

The Dynamics of Educational Attainment for Black, Hispanic, and White Males

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This paper estimates a dynamic model of schooling attainment to investigate the sources of racial and ethnic disparity in college attendance. Parental income in the child's adolescent years is a strong predictor of this disparity. This is widely interpreted to mean that credit constraints facing families during the college-going years are important. Using NLSY data, we find that it is the long-run factors associated with parental background and family environment, and not credit constraints facing prospective students in the college-going years, that account for most of the racial-ethnic college-going differential. Policies aimed at improving these long-term family and environmental factors are more likely to be successful in eliminating college attendance differentials than short-term tuition reduction and family income supplement policies aimed at families with college age children.

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This paper examines the sources of disparity in college attendance between whites and minorities. In a purely statistical sense, disparities in family income largely account for differentials in measured schooling attainment. Yet the interpretation of this statistical relationship is ambiguous. It can arise because of credit constraints operating on families at the time students are deciding to go to school. Current educational policy is predicated on this view. An alternative interpretation is that long-term factors, including long-term levels of family income, determine the abilities required to benefit from college education. The empirical evidence presented in this paper supports the alternative interpretation and suggests that short-term tuition and aid programs targeted toward low-income families during their children's college-going years are unlikely to be effective in eliminating minority-majority differentials in schooling attainment. Longer-term policies that improve scholastic ability are far more likely to alleviate measured educational differentials.

The time series of college enrollment of males classified by racial and ethnic status is presented in figure 1.¹ White males responded with a lag to the rise in the return to college education that began in the early 1980s. Black males responded later, and Hispanic males did not respond at all. The conventional explanation for the disparity in educational attainment and the differential response to the increased return to college education is disparity in the family resources required to finance a college education (see, e.g., Hauser 1993*b*; Kane 1994). Figure 2 is consistent with this claim. The educational response to the rising return to education was the most rapid and the largest for adolescent males whose families are in the top half of the family income distribution. The response of children from families at the bottom of the family income distribution was substantially delayed. Minority children are concentrated in families near the bottom of the overall family income distribution, where real earnings have declined over the past 20 years. The lower real earnings of minority parents coupled with the rise in real tuition costs would seem to suggest that short-term liquidity constraints on educational finance can explain figure 1. Policies founded on this interpretation advocate tuition offsets and income supplements to stim-

¹ The Hispanic-white differential in high school completion has been stable over time. The black-white differential has narrowed, but mostly because of the growing use of general equivalency diploma (GED) certification by blacks (see Cameron and Heckman 1993*b*; Cameron 1996; Heckman, Hsee, and Rubinstein 1999).

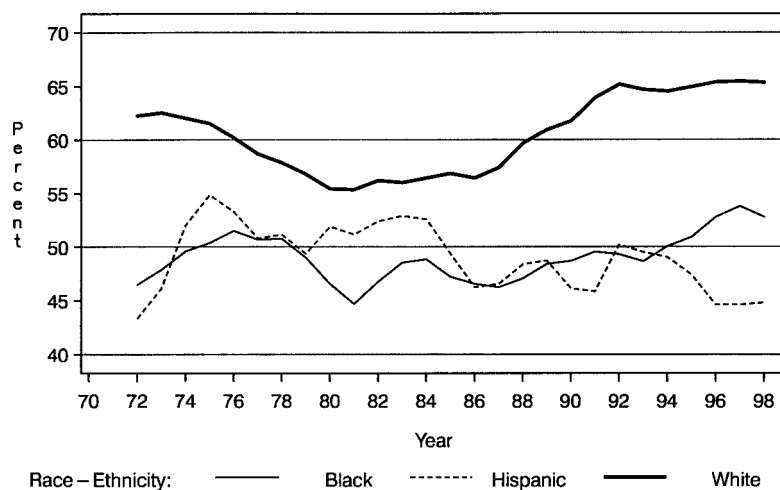


FIG. 1.—College entry proportions of 21–24-year-old male high school graduates and equivalency degree holders. Three-year moving averages are shown. Racial-ethnic groups are mutually exclusive. Source: Authors' calculations from 1972–98 October CPS data files.

ulate the college attendance of students from low-income families (see the essays in Kosters [1999]).²

This paper uses better data and better models than have been used by previous analysts to address the relative importance of long-term family factors compared to short-run liquidity constraints on educational attainment and educational inequality among majority and minority male youth. Using longitudinal data from the National Longitudinal Survey of Youth (NLSY), we estimate how family background, family income, college tuition costs, labor market opportunities, and cognitive ability affect the age- and grade-specific schooling choices of black, Hispanic, and white males starting with those choices made in the early adolescent years. We investigate whether income and other factors are key determinants of educational decisions and, if they are, at what stage in the schooling process they take on their importance.

Schooling attainment is modeled as the outcome of sequential decisions made at each age and each grade from feasible person-specific choice sets. Our analysis builds on our previous work (Cameron and Heckman 1998) and accounts for the selective nature of high school

² The time series for women is different. The gaps are qualitatively the same as for men. However, college enrollment has increased monotonically for all female family income groups since the late 1970s.

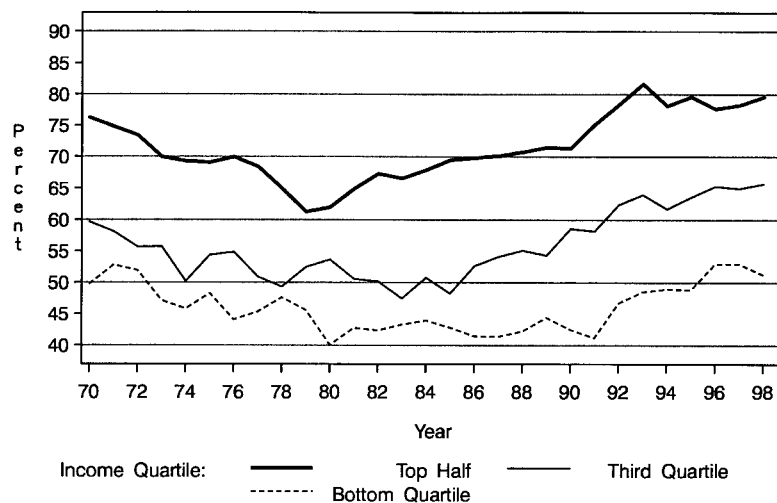


FIG. 2.—College participation by 18–24-year-old male high school completers by parental income quartile. Source: Authors' calculations from October CPS files.

graduates. More able and motivated people progress to higher grades. It is necessary to account for this selection in order to estimate the *ceteris paribus* “causal effect” of the socioeconomic variables we study on educational attainment.

The paper is organized as follows. Section I presents basic facts about educational attainment using the NLSY data and summarizes the conventional interpretation of the evidence. Section II presents the econometric model used to generate estimates. Section III presents estimates of the model. Section IV summarizes the paper.

I. Previous Work, Our Data, and Facts about Schooling Attendance

Influential work on differentials in racial and ethnic schooling attainment by Hauser (1993*a*, 1993*b*), Kane (1994), and others is based on Current Population Survey (CPS) data. These data suffer from major limitations of special importance to analyses of the role of family background and family income on college enrollment decisions. The CPS data report parental characteristics only for persons who are living in the parental home or for those attending college who live in a dormitory or other group quarters. Data on parental characteristics are not available for youths who live on their own, outside of group quarters. The existing evidence on the importance of family background and family

income on schooling choices is derived from samples of dependents, that is, persons living in the parental home or students living in group quarters. These studies analyze the effect of parental variables on college entry conditional on the choice of a living arrangement. These studies thus obscure the effects of parental variables on educational attainment because they condition on another choice variable (residential living decisions) that also depends on parental characteristics. These analysts estimate the effect of parental variables inclusive of their effect on residential decisions. In previous work we demonstrate that, as a consequence of this conditioning, CPS-based studies tend to underestimate the contribution of family income to college attendance decisions because dependency status is positively related to family income (Cameron and Heckman 1993*a*).

We improve on previous work by (1) using a better measure of family income free of the conditioning bias just discussed; (2) estimating a dynamic sequential model of high school graduation and college attendance that controls for the selection bias induced by minority youths who drop out of high school (Willis and Rosen [1979] and Hauser [1993*a*, 1993*b*] consider only the college transition for high school graduates and exclude high school dropouts from their samples); (3) studying schooling transitions from an early age, before much dropping out occurs; and (4) breaking college attendance into two- and four-year college categories.

This paper analyzes the dynamics of schooling attainment from early high school through college entry for black, Hispanic, and white males. We analyze males because their schooling decisions are less complicated by fertility considerations. Our data are drawn from the 1979–91 waves of the NLSY (the military subsample and the nonblack, non-Hispanic disadvantaged samples are excluded). The NLSY collected detailed information about school attendance and completion starting from January 1978. The NLSY allows us to avoid the bias inherent in CPS studies because it is longitudinal, and measures of family income and other variables are available starting as early as age 14 (see App. A), before college and residential decisions are made. Confining the analysis to young men between ages 13 and 16 at the beginning of the sample allows us to construct complete descriptions of schooling dynamics from age 15 through age 24. Few males quit school before age 16, and few return to school after age 24 (see App. A for details).

A. *Basic Facts about Schooling Attainment*

By age 15 substantial differences in schooling attainment already exist across racial-ethnic groups (see fig. 3*a*). Grade 9 is the modal highest grade completed at age 15, and the grade a student would have com-

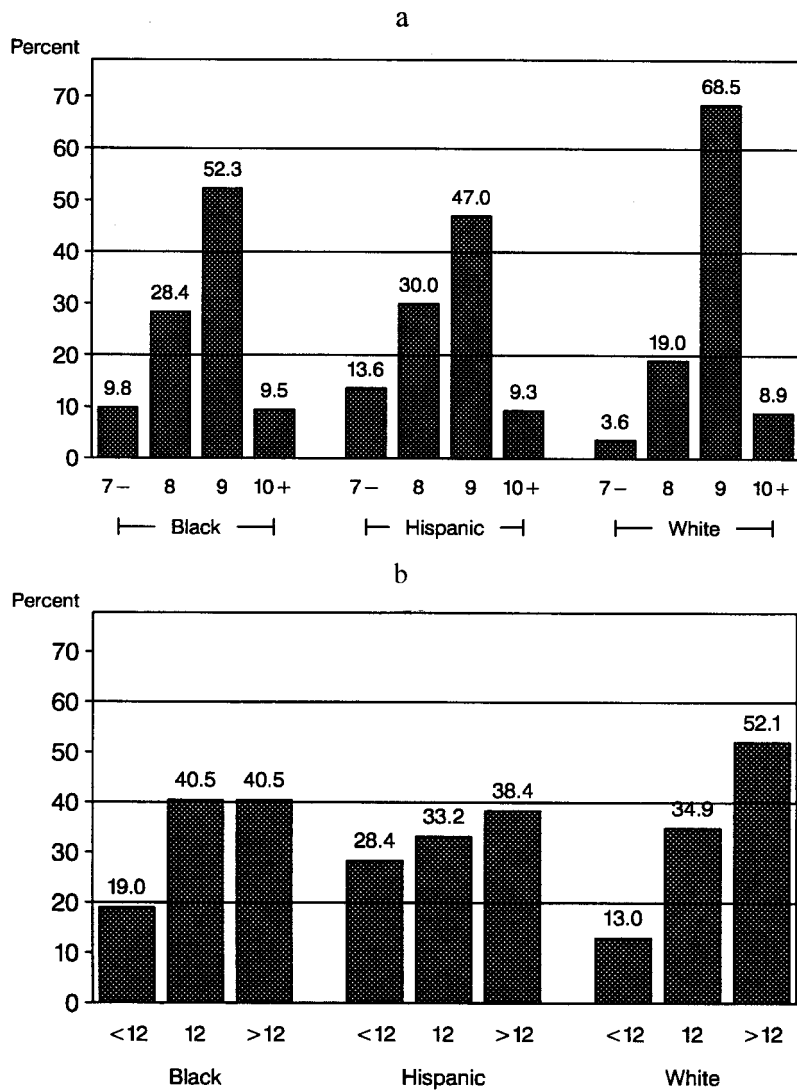


FIG. 3.—*a*, Highest grade completed at age 15. 7- denotes grade 7 or lower, and 10+ denotes grade 10 or higher. *b*, Highest grade completed at age 24. <12 denotes grade 11 or lower, and >12 denotes college attendance. Source: NLSY.

