 0.121 and 0.141 would have been 0.098 and 0.069 for the original income variable and 0.275 and 0.249 for the log of the original income variable. For both of these alternative specifications, the cohort difference was nonsignificant.

8. The zero income coefficients appear somewhat puzzling, but further inspection convinced us that they are not. The income variable is monthly income from the prior March, when we measured spring enrollment status at our chosen baseline reference point for the study. Only 1.1 percent and 0.8 percent of respondents were from families with zero income in March of 1986 and 1996, respectively. Thus, this is a small group of unusual respondents, susceptible to both model specification issues and sampling error. Why was the zero-income coefficient larger in 1986? The zero-income families had relatively high wealth in 1986. The mean net worth of the students who were from zero-income families in 1986 was \$194,161, in contrast to \$107,850 for all other families. In 1996, the mean net worth of zero-income families was only \$21,675 in comparison to \$103,353 for all other families. We therefore expect that some of the zero-income families are a type of temporary zero-income families, and that this was more likely to be the case in 1986. We suspect that many of these families have heads of household who were between jobs but who also had levels of family resources that allowed them to remain in active job

search mode rather than accepting a job at a rather low wage or taking public assistance. This was perhaps more likely to be the case in 1986, either because of sampling error or because of the higher rate of unemployment in 1986 (that is, accordingly to the Bureau of Labor Statistics, 7.2 percent versus 5.5 percent in March 1986 versus March 1996).

9. Again, the point estimates for the zero wealth and negative wealth estimates deserve some attention. There is a literature on how negative net worth is a misleading indicator of wealth (see Kennickell 2003), and our results confirm that some of the families with negative net worth are generally quite resource rich, to the extent that banks are willing to lend them money against their assets because of their relatively high income. But, the group is nonetheless rather heterogeneous, and in view of this heterogeneity, it is not surprising that the point estimates jump around from model to model.

10. Nonetheless, it should be recognized that the sample sizes of the SIPP are relatively small. Rejecting a no-change null hypothesis based on statistical significance tests would require a fairly substantial change between cohorts. Accordingly, it is possible that the small increase in the point estimate for the effect of family income in model I, when coupled with prior knowledge of the results of Ellwood and Kane (2000), would be regarded by a Bayesian as confirmatory evidence that the association between family income and college entry has increased between the 1980s and the 1990s. Of course, an alternative Bayesian could come to the opposite conclusion, after taking note of the slight decline in point estimates for family income when it is specified in logarithmic form, as reported in the main text.

11. See Angrist and Krueger (1999) for a similar setup. See Winship and Morgan (1999) for a sociological account of the counterfactual model of causality. See Sobel (1998) for methodological discussion that prosecutes the literature in social stratification.

12. Indeed, we agree with Sobel (2004:418) that "virtually all of the so-called 'effects' estimated in the [sociological] literature over the past 35 to 40 years are at best fancy associations that have little to do with causation." We will even admit that our coefficients are not fancy.

13. See Goldthorpe (2000: ch. 7) for a cogent presentation of an alternative position.

14. In judging the wisdom of recommending research on natural disasters to find wealth destruction natural experiments, we ultimately decided not to do so. Aside from having to model insurance effects and so forth, such efforts would have to partial out all of the consequences of disasters other than wealth destruction (see Norris et al. 2002), and this seems beyond our current capacities.

15. Erikson and Goldthorpe (1992) offer a somewhat different interpretation than we have just claimed. In their main text, the departure of the United States from the core social fluidity model is downplayed, as attention is focused instead on the comparable global measures of model fit. But tucked within footnote 7 (p. 318)

are hints of the substantial differences. They reach two alternative conclusions (p. 320): "One is that some further support is here provided for Blau's and Duncan's view that the American mobility regime is distinctive at least in the greater openness of more advantaged class positions that it affords; the other is that the deviation from core fluidity that our model captures reflects not so much American social reality as the difficulties we faced in recoding the American data. Our own preference is strongly for the second of these interpretations."

16. The economics literature has also begun to engage the thesis of American exceptionalism in two ways. Bowels and Gintis (2002) further challenge the notion that the United States is properly described as a land of opportunity, and Corak and Heisz (1999) and Solon (2002) note that countries such as Canada and Sweden may have higher rates of intergenerational income mobility, which they surmise may reflect alternative levels of subsidies for human capital accumulation.

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