TABLE 1
COLLEGE ENTRY BY AGE 24 FOR HIGH SCHOOL COMPLETERS (Graduates and GED
Completers) BY TYPE OF COLLEGE FIRST ATTENDED

	Four-Year College (1)	Two-Year College (2)	Any College (3)
A. College Entry Proportions			
1. Blacks	.29 (.02)	.21 (.02)	.50 (.02)
2. Hispanics	.25 (.02)	.28 (.02)	.53 (.03)
3. Whites	.35 (.01)	.26 (.01)	.61 (.01)
B. Predicted Attendance if Minority Family Income Distributions Were Equated to the White Distribution*			
1. Blacks	.36 (.04)	.23 (.03)	.59 (.04)
2. Hispanics	.30 (.05)	.28 (.04)	.58 (.05)

SOURCE.—National Longitudinal Survey of Youth.

NOTE.—Standard errors are in parentheses.

* These predictions are based on family income quartile-specific attendance rates. Family income is measured at ages 15–16 except as noted in App. A.

pleted had he entered the first grade at age 6 and attended school continuously through age 15. While the modal grade is the same for all racial-ethnic groups, minorities are 16–21 percent more likely to be below it. These white-minority differences are largely due to uneven chances of grade advancement during elementary school (National Center for Education Statistics 1997). Less than 1 percent of blacks and whites and 2 percent of Hispanics quit school before age 15.³ Blacks and Hispanics start behind and stay behind. Individuals at grade levels below the modal grade are much more likely to drop out and less likely to enter college if they complete high school. Later schooling attainment is highly correlated with early attainment.⁴

Figure 3*b* summarizes the substantial differences in final schooling attainment at age 24. These are essentially life cycle differentials because there is little entry into college after this age. Whites are 12–14 percent more likely to enroll in college by age 24 (category >12 on the figure), and Hispanics are 15 percent more likely than whites to have not finished high school (<12).⁵

Panel A of table 1 shows differentials in college entry rates for two- and four-year college entry by age 24 for individuals who complete high school (including GED recipients). Column 3 for each group displays

³ This finding is due in part to laws about compulsory schooling attendance and to the lack of labor market opportunities for people younger than 16.

⁴ See Cameron and Heckman (1999*b*) for more discussion of these data.

⁵ To accord with census and CPS conventions, GED attainment and traditional high school graduation have been combined into the category “12.” Cameron and Heckman (1999*a*) show that high school completion rates are higher for whites than for minorities, but the opposite is true for GED certification. Eleven percent of white men earn high school credentials via the GED compared to 17 percent of black and 23 percent of Hispanic men.

the fraction who attend either type of college. White graduates are more likely to enter college than blacks (by 11 percentage points) or Hispanics (by eight percentage points). Whites have the highest four-year college entry rate and Hispanics the lowest. Hispanics, however, show the highest two-year entry rate, which is partially attributable to the regional concentration of Hispanics in states, such as California and Texas, with extensive low-tuition community college networks during the time period of our study.

B. Dynamics of Schooling Transitions

Most educational attainment histories in secondary school follow the standard pattern of no interruption or delay. Even when dropouts are liberally classified as students who leave school for at least eight consecutive weeks during the regular school year, returning to school is a rare event. Only 2–6 percent of high school graduates and 6–12 percent of eventual dropouts report at least one episode of leaving and subsequently returning to school. High school graduation occurs almost exclusively at age 18 or 19. Acquisition of the GED is the main vehicle of high school completion after age 19.

Most college entry occurs by age 19 or 20, immediately after high school completion. Among high school graduates, 82 percent of whites and Hispanics and 73 percent of blacks who ever enter college do so within a year of high school graduation. Between 5 and 8 percent wait more than three years. To the extent that delayed college entry can circumvent short-term credit constraints, this evidence suggests that other factors are keeping high school graduates from ever attending college.

C. The Family Income–Schooling Connection

A central question addressed in this paper is how family income during the adolescent years influences schooling attendance. Panel B of table 1 examines this question in a simple way by answering the counterfactual question, How would the white-minority gap in panel A differ if blacks and Hispanics had the same family income distribution at ages 15–16 as whites? Comparing column 3 of panel B to the actual enrollment rates reported in panel A shows that, for blacks, nine percentage points of the 11-percentage-point gap in overall college entry are eliminated by equalizing family income at ages 15–16. Hispanics recover five percentage points of an eight-percentage-point gap. The remaining adjusted gaps for both blacks and Hispanics are statistically insignificant. Column 1 reveals that blacks are slightly more likely than whites to attend a four-year school once family income is equalized. These coun-

terfactual simulations reveal that family income in the adolescent years is apparently an important predictor of both college entry and the type of college attended for blacks and Hispanics. The interpretation to be placed on this evidence is the topic of this paper.

D. Explanatory Variables

Table 2 defines the basic variables we use in our analysis of schooling attainment and presents their means. We have no information about expected future returns to a college education. These returns are difficult to estimate (for us or for the people we study), and in this paper, we use time dummy variables to proxy the changing structure of the returns to education.⁶

Family background characteristics are more favorable for whites.⁷ Parental education is lowest for Hispanics and highest for whites. Broken homes are more prevalent among blacks and least common among whites. Family incomes in the adolescent years are highest for whites and smallest for blacks.

Annual tuition rates at both two-year and four-year public colleges are highest for whites and lowest for Hispanics (tuition is measured at both the state and the county levels, when available). Minorities live in geographical areas with lower tuition costs and with lower commuting costs. The imputed size of Pell grant awards for four-year college tuition and expenses is about \$1,300 for blacks and Hispanics and about \$500 for whites. Hence, annual net tuition at a four-year college ranges, on average, from \$213 for Hispanics to \$1,302 for whites. The second row from the bottom of the table shows that college access is highest for Hispanics: 92 percent of Hispanics live in a county in which either a two- or four-year college is located compared to only 82 percent of whites. Local labor market conditions also vary across racial-ethnic lines. County unemployment rates show little average variation across groups, though average wages for unskilled workers are about 10 percent higher in counties in which Hispanics reside than in counties in which whites live. Black opportunity costs lie in between.

Finally, large differences between white and minority scholastic ability as measured by the Armed Forces Qualification Test (AFQT) are evident. The AFQT figures prominently in our analysis below. Our measure of

⁶ Standard measures of secondary school quality were investigated, but their contribution was found to be negligible when family background measures were included in the list of explanatory variables in the analysis presented below, and so we do not report them here.

⁷ Family size is much larger for blacks and Hispanics than for whites. These reported family sizes are inflated because of the family size-biased nature of the NLSY sampling frame. Adjusting for this oversampling does not affect any conclusion of this paper.

TABLE 2
 NAMES AND MEANS OF VARIABLES USED IN THE SCHOOLING-TRANSITION ANALYSIS

VARIABLE NAME	VARIABLE DETAILS	VARIABLE MEANS		
		White	Black	Hispanic
Highest grade completed of father	Measured at age 14	12.2 (.08)	9.3 (.09)	7.9 (.17)
Highest grade completed of mother	Measured at age 14	11.9 (.06)	10.7 (.08)	7.8 (.16)
Number of siblings	Measured at age 14	2.9 (.05)	4.7 (.09)	4.5 (.12)
Broken home	Absence of one or both biological parents at age 14	.13 (.01)	.43 (.02)	.27 (.02)
Southern residence	In southern census at age 14	.28 (.01)	.57 (.02)	.26 (.02)
Urban residence	In an urban area at age 14	.73 (.01)	.82 (.01)	.93 (.01)
Family income	Two-year average of family income measured at ages 15 and 16 (thousands of 1994 dollars; details in App. A)	44.8 (.06)	25.4 (.05)	30.3 (.07)
County average earnings*	Measured annually in thousands of 1994 dollars (full details in App. A)	20.1 (.32)	21.0 (.12)	22.3 (.15)
County unemployment rate*	Measured annually (full details in App. A)	6.3 (.12)	6.0 (.12)	6.9 (.16)
Four-year college tuition*	In-state tuition and fees at public colleges in county (if available) or state of residence (measured annually in 1994 dollars; details in App. A)	1,850 (14)	1,705 (17)	1,528 (14)
Two-year college tuition*	Same as above for two-year colleges	888 (9)	798 (11)	580 (17)
Four-year college Pell grant eligibility*	Pell grant eligibility (in 1994 dollars; details in App. A); constructed separately for two-year public institutions	548 (12)	1,260 (28)	1,315 (23)
College proximity	Presence of a public two- or four-year school in county of residence; measured annually and also broken out by two- or four-year colleges	.82 (.01)	.89 (.10)	.92 (.01)
AFQT score	Score on the AFQT adjusted for age at test time (details in App. A)	71.8 (.62)	46.4 (1.1)	54.4 (1.9)

NOTE.—Standard errors of the means are in parentheses.

* The means for these time-varying variables are taken at age 18.

the AFQT is age-adjusted. Moreover, because our sample members are 15–18 years of age at the time they take the test (1980) and none had entered college when the AFQT tests were taken, there is little effect of high school graduation and no effect of college attendance on the test score so that the test score is relatively free of endogeneity from schooling.⁸ Further information about the AFQT is given in Appendix A.

The univariate relationships indicate that tuition costs play a small role in accounting for racial and ethnic differences and that family factors and scholastic ability play a more substantial role. These findings are sustained in the more refined econometric analysis, to which we now turn.

II. An Econometric Model of Schooling Attainment

We extend the econometric models previously used in the literature on the economics of schooling attainment by formulating and estimating a dynamic discrete choice model of schooling from age 15 to 24. Previous analysts have estimated the determinants of “highest grade completed” or whether or not a person attends college for a sample of people who have already completed high school. The point of departure for our research is the recognition that schooling attainment at any age is the outcome of previous schooling choices. The probability that a person enters college depends on high school graduation, which in turn depends on finishing grade 11, and so forth back to the earliest schooling decisions. For minority groups and low-income whites, high school graduates are select members of the source population, and it is important to control for the effects of such educational selectivity to isolate causal effects of tuition and family background on college attendance. Cameron and Heckman (1998) document the empirical importance of controlling for educational selectivity in isolating *ceteris paribus* effects of family background on schooling decisions.

Researchers such as Willis and Rosen (1979) and Hauser (1993*a*, 1993*b*) who have studied how family factors affect the highest grade of schooling completed do not distinguish the effect of family income on the high school attendance decisions from its effect on college entry. Family factors and other influences may affect schooling decisions differentially by age and grade level.

To sort out the influence of family income on college entry for high school completers from its accumulated long-term influence in making

⁸ Throughout this paper we use an age-adjusted measure of AFQT. Dropping all persons who had graduated from high school by 1980 from the 15–18-year-old group does not affect any of our conclusions.

people eligible to attend college, conventional methods used in the educational attainment literature are problematic. Our methodology enables us to separate out age-by-age influences of variables such as family income in a general way. By analyzing the entire set of age-specific schooling decisions from age 15 through age 24, we are able to parcel out by age the influences of family income and other variables. Using our estimated econometric model, we can then evaluate the consequences of policies that seek to promote college attendance through raising high school graduation.⁹

Let age be denoted by a ($a \in \{\underline{a}, \dots, \bar{a}\}$, where \underline{a} is the initial age and \bar{a} is the highest age). Schooling choices at a determine schooling levels at age $a + 1$. Schooling attainment at age a is $j_a \in J$ (J is a set of possible attainment states over all ages). Agents with schooling status j_a make their choices about schooling at age $a + 1$ from the feasible choice sets C_{a,j_a} . Let $D_{a,j_a,c} = 1$ if option $c \in C_{a,j_a}$ is chosen by a person of age a with schooling status j_a . When some choice is made, $\sum_{c \in C_{a,j_a}} D_{a,j_a,c} = 1$. The model is fundamentally recursive; the choice made at a affects the choice set at age $a + 1$.

While this notation may appear to be cumbersome, it allows us to consider schooling attainment processes that are more general than the grade progression models presented in Bartholomew (1973), Mare (1980), or Cameron and Heckman (1998). For example, schooling attainment may consist of 11 years of formal school and a GED; grade transition probabilities may be age (or time) specific. The model recognizes the variety of possible schooling trajectories leading to a given level of attainment.

Assume that agents choose optimally at each age and schooling status j_a , inclusive of the options for further schooling opened up by attaining status j (Comay, Melnik, and Pollatschek 1973). Then the optimal choice at age a , denoted by a hat, is

$$\hat{c}_{a,j_a} = \arg \max_{c \in C_{a,j_a}} \{V_{a,j_a,c}\},$$

where $V_{a,j_a,c}$ is the value of option c at age a for a person with j_a years of schooling. Then $D_{a,j_a,c} = 1$ for $c = \hat{c}_{a,j_a}$ and $D_{a,j_a,c} = 0$ otherwise. The model is fundamentally sequential: the choice set C_{a,j_a} confronting the

⁹ Two other problems that past researchers have generally ignored are also handled in our framework. First is the issue of time-varying explanatory variables, such as indicators of the state of the labor market. Previous frameworks are fundamentally atemporal and accommodate variables that change over time only in arbitrary ways. Heckman and Singer (1985) and Heckman and Walker (1990) discuss the introduction of time-varying covariates into duration models. Second, we control for unobserved characteristics. Our framework builds on the work of Cameron and Heckman (1998), who show that serious biases in the estimated effects of family income on schooling arise when unobserved variables that are persistent over grade transitions are not controlled for in estimation.

agent at age a is a consequence of choices made in the previous period. Observe that $j_a = \hat{c}_{a-1, j_{a-1}}$. To avoid notational clutter, henceforth we drop the a subscript on j except where making it explicit clarifies the discussion. We also refer to the choice made at age a as c_a .

For computational simplicity we approximate $V_{a,j,c}$ using a linear-in-the-parameters form:

$$V_{a,j,c} = \mathbf{Z}'_{a,j,c} \boldsymbol{\beta}_{a,j,c} + \epsilon_{a,j,c} \tag{1}$$

where $\mathbf{Z}_{a,j,c}$ is a vector of observed (by the econometrician) constraint and expectation variables at age a for a person of schooling attainment j , and $\epsilon_{a,j,c}$ is an unobservable from the point of view of the economic analyst. Heckman (1981), Eckstein and Wolpin (1989), and others advocate this linear-in-the-parameters structure as a starting point for a more general analysis of discrete dynamic choices. The essential idea in this model, as in any sequential model of discrete choice, is that the choice sets $C_{a,j}$ as captured in (1) by the $\mathbf{Z}_{a,j,c}$, $\boldsymbol{\beta}_{a,j,c}$ and $\epsilon_{a,j,c}$ are determined by previous choices. Econometrically, this creates the possibility at any point in the decision process that the $\mathbf{Z}_{a,j,c}$ conditional on past choices are endogenous.

In this paper we follow the computational simplification for discrete choice processes proposed by Heckman (1981, app.) and assume that $\epsilon_{a,j,c}$ is characterized by a factor structure

$$\epsilon_{a,j,c} = \alpha_{a,j,c} \eta + \nu_{a,j,c} \tag{2}$$

where η is a mean zero, unit variance random variable. We make the following two assumptions.

ASSUMPTION 1. The random variable η is independent of $\nu_{a,j,c}$ for all a , j , and c . In addition, all η and $\nu_{a,j,c}$ are independent across people.

ASSUMPTION 2. The term $\nu_{a,j,c}$ is an extreme value random variable and is independent of all other $\nu_{a',j'',c''}$ except for $a = a'$, $j = j''$, and $c = c''$.

Assumptions 1 and 2 produce an extension of McFadden's (1974) conditional logit model. Conditioning on η , we obtain

$$\begin{aligned} \Pr(D_{a,j,c'} = 1 | \mathbf{Z}_{a,j}, \eta) &= \Pr(\arg \max_c V_{a,j,c} = c' | \mathbf{Z}_{a,j}, \eta) \\ &= \frac{\exp(\mathbf{Z}'_{a,j,c'} \boldsymbol{\beta}_{a,j,c'} + \alpha_{a,j,c'} \eta)}{\sum_{c \in C_{a,j}} \exp(\mathbf{Z}'_{a,j,c} \boldsymbol{\beta}_{a,j,c} + \alpha_{a,j,c} \eta)}, \end{aligned} \tag{3}$$

where $\mathbf{Z}_{a,j}$ is the collection of the $\mathbf{Z}_{a,j,c}$ arrayed in a vector. As a consequence of the one-factor assumption (assumption 1), any dependence

between $D_{a,j,c}$ and $D_{a',j'',c''}$, $a \neq a'$, for the same person conditional on $\mathbf{Z}_{a,j,c}$ and $\mathbf{Z}_{a',j'',c''}$ arises from η , the person-specific effect.¹⁰

One way to eliminate this dependence is to estimate η for each person, or condition it out, using the methods surveyed in Arellano and Honoré (2001). In general, estimating η along with the other parameters of a nonlinear model such as ours produces inconsistent estimates. We estimate our model by making the following assumption.

ASSUMPTION 3. The $\mathbf{Z}_{a,j,c}$ are independent of η for all $a, j \in C_{a,j}$ and for all choice sets.

That is, all $\mathbf{Z}_{a,j,c}$ are independent of η . This does not imply that the $\mathbf{Z}_{a,j,c}$ ($a > \underline{a}$) conditional on past choices are independent of η . In general they are not, so it is necessary to model the history of the process leading up to any transition being analyzed in order to account for the induced conditional endogeneity.¹¹

With these assumptions in hand, we may write down the probability of any schooling history by building up the sequence of age-specific probabilities over the life cycle. Let $D_{\underline{a},c}$ denote the initial schooling attainment state at age \underline{a} (the initial age); $\sum_{c \in C_{\underline{a}}} D_{\underline{a},c} = 1$, where $C_{\underline{a}}$ is the set of possible initial states; and $\mathbf{Z}_{\underline{a}}$ is the data vector for the initial state collecting all of the $\mathbf{Z}_{\underline{a},c}$ into one vector. We denote $d_{\underline{a},c}$ as the realization from $D_{\underline{a},c}$. For simplicity, we also parameterize the initial state probability using our extension of the multinomial logit model:

$$\Pr(D_{\underline{a},c'} = 1 \mid \mathbf{Z}_{\underline{a}}, \eta) = \frac{\exp(\mathbf{Z}'_{\underline{a},c'} \boldsymbol{\beta}_{\underline{a},c'} + \alpha_{\underline{a},c'} \eta)}{\sum_{c \in C_{\underline{a}}} \exp(\mathbf{Z}'_{\underline{a},c} \boldsymbol{\beta}_{\underline{a},c} + \alpha_{\underline{a},c} \eta)}. \quad (4)$$

At age $\underline{a} + 1$, the agent has choice set $C_{\underline{a}+1,c'}$, and we may write the probability that c is chosen given that c' was the initial state at \underline{a} as $\Pr(D_{\underline{a}+1,c,c} = 1 \mid \mathbf{Z}_{\underline{a}+1,c',c}, \eta)$, where we make explicit the conditioning on the previous choice (which defines the feasible choice set at age $\underline{a} + 1$). Note that $j_{\underline{a}+1} = c'$. The probability of any sequence of life cycle schooling histories, such as $(D_{\underline{a},c'} = 1, D_{\underline{a}+1,c_{\underline{a}},c} = 1, D_{\underline{a}+2,c_{\underline{a}+1},c} = 1, \dots, D_{\bar{a},c_{\bar{a}-1},c} = 1)$, given the relevant conditioning sets and η , is

$$\begin{aligned} & \Pr(D_{\underline{a},c'} = 1 \mid \mathbf{Z}_{\underline{a}}, \eta) \cdot \Pr(D_{\underline{a}+1,c',c} = 1 \mid \mathbf{Z}_{\underline{a}+1,c'}, \eta) \\ & \cdot \Pr(D_{\underline{a}+2,c_{\underline{a}+1},c} = 1 \mid \mathbf{Z}_{\underline{a}+2,c_{\underline{a}+1}}, \eta) \cdots \Pr(D_{\bar{a},c_{\bar{a}-1},c} = 1 \mid \mathbf{Z}_{\bar{a},c_{\bar{a}-1}}, \eta). \end{aligned} \quad (5)$$

While notationally formidable, this expression is just the probability that a person starts in initial state $c' \in J$ at age \underline{a} , progresses to $c_{\underline{a}+1}$ at

¹⁰ This model is more general than the McFadden model (*a*) because of the factor component and (*b*) because the coefficients are permitted to vary across elements of the choice set. Normalizations needed to identify the model are presented in App. B.

¹¹ Heckman (1981) and Cameron and Heckman (1998) demonstrate how conditioning on the history of the life cycle process corrects for the induced dependence between η and $\mathbf{Z}_{a,j,c}$, $a > \underline{a}$, given the history of previous choices.

age $\underline{a} + 2$ by choices made at age $\underline{a} + 1$ given the choice set $C_{\underline{a}+1, c_{\underline{a}}}$ produced by the choice at age \underline{a} , and so forth, and eventually progresses to state $c_{\bar{a}}$ at age \bar{a} (the final age) given the choice $c_{\bar{a}-1}$ made at age $\bar{a} - 1$.

The model we estimate integrates out the η in expression (5) using the distribution $F(\eta)$. The full likelihood and a discussion of the non-parametric likelihood estimator and model identification and normalizations for $F(\eta)$ are presented in Appendix B. Asymptotic distribution theory is based on the analysis of Chen, Heckman, and Vytlacil (1998). In brief, we normalize $\text{Var}(\eta) = 1$ and $E(\eta) = 0$. As is common in multinomial logit models, it is necessary to normalize one benchmark state to zero for each choice set ($\beta_{a, j, \tilde{c}} = \mathbf{0}$ and $\alpha_{a, j, \tilde{c}} = 0$ for benchmark state \tilde{c} for each a, j). The benchmark is the dropout state in each transition.

Our model nests a variety of widely used models as special cases. The following assumption delineates our model from these special cases.

ASSUMPTION 4. (A) $C_{a, j} = C_j$ for all a ; (B) $C_j = \{j, j + 1\}$; and (C) $\alpha_{a, j, c} = \alpha_{j, c} = 0$ for all a, j .

The grade progression model of Bartholomew (1973) and Mare (1980) specifies the choice sets facing individuals to be independent of age (part A of assumption 4) and to possess two elements: either continue to the next grade or not. Let j be the current grade level. Then part B of assumption 4 holds. These models also ignore heterogeneity (part C) and so do not account for educational selectivity. Cameron and Heckman (1998) maintain parts A and B of assumption 4 but relax part C. They build models that account for grade-specific effects of a common unobservable variable.

Unlike Willis and Rosen (1979) and Kane (1994), in this paper we model the schooling transitions leading up to high school graduation and do not focus solely on high school graduation and college attendance, excluding high school dropouts. This is empirically important for understanding Hispanic schooling histories because many Hispanics drop out of school before grade 11. Cameron and Heckman (1998) document the empirical importance of controlling for educational selectivity in estimating the ceteris paribus effect of family variables on grade transitions.

III. Evidence on Educational Selectivity and the Dynamics of Schooling

A. Choices: Estimates from the NLSY Data

In this section, we apply the econometric model presented in Section II to jointly estimate the contribution of family income, family background, scholastic ability, tuition costs, and opportunities in unskilled

labor to schooling attainment by age and grade. Our analysis pertains to educational transitions for ages 15–24. We also simulate the estimated model to address three questions: (1) Which variables have the most influence on schooling attainment at various ages? (2) Can differences in personal endowments and family characteristics explain gaps in white-minority schooling attainment? (3) Is the estimated influence of family income on college attendance primarily a consequence of long-run family environment or short-term family borrowing constraints during the adolescent years?

We answer the first question, variable by variable, by equating the distribution of the characteristics for blacks, Hispanics, and whites while holding the distribution of the other characteristics at their sample levels, and by measuring how high school graduation and college enrollment respond. The second question is answered by simulating schooling outcomes when minorities have the entire bundle of variables possessed by whites. We address the third question by comparing the estimated effects of family resources on schooling when scholastic ability (AFQT) or family background variables are included as explanatory variables and when they are not. We interpret AFQT as the outcome of long-term family and environmental factors produced in part from the long-term permanent income of families. Family background variables have the same interpretation. To the extent that the influence of family income measured at a point in time is diminished by the inclusion of AFQT or family background variables, we can conclude that long-run family factors crystallized in these variables are the driving forces behind schooling attainment, and not short-term credit constraints experienced in the late adolescent years.

This section begins with a description of how we implement the econometric model discussed in Section II and the goodness-of-fit tests used to evaluate the estimates produced from it. In a nonlinear model, parameter estimates are difficult to interpret. As a consequence, we focus on simulations of estimated models, which are used to address the questions posed in the introduction to this paper.

B. Estimating the Baseline Econometric Model

The initial conditions of the model are specified in the following way. Individuals enter our sample at age 15 in grade 10, grade 9, or grade 8 or below. From their initial grade, either they stay in school and move to the next highest grade level or they drop out. Thus the set of possible destination states at age 16 is not just the set of highest grades completed but the possible highest grades completed for the number of possible attendance states (attend, not attend, taking or not taking a GED). From age 16, an individual currently attending grade 11 has the option of

continuing in school and graduating or dropping out. An individual who dropped out after completing grade 9 at age 16, for instance, has the options of returning to school to complete grade 10, completing high school through GED certification, or not returning to school.¹² The distinction between GED and traditional high school completion is important in accounting for black, Hispanic, and white schooling differences because of the large number of minorities completing high school through receipt of a GED (Cameron and Heckman 1999*a*).

Once a person finishes high school through GED attainment or high school graduation, he may choose to enter a two-year college or a four-year college or not enter college at all. Once a person enters college, he is no longer followed in our analysis. If a person does not enter college immediately after high school completion, we estimate his chances of college entry until age 24. Very little college entry occurs after age 24, so this restriction is of no practical consequence.

The number of possible transitions proliferates rapidly as individuals get older. There are few observations for many of these transitions, so it is not possible to estimate the associated parameters $\beta_{a,j,c}$ or $\alpha_{a,j,c}$ with any precision. Some judgment has to be made to limit the number of estimated parameters.

Our strategy is as follows.

1. For transitions with relatively few (less than 30) observations, we estimate only the intercepts and not the slope parameters in $\beta_{a,j,c}$. Factor loadings for these parameters ($\alpha_{a,j,c}$) are set to zero.
2. We test for the presence of age (a), state (j), and destination (c) interactions in the slope coefficients (denoted $\beta_{a,j,c}$) and factor loadings ($\alpha_{a,j,c}$) to see if restricted specifications are consistent with the data (we test specifications that suppress some or all of these interactions).
3. We test for differences in the slope coefficients and factor loadings among blacks, whites, and Hispanics for the transitions that are not “rare” as defined in point 1, maintaining ethnic/race-specific intercepts for each transition.

Appendix C (available on request) summarizes these tests. Briefly, we find (*a*) strong evidence of racial and ethnic differences in slope coefficients and factor loadings and (*b*) that age matters greatly in determining schooling transitions. Thus, even when we control for unobservables, the age at which youths attain a grade determines their transitions to later grades. This casts doubt on the conventional grade transition model. By imposing the restrictions that are not rejected, we

¹² Only a small number of people make these transitions; they are not important for our analysis.

alleviate the problem of parameter proliferation and estimate a parsimonious description of life cycle schooling transitions for male youths. We also note in Appendix C that a model that accounts for η , persistent across spells, fits the data better than a model that ignores such persistent heterogeneity.

All the baseline models and tests are developed for the case in which AFQT is excluded from the model. We do this because of the controversial nature of the AFQT variable. This strategy gives us a conventional benchmark framework against which we can compare a model that includes AFQT as a regressor.

We describe the implications of the estimates of the model that survive our specification tests by discussing, in the following order, (1) the determinants of the probability of the initial schooling state, (2) the determinants of secondary school transitions for those who attend school, (3) the schooling decisions for those who have left school, and (4) the college entry decision. Specifications of all transitions in which slope coefficients are estimated include family income, the background variables presented in table 2, average wages in the local labor market, college proximity, and two-year college tuition net of Pell grant subsidies.¹³ Year dummies are entered in all specifications. Two additions to the determinants of college entry complete the model: first, net four-year tuition was entered as the price of four-year college entry and net two-year tuition was included as a two-year college price; second, college proximity was disaggregated into separate measures for two-year college proximity and four-year college proximity. All models are estimated both with and without the AFQT score, and all models are estimated separately for blacks, Hispanics, and whites. Parameter estimates are presented in Appendix C (available on request).

Initial grade level.—As noted, few individuals leave school before age 15. Hence, initial grade levels are estimated by a multinomial logit (controlling for η) for grade 10, 9, or 8 or less.¹⁴ Further disaggregation of “8 or less” into “grade 8” and “grade 7 or less” was not empirically important as judged by model selection criteria.

Secondary school transitions for school attenders.—Because exiting school with a GED in hand is rare (most GED attainers leave school for at least a short period before GED certification), the probability of moving to this state is estimated only with an intercept term (i.e., the slope parameters defined as $\beta_{a,j,c}$ and factor loading $\alpha_{a,j,c}$ are set to zero). In addition, because secondary school transitions are almost nonexistent after age 19, these transitions were also modeled with an intercept only.

¹³ In general, the local unemployment rate was found not to affect schooling choices at any level. See below for more details.

¹⁴ A small number of individuals, less than 1 percent, were already in grade 11 at age 15 and were deleted from the analysis (see App. A).

A more important restriction concerns specification of the origin educational state on secondary school continuation probabilities. For instance, one might ask whether at age 15 the coefficient vectors (both $\beta_{a,j,c}$ and $\alpha_{a,j,c}$) governing school continuation are the same regardless of whether a student is initially in grade 8 or less, grade 9, or grade 10. This restriction is tested in two ways. The most restrictive version of the test assumes that all coefficients including the intercept are identical. The second version of the restriction allows an intercept alone to depend on the origin state. Hence, except for dummy variables that represent grade levels at age $a - 1$, the slope coefficients on $\mathbf{Z}_{a,j,c}(\beta_{a,j,c})$ are identical. The weaker hypothesis is not rejected at any age for any racial-ethnic group. Hence, the only interactions with the current state for the estimated model parameters are intercept terms (see tables C-1 and C-2 in App. C, available on request).

High school dropouts.—High school dropouts face three choices: return to school, obtain a GED, or neither. Because both “return” and “GED” are relatively rare events, these transitions are modeled with a single multinomial logit with heterogeneity for all ages 16–20, with age effects introduced through a set of indicator variables for age. These transitions are parameterized with an intercept term only for ages 21–24.

College entry.—Statistical tests reject the hypothesis that the determinants of two-year college entry are the same as those of four-year entry. Indeed, separating college entry into two- and four-year entry is important in explaining white-minority differences in schooling attendance (see Cameron 1996, tables 35, 37).

C. Goodness of Fit and the Importance of Unobserved Heterogeneity

A natural question is whether it is necessary to control for unobserved heterogeneity. Can a simpler scheme that ignores serially correlated unobservables account for schooling histories? A Bayesian information criterion model selection procedure rejects a specification with no heterogeneity for all racial-ethnic groups (see App. tables C-4 and C-5, available on request).

A second way to judge the importance of heterogeneity is to examine whether or not introducing it makes a difference in estimated coefficients and in estimated marginal probabilities. The impact of background and economic variables is generally much stronger at each grade once account is taken of the selective nature of schooling attainment status. A similar result is reported in Cameron and Heckman (1998). Controlling for heterogeneity is especially important for the analysis of Hispanic college transitions. Recall that Hispanic college attenders are a very select group. For the sake of brevity we do not report these results.

A third way to gauge the importance of correcting for unobserved

heterogeneity, η , is through goodness-of-fit tests. Using a variety of predictions of schooling attainment probabilities at different ages, we find that models with heterogeneity better predict better schooling (see App. tables C-4 and C-5). Therefore, all the simulations conducted below pertain to a model that includes the heterogeneity correction. Specifications with and without AFQT will figure prominently in our interpretation of the data.¹⁵

D. *Are There Differences in Racial-Ethnic Schooling Behavior?*

A central concern of this paper is assessing the relative importance of endowments, resources, and prices facing agents and differentials in racial and ethnic responses to identical constraints and opportunities in explaining racial-ethnic gaps in educational attainment. We take three approaches to making this assessment. First, each model is estimated separately by race, and tests for parameter equality among racial-ethnic groups are conducted age by age to pinpoint differences. Second, we conduct counterfactual simulations to summarize the overall quantitative importance of behavioral differences (parameters) versus endowments (covariates) in explaining racial-ethnic schooling differentials. Third, we present side-by-side comparisons of the effects of individual variables.

The schooling model is estimated separately for blacks, Hispanics, and whites (including separate heterogeneity distributions $F(\eta)$), although imposing a common heterogeneity distribution produces very similar results. When separate heterogeneity distributions are maintained, the behavioral parameters (all the $\beta_{a,j,e}$ parameter vectors) are tested for equality. In all cases, racial-ethnic equality is strongly rejected, indicating that differences in response to the same variables play an important role in explaining schooling differences.¹⁶

A decomposition of the schooling attainment gap into sources due to behavior (parameter differences) and endowments (covariate differences) is described here. Let $\hat{\beta}_w$ and $\hat{\beta}_b$ denote estimates of the white and black parameter vectors, where $\hat{\beta}_w$ and $\hat{\beta}_b$ are inclusive of the $\beta_{a,j,e}$, $\alpha_{a,j,e}$, and $F(\eta)$ estimated for each group. Let \mathbf{Z}_b and \mathbf{Z}_w represent

¹⁵ It is of some econometric interest to notice that AFQT still plays a large role in explaining the dynamics of schooling attainment even after the heterogeneity control is applied. Apparently, the unmeasured factor, sometimes called “ability,” does not fully capture the intertemporal correlation in schooling decisions, nor does it supplant AFQT in explaining the dynamics of schooling attainment. See Cameron and Heckman (1998) for further evidence on this point.

¹⁶ See table C-3 of App. C. Separate age-by-age tests of equality for each schooling transition are also rejected, as are tests for the entire set of parameter estimates. The rejections are stronger when we impose a common heterogeneity distribution across all groups.

random draws from the black and white covariate distributions, respectively. The probability of completing high school at age 24, say, is the sum over the probability of all paths that lead to GED attainment and high school graduation by that age. For convenience, let $\Pr(\cdot)$ denote, say, the overall probability of high school completion by age 24. The following expression shows one way of decomposing the difference in predicted high school graduation rates:

$$\begin{aligned} & E_{\mathbf{Z}_w} \Pr(\mathbf{Z}'_w \hat{\boldsymbol{\beta}}_w) - E_{\mathbf{Z}_b} \Pr(\mathbf{Z}'_b \hat{\boldsymbol{\beta}}_b) \\ &= E_{\mathbf{Z}_w} [\Pr(\mathbf{Z}'_w \hat{\boldsymbol{\beta}}_w) - \Pr(\mathbf{Z}'_w \hat{\boldsymbol{\beta}}_b)] + [E_{\mathbf{Z}_w} \Pr(\mathbf{Z}'_w \hat{\boldsymbol{\beta}}_b) - E_{\mathbf{Z}_b} \Pr(\mathbf{Z}'_b \hat{\boldsymbol{\beta}}_b)] \\ &= \text{gap due to behavior} + \text{gap due to endowments}, \end{aligned} \quad (6a)$$

where $E_{\mathbf{Z}_b}$ and $E_{\mathbf{Z}_w}$ are expectations taken with respect to the distributions of the random vectors \mathbf{Z}_b and \mathbf{Z}_w . The “behavior” gap lets behavior differ between whites and blacks, as reflected in the parameter estimates, but imposes a distribution of covariates for the black population identical to the distribution in the white population. The “endowment” gap reveals the portion of the total high school graduation gap attributable to differences in the distributions of \mathbf{Z}_b and \mathbf{Z}_w and is evaluated using the black parameter estimates.

An alternative way of making the decomposition evaluates the “behavior” difference at the black covariate distribution and the “endowment” difference for whites:

$$E_{\mathbf{Z}_b} [\Pr(\mathbf{Z}'_b \hat{\boldsymbol{\beta}}_w) - \Pr(\mathbf{Z}'_b \hat{\boldsymbol{\beta}}_b)] + E_{\mathbf{Z}_w} \Pr(\mathbf{Z}'_w \hat{\boldsymbol{\beta}}_w) - E_{\mathbf{Z}_b} \Pr(\mathbf{Z}'_b \hat{\boldsymbol{\beta}}_w). \quad (6b)$$

Because estimated parameter vectors are not identical, the two decompositions will weight various elements of the covariate vector differently. In principle, one can have two very different estimates of the relative importance of endowment and behavior in explaining racial-ethnic schooling differentials.

Table 3 reports sample analogues of counterfactual schooling attainment gaps corresponding to the “behavior” difference:

$$E_{\mathbf{Z}_w} [\Pr(\mathbf{Z}'_w \hat{\boldsymbol{\beta}}_w) - \Pr(\mathbf{Z}'_w \hat{\boldsymbol{\beta}}_b)], \quad (7a)$$

$$E_{\mathbf{Z}_b} [\Pr(\mathbf{Z}'_b \hat{\boldsymbol{\beta}}_w) - \Pr(\mathbf{Z}'_b \hat{\boldsymbol{\beta}}_b)]. \quad (7b)$$

Estimates of (7a) reveal the size of the white-black schooling gap if blacks were given the white endowment distribution, and estimates of (7b) show the size of the schooling gap were whites assigned the black endowment distribution. If endowments fully explain schooling gaps, behavioral differences are unimportant, and the expressions will be near

TABLE 3
CAN COVARIATE DIFFERENCES EXPLAIN RACIAL-ETHNIC SCHOOLING GAPS?

	White-Black Gap (1)	White-Hispanic Gap (2)
A. Complete Grade 9 or More by Age 15 (Initial Condition)		
1. Actual white-minority gap Exclude AFQT:	.16 (.02)*	.21 (.02)*
2. Gap when minorities have white covariates	.02 (.03)	.04 (.05)
3. Gap when whites have minority covariates	-.01 (.04)	-.00 (.06)
Include AFQT:		
4. Gap when minorities have white covariates	-.05 (.03)*	-.01 (.05)
5. Gap when whites have minority covariates	-.10 (.03)*	-.02 (.07)
B. High School Completion Gap (In- cludes GED Attainment)		
1. Actual white-minority gap Exclude AFQT:	.06 (.01)*	.14 (.02)*
2. Gap when minorities have white covariates	-.05 (.04)	.01 (.05)
3. Gap when whites have minority covariates	-.07 (.04)*	-.04 (.05)
Include AFQT:		
4. Gap when minorities have white covariates	-.09 (.03)*	-.03 (.04)
5. Gap when whites have minority covariates	-.14 (.03)*	-.12 (.04)*
C. College Entry Probabilities Given High School Completion		
1. Actual white-minority gap Exclude AFQT:	.11 (.02)*	.07 (.02)*
2. Gap when minorities have white covariates	-.00 (.02)	-.06 (.04)*
3. Gap when whites have minority covariates	-.07 (.03)*	-.12 (.04)*
Include AFQT:		
4. Gap when minorities have white covariates	-.08 (.02)*	-.07 (.03)*
5. Gap when whites have minority covariates	-.14 (.02)*	-.14 (.04)*
D. Population College Entry Gap (Un- conditional on High School Completion)		
1. Actual white-minority gap Exclude AFQT:	.12 (.02)*	.14 (.02)*
2. Gap when minorities have white covariates	-.04 (.04)	-.04 (.05)
3. Gap when whites have minority covariates	-.09 (.02)*	-.12 (.04)*

TABLE 3
(Continued)

	White-Black Gap (1)	White-Hispanic Gap (2)
Include AFQT:		
4. Gap when minorities have white covariates	-.12 (.03)*	-.07 (.04)*
5. Gap when whites have minority covariates	-.16 (.03)*	-.15 (.04)*

NOTE.—In all cases, the actual schooling gaps computed from actual sample outcomes and from the estimated model are identical in the first two significant digits. Standard errors (in parentheses) were calculated using 500 random draws from the distributions of the underlying estimated parameters.

* Significant at the 10 percent level or less.

zero. If behavior and not endowments underlies actual schooling differences, the expressions will be about the same size as the actual schooling gap.¹⁷

Schooling gaps are simulated at four levels and shown in panels A–D of the table: the initial state at age 15, high school completion (GED attainment and high school graduation combined), college entry conditional on high school completion (combined two- and four-year entry), and overall college entry unconditional on high school completion. The first row of each panel shows the actual gap between whites and minorities.¹⁸ Throughout the table, standard errors of the predictions are given in parentheses, and an asterisk is used to denote statistical significance at the 10 percent level or less. The second and third rows of each panel present simulated schooling gaps when minorities receive the white distribution of endowments (corresponding to the counterfactual shown in eq. [7a]) and when whites receive the minority distribution (corresponding to [7b]). Endowments include all family background, family income, and other variables used in the analysis. The fourth and fifth rows of each panel repeat the exercise when AFQT scores are included in the analysis. A positive number means that whites are more likely to achieve the indicated schooling level, and a negative number indicates the opposite.

¹⁷ In these decompositions, racial-ethnic differences in heterogeneity distributions are folded into the “behavior difference.” An alternative decomposition breaks the behavior differences into differences in the distribution of the unobserved heterogeneity distributions, $F(\eta)$, and differences in other behavioral parameters. Though accounting for unobserved heterogeneity is important in explaining schooling choices for each racial group, differences in estimated heterogeneity distributions across racial and ethnic groups explain little of the attainment gaps between whites and minority groups. In no case can differences in $F(\eta)$ account for more than one or two percentage points of white-minority schooling gaps reported in table 2. See table C-6 of App. C (available on request) for these results.

¹⁸ Values of predicted schooling attainment are identical to corresponding sample values up to the third significant digit in all cases, indicating that predicted values were reliable estimates of actual values.

Row 1 of panel A shows the percentage point difference between whites and blacks (col. 1) and whites and Hispanics (col. 2) in achieving grade 9 or higher at age 15. The gap is 16 percentage points for blacks and 21 percentage points for Hispanics. Rows 2 and 3 show that equating endowments essentially eliminates the schooling disparities. What is left over is not statistically significantly different from zero. Adding scholastic ability as measured by AFQT to the model (rows 4 and 5) shows that blacks and Hispanics are more likely than whites (by between one and 10 percentage points) to be at the mode grade or above at age 15.¹⁹

Panel B presents counterfactual simulations of giving minorities the white endowment for high school completion rates by age 24. Of the actual six- and 14-percentage-point gap between white-black and white-Hispanic high school completion, rows 2 and 3 show blacks gaining a five- to seven-percentage-point advantage over whites when endowments are equal, whereas white and Hispanic completion rates become statistically identical. Equating AFQT with the other endowments raises the black advantage to between nine and 14 percentage points. Hispanics gain a three- to 12-percentage-point advantage. The minority advantage is statistically nonnegligible.

Among high school graduates, assigning minorities white endowments gives minorities an advantage between zero and 12 percentage points over whites in college entry (panel C), which rises to between eight and 14 percentage points when AFQT is also equalized. The estimated minority advantage is statistically significant.

Panel D decomposes the *population* probability of college entry (i.e., not conditioning on high school completion). The gap in the unconditional probabilities of college entry is a function of the differentials shown in panels A–C. The actual gap in college entry for blacks and Hispanics compared to whites is 12 and 14 percentage points, respectively. In accordance with the findings above, equating endowments between whites and minority groups raises minority schooling above the white rate by between four and 12 percentage points. The minority advantage jumps to between 12 and 16 percentage points for blacks and between seven and 15 percentage points for Hispanics when scholastic ability, as measured by the AFQT, is included in the empirical model.²⁰

¹⁹ One potential problem with the use of the AFQT score in these data is that the test was taken in 1980, while the sample members were still of high school age. Thus, because AFQT is both a cause and a consequence of schooling, there may be a problem with reverse causality. However, for our subsample of the NLSY, AFQT was measured before any member of the sample was eligible for college entry. Thus AFQT is predetermined with respect to college entry. In addition, our measure of AFQT is age-adjusted.

²⁰ One might attempt to explain the covariate-adjusted minority advantage over whites in college entry as a result of race-directed preferences and subsidies. It is important to keep in mind that the minority advantage appears for high school completion and completion of grade 9 or higher at age 15. At these grades there is no affirmative action.

Table 3 reveals that at each schooling level minority schooling generally rises above white levels when endowments are equalized. The minority advantage is larger when the white-minority schooling gap is predicted using the minority distribution of endowments than when it is predicted at higher socioeconomic values using the white distribution. This finding indicates that the minority advantage over whites in schooling completion is more pronounced at the lower end of the socioeconomic scale and lessens as we move up the scale. The point is reinforced when AFQT is added to the model.

The effect of equalization of endowments on schooling attainment of earlier levels of education has not previously been studied. Previous analysts have studied high school completion and college entry using dummy variables for different race groups while constraining slope coefficients on controls to be the same. We fit more general models for all levels of attainment with separate slope coefficients by race. We reject the hypothesis of equality of response to common opportunities (i.e., common slope parameters for whites and minorities) that is assumed in previous studies of ethnic and racial differences in educational attainment. Ours is the most general analysis yet performed on this question because we consider more grade transitions, allow for dynamic responses to time-varying variables such as wages and tuition costs, test for common behavior (common slope parameters), and control for educational selectivity, which, if uncontrolled, could generate differences in estimated racial and ethnic coefficients even when there are no differences in the true parameters.²¹ In results reported in table C-6 of Appendix C, when a common heterogeneity distribution is used for all racial and ethnic groups, similar qualitative findings emerge.²²

Other evidence supports the idea that race-directed subsidies cannot account for the minority advantage. Both Kane (1998) and Brewer and Eide (1999) show that race-based preferences and subsidies to college entry have been concentrated in elite four-year colleges but are practically nonexistent at two-year and unselective four-year schools.

²¹ See, e.g., Hauser (1993*a*), who uses parental background but not AFQT in reaching similar conclusions regarding the importance of long-term factors on college enrollment. The effect of equalization of background variables in making college entry higher for blacks than for whites has been noticed in a number of highly restricted models of high school completion or college entry, usually in a probit model as the estimated coefficient associated with a binary indicator of racial-ethnic identity. However, previous research does not control for educational selectivity as we do, leaving open the possibility that the effect is an artifact of selection bias. Our analysis reveals that the effect remains even when we control for selection bias. The finding that blacks and Hispanics are more likely both to complete high school and to enter college is robust across a number of specifications. A simpler specification that does not disaggregate college entry into two- and four-year college entry produces slightly larger estimates of the minority advantage in college entry. Disaggregation of the schooling and dropout states into working, not working, and schooling states produces the same conclusions (see Cameron 1996, table 36).

²² However, minority schooling attendance generally increases when a common distribution is imposed but AFQT is deleted from the model. Giving minorities the white distribution of unobservables operates like an increase in AFQT. This suggests that η is

E. Effects of Individual Variables

Table 4 presents evidence on sources of schooling differences between whites and minorities by decomposing the schooling gaps just studied into the contribution made by each explanatory variable. It also presents evidence on the robustness of family income and other family factors when AFQT is included as an explanatory variable. Because racial and ethnic groups may vary in their sensitivity to differences in family income or tuition policies, the schooling attainment predictions displayed in the table are based on estimates of the econometric model that are made separately for each racial-ethnic group.

Panel A of table 4 presents counterfactual simulation results for completing grade 9 by age 15, the first schooling attainment state for our model. Panel B shows results for completing high school (by graduation or exam certification). Panel C displays the simulations for entry into college conditional on completing high school, and panel D presents the results for entering college unconditional on high school attainment. This final simulation measures the net effects of background on college entry operating through schooling completion at all prior stages.

The last row in each panel shows the actual white-minority gaps in attainment, which in all cases are essentially identical to the gap predicted by the model. Rows 1–5 in panel A and rows 1–8 in panels B–D present the changes in the schooling gaps when the variable named in the left-hand column is adjusted to the white mean level while the other variables are held fixed at minority sample values. For example, the number in column 1 and row 1 of panel A shows that if the six components of family background listed in rows 1*a–f* are adjusted for blacks to white levels, then the black rate of completing grade 9 closes by six percentage points to only 10 percentage points lower than the white rate rather than the 16 percentage points actually found in the data.²³

In panel A, rows 1*a–f* show the change in the gap when the listed individual components of family background are equated. Row 2 shows the change when minority family income is adjusted to white levels. In panels B–D, row 3 displays the same change when county average wages are equalized, and row 4 shows the effect of adjusting both tuition and college proximity. Measures of local wage rates, college tuition, and

capturing ability because when measures of ability are added, the adjusted gap in favor of minorities increases.

²³ In these simulations, only the means of the variables are equated between the two groups. Consider the adjustment for sibling size. This is accomplished by finding the difference in the mean number of siblings. Because these models are nonlinear, there are other ways of making these simulations, such as using the marginal distributions of white attributes but preserving the original covariance structure. These more elaborate methods have little impact on the simulation results. We report the simplest and most easily replicable results. The small size of our samples prevents us from using nonparametric methods to equate the full distribution of characteristics.

TABLE 4
CHANGE IN THE PREDICTED WHITE-MINORITY SCHOOLING GAP WHEN MINORITY
EXPLANATORY VARIABLES ARE EQUATED TO WHITE LEVELS

	WITHOUT AFQT SCORE		WITH AFQT SCORE	
	Black (1)	Hispanic (2)	Black (3)	Hispanic (4)
A. Change in Minority Probability of Being in Grade 9 or Higher at Age 15				
1. Equating all family background components	.06 (.020)*	.08 (.023)*	.01 (.022)	.02 (.019)
Individual components:				
<i>a.</i> Number of siblings	.03 (.009)*	.03 (.012)*	.02 (.010)*	.01 (.012)
<i>b.</i> Highest grade of father	.04 (.021)*	-.01 (.028)	.01 (.022)	-.03 (.029)
<i>c.</i> Highest grade of mother	.01 (.005)*	.06 (.020)*	.004 (.007)	.04 (.021)*
<i>d.</i> Broken home	-.01 (.009)	-.001 (.007)	-.004 (.010)	.003 (.007)
<i>e.</i> Urban residence	-.004 (.009)	-.004 (.009)	-.01 (.010)	.001 (.006)
<i>f.</i> Southern residence	-.01 (.004)*	-.000 (.010)	-.02 (.008)*	-.000 (.007)
2. Equating family income	.09 (.023)*	.12 (.021)*	.07 (.024)*	.04 (.020)*
3. Equating AFQT scores	NA	NA	.17 (.033)*	.16 (.026)*
4. Equating rows 1 and 2	.14 (.025)*	.18 (.032)*	.07 (.024)*	.06 (.026)*
5. Equating rows 1-3	NA	NA	.22 (.028)*	.22 (.030)*
6. Actual white-minority gap	.16	.21	.16	.21
B. Change in Minority Probability of High School Completion at Age 24 (High School Graduation and GED Attainment Combined)				
1. Equating all family background components	.07 (.013)*	.05 (.021)*	.03 (.015)*	-.01 (.024)
Individual components:				
<i>a.</i> Number of siblings	.02 (.007)*	.03 (.010)*	.01 (.007)*	.01 (.010)
<i>b.</i> Highest grade of father	.04 (.015)*	-.01 (.032)	.02 (.016)	-.04 (.029)*
<i>c.</i> Highest grade of mother	.01 (.005)	.03 (.020)	-.001 (.004)	.02 (.022)
<i>d.</i> Broken home	.01 (.008)	-.001 (.003)	.01 (.007)	-.001 (.005)
<i>e.</i> Urban residence	.001 (.005)	.004 (.003)	-.01 (.007)	-.01 (.007)
<i>f.</i> Southern residence	.01 (.012)	.001 (.003)	.01 (.004)	-.001 (.004)
2. Equating family income	.07 (.016)*	.08 (.018)*	.05 (.017)*	.02 (.019)

TABLE 4
(Continued)

	WITHOUT AFQT SCORE		WITH AFQT SCORE	
	Black (1)	Hispanic (2)	Black (3)	Hispanic (4)
3. Equating local average wages	.01 (.004)*	.01 (.008)	.001 (.002)	-.01 (.010)
4. Equating tuition and college proximity	-.004 (.003)*	-.004 (.009)	-.007 (.003)*	.009 (.008)
5. Equating AFQT scores	NA	NA	.11 (.019)*	.16 (.019)*
6. Equating rows 1 and 2	.12 (.013)*	.12 (.021)*	.07 (.016)*	.01 (.023)
7. Equating rows 1-4	.12 (.013)*	.13 (.023)*	.07 (.017)*	.02 (.028)
8. Equating rows 1-5	NA	NA	.16 (.014)*	.17 (.025)*
9. Actual white and minority gap	.06	.14	.06	.14
C. Change in Minority College Entry Probabilities at Age 24 Given High School Completion				
1. Equating all family background components	.10 (.027)*	.11 (.026)*	.06 (.021)*	.06 (.015)*
Individual components:				
a. Number of siblings	.03 (.012)*	.03 (.012)*	.02 (.011)*	.01 (.009)
b. Highest grade of father	.08 (.028)*	.03 (.032)	.06 (.020)*	.02 (.029)
c. Highest grade of mother	.003 (.008)	.05 (.025)*	-.005 (.006)	.01 (.020)
d. Broken home	-.01 (.011)	.01 (.009)	-.002 (.009)	.01 (.009)
e. Urban residence	.01 (.007)	-.001 (.005)	.01 (.013)	.01 (.007)
f. Southern residence	-.01 (.005)*	-.001 (.008)	-.02 (.009)*	-.001 (.008)
2. Equating family income	.05 (.023)*	.03 (.013)*	-.001 (.010)	-.02 (.019)
3. Equating local average wages	.004 (.006)	.04 (.013)*	.002 (.003)	.03 (.010)*
4. Equating tuition and college proximity	-.03 (.006)*	-.05 (.016)*	-.02 (.004)*	-.05 (.016)*
5. Equating AFQT scores	NA	NA	.15 (.028)*	.12 (.022)*
6. Equating rows 1 and 2	.14 (.027)*	.13 (.023)*	.06 (.023)*	.04 (.023)*
7. Equating rows 1-4	.12 (.029)*	.13 (.030)*	.04 (.025)*	.02 (.030)
8. Equating rows 1-5	NA	NA	.20 (.029)*	.14 (.030)*
9. Actual white and minority gap	.11	.07	.11	.07

TABLE 4
(Continued)

	WITHOUT AFQT SCORE		WITH AFQT SCORE	
	Black (1)	Hispanic (2)	Black (3)	Hispanic (4)
D. Change in Minority College Entry Probabilities at Age 24 (Unconditional on High School Graduation)				
1. Equating all family background components	.13 (.027)*	.12 (.026)*	.06 (.023)*	.03 (.025)
Individual components:				
<i>a.</i> Number of siblings	.04 (.011)*	.04 (.013)*	.02 (.010)*	.01 (.012)
<i>b.</i> Highest grade of father	.10 (.026)*	.02 (.021)	.06 (.020)*	-.01 (.026)
<i>c.</i> Highest grade of mother	.004 (.008)	.06 (.028)*	-.005 (.006)	.02 (.023)
<i>d.</i> Broken home	-.01 (.011)	-.01 (.007)	.003 (.010)	.01 (.007)
<i>e.</i> Urban residence	.01 (.011)	-.001 (.010)	.005 (.012)	.01 (.008)
<i>f.</i> Southern residence	-.003 (.011)	-.001 (.006)	-.02 (.011)*	-.001 (.007)
2. Equating family income	.09 (.023)*	.06 (.021)*	.03 (.022)	-.007 (.018)
3. Equating local average wages	.01 (.005)*	.04 (.013)*	.001 (.005)	.02 (.013)
4. Equating tuition and college proximity	-.03 (.006)*	-.04 (.015)*	-.02 (.005)*	-.04 (.013)*
5. Equating AFQT scores	NA	NA	.20 (.030)*	.19 (.023)*
6. Equating rows 1 and 2	.19 (.027)*	.18 (.030)*	.09 (.029)*	.03 (.026)
7. Equating rows 1-4	.16 (.029)*	.18 (.032)*	.07 (.030)*	.01 (.030)
8. Equating rows 1-5	NA	NA	.25 (.033)*	.21 (.034)*
9. Actual white and minority gap	.12	.14	.12	.14

NOTE.—The simulations were calculated by shifting the mean of the explanatory variables for minorities to the mean of the variables for whites. For example, the number in row 2 of panel D represents the rise in college entry if all black families were given an income transfer equal to the mean difference in black and white family incomes. The standard errors (in parentheses) were calculated using 500 draws from the estimated parameter distributions.

* Significant at the 5 percent level or less.

college proximity are not included as determinants of the initial condition.²⁴ The effect of equalizing ability test scores is shown in row 3 of panel A and row 5 of panels B–D. Finally, rows 4 and 5 in panel A and rows 6–8 in panels B–D show the combined effects of various incremental simulations. Columns 1 and 2 show the change in the predicted gap between whites and blacks and whites and Hispanics, respectively,

²⁴ There is no obvious economic justification for including these variables as determinants of grade level at age 15, and when included, their effects are unimportant.

when AFQT is deleted. Columns 3 and 4 show the corresponding calculation when AFQT score is included in the set of explanatory variables as a proxy for long-term family and environmental influences.²⁵

From columns 1 and 2 in each panel, we reach several important conclusions. First, row 2 shows that adjusting for family income alone largely eliminates the white-black gap in high school completion and a substantial fraction of the Hispanic-white differential. A similar conclusion holds for college entry given high school completion (panel C), though the effect is less dramatic. Equating family income raises black and Hispanic college enrollment five and three percentage points, respectively. The actual gaps for blacks and Hispanics are 11 and seven percentage points. At age 15, family income alone accounts for about half the sizable gap in grade 9 completion.

Before we conclude that family income is the whole story in explaining racial and ethnic schooling differentials, it is important to examine rows 1*a-f*, which show that a similar story can be told for family background factors. The effects of equating family background (holding family income and other explanatory variables constant) are particularly strong for attending college. For blacks and Hispanics, they explain, respectively, 10 and 11 percentage points of the 11- and seven-percentage-point gaps between blacks and whites (panel C).

Local labor market variables representing opportunities available to persons with little education are behaviorally significant in our estimated model. However, they contribute only modestly or not at all to explaining white-minority differences (row 3 of panels B–D) because average differences in local labor market conditions among the groups are small (see the means in table 2). The wage effect is statistically significant half the time. Nevertheless, in all but one instance, equating wages raises minority schooling, particularly for Hispanic college entry. The positive wage effect is due to higher wage rates in areas in which blacks and Hispanics are concentrated; lowering the opportunity cost of schooling to white levels *promotes* minority schooling completion.

Adjusting for tuition and college proximity *increases* the gap between whites and minorities for both college entry (both unconditional and for high school graduates) and high school completion (row 4 of panels

²⁵ Only year effects are not reported. They never account for more than one-half of one percentage point of the schooling gap shown in the last row of each panel. The difference in the actual gap shown in the last row and the simulated gap shown in the second and third rows from the bottom are analogous to those shown in rows 2 and 4 of each panel in table 3. This difference is identical or within a percentage point of the difference shown in table 3.

B–D).²⁶ Blacks and Hispanics face lower average tuition and lower college commuting costs than whites. Equating minority to white levels lowers college participation for blacks and Hispanics. Tuition and commuting costs do not explain minority-majority discrepancies in college attendance.

Row 4 of panel A and row 6 in panels B–D show the combined effect of adjusting black (col. 1) and Hispanic (col. 2) levels of both family background and family income to white levels when AFQT is excluded from the estimation equations. Family background and income together substantially explain white-minority schooling gaps of all four schooling levels. In fact, for the higher levels, in all cases except one (Hispanic high school completion), adjusting family background and family income to white levels overpredicts the schooling attainment gap. Blacks and Hispanics are more likely to complete high school and attend college than whites with the same levels of family background and income. The same conclusion applies to row 7 of panels B–D, which shows the change in the gap when all minority variables listed in rows 1–4 are equated to white levels. Other variables explain little of the white-minority schooling gaps.

Columns 3 and 4 repeat the same simulation exercises when AFQT is included as an explanatory variable. Comparing the family income and background effects in row 2 when AFQT is in the model (cols. 3 and 4), and when it is not, demonstrates that the effect of the family income variables for high school completion, college attendance given completion, and college attendance is substantially weakened by the inclusion of AFQT scores. What is noteworthy is that the estimated family income effects are weakened most in the later schooling transitions. Family income continues to play an important role in explaining grade at age 15, a modest role in explaining high school completion decisions, and no role in college entry decisions.

Equalization of schooling attainment also appears, albeit in a somewhat less dramatic fashion, when family background variables other than income are included in the model (see row 1). Thus our conclusions do not rely solely on estimates based on models that include the AFQT. Long-term family factors and not family income account for much more of the college enrollment gap. This does not say that long-run family income does not matter in explaining college attendance; family income has positive effects on initial grade and high school completion, making a person eligible for college entry. However, it does say that short-run

²⁶ College tuition and proximity were included in the explanatory variables for high school completion. As college entry may be a primary motivator of high school graduation, higher college costs could lead to lower high school graduation rates. We find little evidence of such effects. For college entry, by contrast, both college tuition and proximity are statistically significant and numerically important for all race groups.

liquidity constraints experienced at the college-going ages are much less important than long-run factors in promoting college attendance.

Equating family background and income raises Hispanic high school completion and college entry for high school graduates by 12 and 13 percentage points, respectively, when AFQT is not included (row 6 of panels B and C). The corresponding reduction in the gaps due to these variables when AFQT is included is only one and four percentage points, respectively. If minority AFQT alone were adjusted to white levels, then overall college entry would rise by 20 percentage points for blacks and 19 percentage points for Hispanics (row 5 of cols. 3 and 4 in panel D). The effects of conditioning on AFQT in the college entry equation for high school completers are 15 and 12 percentage points for blacks and Hispanics, respectively (panel C). Regardless of income and family background, at the same AFQT level, blacks and Hispanics enter college at rates that are substantially *higher* than the white rate. The predictions for high school completion are similarly dramatic. The role of AFQT in explaining racial and ethnic schooling differences is thus seen to be very important. It is long-run factors that promote scholastic ability that explain most of the measured gaps in schooling attainment, and not the short-run credit constraints faced by students of college-going age that receive most of the attention in popular policy discussions. The long-run factors that promote college readiness are proxied by AFQT. Even if we exclude AFQT from the analysis, parental background factors play essentially the same role as AFQT, although the effects are weaker.

Elsewhere (table C-7 of App. C), we compute estimated average derivatives of the probability of completing various levels of schooling by age 24 with respect to the covariates, separately for blacks, Hispanics, and whites. Consistent with the results above, introducing AFQT eliminates the effect of family income on college entry for high school completers, but it does not eliminate the effect of family income on earlier schooling transitions. Family income still plays a role at earlier grades, albeit a diminished one when AFQT is included in the model. If family income is interpreted as a source of short-run credit constraints, credit constraints are more important for the high school dropout and completion decision than for the college enrollment decision.

Table 5 amplifies the family income findings by documenting predicted changes in schooling attainment in response to a substantial \$10,000 rise in family income on college entry among high school completers, both with and without AFQT included in the prediction equations. Comparing rows 1 and 2 of panel A shows that family income effects of college enrollment essentially vanish when AFQT is included in the model. (Similar results are found for the effect of the same increase in family income on college enrollment not conditioning on high school graduation. Again, the effects of family income at earlier

TABLE 5
EFFECT OF A \$10,000 INCREASE IN FAMILY INCOME ON THE CHANCES OF TWO-YEAR AND
FOUR-YEAR COLLEGE ENTRY BY HIGH SCHOOL COMPLETERS

	Black (1)	Hispanic (2)	White (3)
A. Combined Two- and Four-Year College Effect			
1. Excluding AFQT	.032 (.011)*	.016 (.007)*	.016 (.006)*
2. Including AFQT	-.001 (.012)	-.015 (.012)	.004 (.006)
B. Excluding AFQT and Disaggregating Two-Year and Four-Year Effects			
1. Two-year college	.004 (.007)	-.005 (.005)	-.004 (.005)
2. Four-year college	.027 (.008)*	.020 (.005)*	.020 (.005)*
C. Including AFQT and Disaggregating Two-Year and Four- Year Effects			
1. Two-year college	-.005 (.008)	-.010 (.008)	-.006 (.005)
2. Four-year college	.005 (.008)	-.004 (.010)	.010 (.005)*

NOTE.—Standard errors are in parentheses. High school completers are composed of GED recipients and high school graduates combined.

* Significant at the 5 percent level or less.

transitions are stronger.) Panels B and C of the table make an additional point. Even when AFQT is not included among the explanatory variables, family income is only a trivial determinant of two-year college entry. Rows 1 and 2 of panel B reveal that the measured effect of family income is largely due to its influence on four-year entry and not to its effect on two-year college entry. Including AFQT among the explanatory variables (panel C) does not reverse this conclusion; it only weakens the estimated effect of family income on four-year college entry. This evidence reveals that when we control for ability, the estimated relationship between income and college entry is mainly a relationship between income and entry into a four-year college.

It might be argued that our measure of family income suffers from measurement error, and a weak estimated family income effect conditional on long-term factors is simply a consequence of this. Elsewhere (Cameron and Heckman 1999b), we address this problem and find that alternative measures of family income still result in a weak effect of family income on schooling in a model identical to the one discussed here. Three-year averages and single-year measures of family income produce results little different from those reported in this paper.²⁷

²⁷ These simulations are available on request from the authors. Multiple-year measures of family income are available only for a substantially smaller subsample of the data. For that reason we report the results for the two-year average of income, measured at ages 15–16. Effects from single-year measures of family income, rather than the two-year average we employ, yield income effects only slightly smaller than those reported above.

An additional and important point about measurement error is that it should render the family income measure a poor predictor of other outcomes besides schooling. Using

It is important to notice that family income plays a more important role in earlier schooling transitions compared to its effect on later ones. This evidence suggests that it is family income at earlier ages and not later ones that matters in explaining college attendance. Its effect, however, is on college readiness (high school completion), and not directly on college attendance for high school graduates.²⁸

F. Effects of College Tuition

Table 6 considers the effects of a rise in both two-year and four-year gross tuition costs by \$1,000 on two- and four-year college entry for high school graduates of each racial-ethnic group. We estimate three separate specifications: (1) a model estimated using year effects alone (no family background or AFQT effects), (2) a model that adds family background and other variables (the baseline specification used in this paper), and (3) a model that adds AFQT to specification 2. All estimates are computed using the entire dynamic model, which controls for unobserved heterogeneity; each model is estimated separately for each ethnic/racial group. Rows 1 and 2 of each panel show both own and cross effects of a rise in tuition: the effect of two-year tuition on two-year entry (own effect) and four-year entry (cross effect) and vice versa. Row 3 of each panel shows the impact of a rise in both two- and four-year tuition of two-year entry, four-year entry, and combined entry.²⁹

Comparing estimates in panels A and B reveals that controlling for family background weakens estimated tuition effects, sometimes substantially; when panels B and C are compared, AFQT and family background operate in a similar fashion. Two-year tuition effects are large and statistically significant, though a substantial portion of the decline in two-year attendance as two-year tuition rises is attributable to individuals who switch to a four-year college instead. In specifications with and without AFQT included as a regressor, four-year tuition effects are inconsequential and sometimes perversely positive, though statistically insignificant.

Nonetheless, owing to large two-year effects, the combined effects of a rise in both two- and four-year tuition are substantial. However, given

an NLSY extract comparable to the one used for this paper, Cameron and Taber (1999) report effects of family income on schooling similar to those discussed here but find a large and statistically powerful relationship between family income (measured during adolescence) and hourly wages measured between 10 and 15 years after schooling is completed.

²⁸ This finding is consistent with the recent evidence of Duncan et al. (1998) and work by Levy and Duncan (2000).

²⁹ The estimates of a tuition rise on combined enrollment shown in panel B are close to the median estimate (after an inflation adjustment) presented by Leslie and Brinkman (1988) in their survey of over 20 studies of the effect of tuition on college enrollment.

TABLE 6
OWN EFFECTS AND CROSS EFFECTS OF A \$1,000 RISE IN TWO-YEAR TUITION, FOUR-YEAR TUITION, AND BOTH ON ENROLLMENTS AT TWO-YEAR AND FOUR-YEAR COLLEGES FOR HIGH SCHOOL COMPLETERS

	BLACK			HISPANIC			WHITE		
	Enrollment Change at			Enrollment Change at			Enrollment Change at		
	Two-Year (1)	Four-Year (2)	Combined (3)	Two-Year (4)	Four-Year (5)	Combined (6)	Two-Year (7)	Four-Year (8)	Combined (9)
A. Exclude Family Background, Exclude AFQT									
1. Two-year tuition rise	-.10* (.02)	.03* (.01)	-.07* (.02)	-.11* (.03)	.03* (.01)	-.08* (.03)	-.15* (.02)	.07* (.02)	-.09* (.02)
2. Four-year tuition rise	.01* (.01)	-.04* (.00)	-.03* (.01)	.01 (.02)	-.03 (.01)	-.02 (.02)	.01* (.01)	-.09* (.02)	-.08* (.02)
3. Rise in both	-.09* (.02)	-.01* (.01)	-.10* (.02)	-.10* (.02)	-.00 (.02)	-.10* (.03)	-.14* (.03)	-.02* (.01)	-.17* (.03)
B. Include Family Background, Exclude AFQT									
1. Two-year tuition rise	-.08* (.02)	.02 (.02)	-.06* (.02)	-.10* (.03)	.02 (.01)	-.08* (.03)	-.11* (.03)	.04* (.01)	-.06* (.02)
2. Four-year tuition rise	.01 (.01)	.01 (.01)	.02 (.01)	.01 (.01)	.01 (.01)	.02 (.01)	.02 (.01)	-.02 (.01)	-.00 (.01)
3. Rise in both	-.07* (.02)	.04* (.01)	-.03* (.02)	-.09* (.04)	.03 (.01)	-.06* (.03)	-.10* (.03)	.04* (.01)	-.06* (.03)
C. Include Family Background and AFQT									
1. Two-year tuition rise	-.08* (.02)	.04 (.02)	-.05* (.02)	-.10* (.04)	.02 (.01)	-.09* (.03)	-.10* (.02)	.05* (.01)	-.05* (.02)
2. Four-year tuition rise	.04 (.02)	-.01 (.02)	.03 (.02)	.01 (.01)	-.00 (.00)	.01 (.01)	-.01 (.01)	.01 (.01)	-.00 (.01)
3. Rise in both	-.05* (.02)	.03 (.02)	-.02 (.02)	-.09* (.04)	.02 (.01)	-.08* (.04)	-.11* (.02)	.06* (.02)	-.05* (.02)

NOTE.—High school completers are composed of GED recipients and high school graduates combined.

* Significant at the 10 percent level.

the pattern of smaller black responses to tuition, the recent rise in college tuition works to *reduce* black-white differences in college enrollment. The more negative effect of tuition on college enrollment for Hispanics accounts for only a small component of the difference between Hispanic and white college enrollment rates. For all groups, the estimated effect of the tuition increase operates mainly through its effect on attendance at community colleges, where there is greater sensitivity to tuition (row 3 of each panel). There is virtually no effect of an increase in college tuition on enrollment in four-year schools.³⁰

A notable exclusion from the discussion is the influence of financial gift aid (Pell grants) on college entry. For the results reported above, we measure tuition net of Pell grant eligibility. When tuition and Pell grant aid are disaggregated into separate measures, Pell grant eligibility has only a minor effect. This finding is consistent with the evidence reported in Kane (1994). A \$1,000 increase in Pell grant entitlements produces less than a 1 percent increase in enrollments as opposed to about a 6 percent response from a comparable change in tuition. Pell grants offset tuition costs for low-income people and should have effects of equal but opposite magnitude to tuition.

This difference in responses is consistent with evidence that many Pell grant-eligible individuals do not apply for the grant because they

³⁰ We also examine responsiveness to tuition increases by quartile of the family income distribution by adding a tuition-family income interaction into the model. While individuals in the top quartile are less responsive to tuition increases, we cannot reject the hypothesis that tuition effects are the same across income classes for any demographic group with conventional levels of statistical significance. We also cannot reject the hypothesis of equality of tuition effects across quartiles of the AFQT distribution. See panel D of table C-8 of App. C (available on request). Finally, the introduction of geographic dummy variables further weakens the tuition effects reported here (see Cameron and Heckman 1999*a*).

One interpretation of the pattern of price sensitivity seen here is that four-year schools draw from the population of school completers who invested more in academic training during high school. High school completers ill-prepared to undertake a four-year college curriculum are insensitive to four-year tuition changes since they are unlikely to attend or even be accepted by most four-year colleges. Two-year colleges are less stringent in their admission standards, and, as expected, two-year entrants have significantly lower AFQT scores than four-year entrants (Cameron and Heckman 1999*b*).

An alternative interpretation, suggested by Robert Topel, is that greater measured price sensitivity for community college attendance is due to measurement error attributable to three possible factors: (1) community colleges are more uniform in quality than four-year colleges, and by an errors in variables argument, the four-year price elasticity would be downward biased. (2) Community colleges have geographic districts, whereas four-year colleges do not. Thus the measurement of price is better. (3) Institutional scholarship aid based on income is more relevant for four-year schools, and because we cannot measure institutional scholarship aid, we do not measure the true net price. Our measure of net price adjusts only for Pell grant eligibility, and not institutional aid. Low-income individuals are more heavily subsidized, so our measure of price overstates the true price. Note, however, that the pattern of price sensitivity shown by the data is essentially the same across racial/ethnic groups, so for measurement error to be at least a partial explanation, it must operate in the same way for each group.

lack the necessary scholastic preparation for college. This further supports the argument that it is college readiness and not credit constraints that explains majority-minority shortfalls in educational attainment. Orfield (1992) and Kane (1995) speculate that poor people may not have reliable information about their own Pell grant eligibility. If so, family background factors play a substantial role in making people aware of their Pell grant benefits. However, it seems unlikely to us that children from poor families who have persevered through high school would be unaware of their eligibility for grants and loans. It seems more likely that able and college-ready students are aware of their eligibility for grants and act on the information. Indeed, Heckman, Smith, and Wittekind (1997) find that low-income high school graduates are much more aware of their eligibility for Pell grant programs than are high school dropouts.³¹

G. Supporting Evidence from Other Studies

Cameron and Heckman (1998) analyze the determinants of grade-by-grade schooling attainment for cohorts of American males born between 1908 and 1964. Consistent with the idea that family income and family background factors reflect long-run and not short-term influences on schooling attainment, they find that income and family background factors are powerful determinants of schooling continuation decisions from elementary school through graduate school. An appeal to borrowing constraints is not required to explain the relationship between family income and college attendance decisions for five cohorts of American males.

Cameron and Taber (1999) estimate the importance of borrowing constraints directly in a model that incorporates Becker's (1972) insight that both the costs of and returns to schooling will be influenced by borrowing constraints. Using a variety of methods, they find no evidence that borrowing constraints play a role in the schooling decisions of recent cohorts of American youths. Additional evidence on this point is presented by Altonji and Dunn (1996), who find no evidence that returns to education vary systematically with family income. Stanley (1999) examines the impact of the G.I. Bill on college-going decisions of Korean War veterans. Consistent with our story, he finds that the take-up of college subsidies was concentrated almost exclusively among

³¹ They report that the log odds ratio that a high school graduate with no college education is aware of the Pell grant program is twice as great as the log odds ratio for a high school dropout. The Orfield argument might be salvaged by claiming that persons who were unaware of Pell grants in high school failed to graduate because they were unaware of the grants, overestimated the costs of college, and failed to complete high school because of their underestimate of the true rate of return.

veterans from families in the top half of the socioeconomic distribution.³²

IV. Summary and Conclusions

This paper examines the determinants of college entry and the sources of disparity in educational attainment between minorities and whites. The strong correlation between college attendance and family income is widely interpreted as evidence that short-term borrowing constraints impede enrollment. We argue that the importance of short-term credit constraints is greatly exaggerated. It is the long-term influence of family income and family background as captured by our measure of ability, or equivalently by parental education, that best explains the correlation. Family income matters, but it has its greatest influence on forming the ability and college readiness of children and not in financing college education. Family income is more important in explaining earlier grade transitions than later ones, suggesting that money spent on tuition policy aimed at high school graduates does not target the right population.

We examine racial-ethnic differences in schooling. We find that when we control for family background, minorities are more likely than whites to graduate from high school and attend college. Again, it is long-term factors that mainly account for this relationship, not short-term cash constraints that can easily be fixed by Pell grants or other transfer programs offered to children late in their life cycle of adolescent development. It is early differences in resources and not later ones that matter more.³³ Tuition and opportunities in unskilled labor markets play only a minor role in accounting for majority-minority differences in college enrollment.

We raise a number of questions about the empirical and intellectual foundations of current government income subsidy programs designed to promote college attendance. The edifice in place is currently very generous to minorities and to children from poor families. A main conclusion from our work is that to raise college attendance and success in college, policy should focus on ensuring that more students graduate from high school and obtain the skills and motivation required to perform successful college work. Government policies such as Pell grants and other tuition subsidies focus on getting high school graduates into college, but our evidence suggests that the scope for such policies is

³² Evidence presented by Eckstein and Wolpin (1999) on determinants of high school graduation is in general agreement with our evidence. Their "types" are essentially the discrete factors introduced in Cameron and Heckman (1989) and used in this paper. They find that these proxies for long-run factors are the major determinants of high school graduation.

³³ See the evidence summarized in Heckman (2000).

minimal because most of the problem of disparity in schooling attainment among racial, ethnic, and income groups arises at earlier points in the life cycle of children from poor families.

Appendix A

Data and Sample Inclusion Criteria

This Appendix presents our sample inclusion criteria and the way we construct the choice sets facing persons. The measurement of family income and other family background variables, local tuition, college proximity, local labor market, and the AFQT is documented.

A. Background on the NLSY Data and Sample Inclusion Criteria

The micro data we use are taken from the 1979–91 waves of the National Longitudinal Survey of Youth. The NLSY includes a randomly chosen sample of 6,111 U.S. youths and a supplemental sample that includes 5,296 black, Hispanic, and nonblack, non-Hispanic economically disadvantaged young people. Interviewees have been surveyed annually beginning in 1979. Our sample is restricted to males in the random or the black and Hispanic supplemental samples. These samples are statistically representative of their populations.

We analyze only individuals aged 13–16 in January 1978, when monthly school attendance records commence. Approximately 6–8 percent of individuals of each sample were excluded from the analysis for one of four reasons. First, individuals not attending school full-time at age 15 were excluded (0.2 percent of whites, 0.8 percent of blacks, and 2.2 percent of Hispanics). Second, we eliminate approximately 1 percent of individuals of each sample reported having completed grade 11 or higher at age 15. Third, about 3 percent of each sample is dropped because schooling records are seriously incomplete. Finally, another 3–5 percent of individuals of each sample missed more than one interview between their initial 1979 interview and their age 21 interview (which occurs between 1982 and 1986) and were excluded to ensure complete records on school attendance during crucial schooling years. About 2 percent of individuals who were included in each initial sample exited from the data sometime between ages 21 and 24.

These restrictions bring our sample sizes to 915 blacks, 686 Hispanics, and 1,417 whites. Summary statistics are calculated from these samples. For our multivariate analysis, approximately 13 percent of each sample was lost because of missing values in family income, AFQT, or another variable (see Sec. C below).

One final limitation is placed on the observations used in the multivariate analysis. In order to guarantee that AFQT test scores were not influenced by college attendance, a small fraction of the sample was dropped—those who took the test after completing high school. The Armed Services Vocational Aptitude Battery (ASVAB), from which the AFQT is derived (see below), was administered in the summer of 1980. Individuals who were in the oldest cohort of our sample and who were also in grade 10 at age 15 would have been eligible to enter college in the fall of 1979 if they attended school without interruption. About 3 percent of each sample is dropped after age 17 to ensure that AFQT is measured prior to college attendance. The oldest cohort of the NLSY data comprises roughly 30 percent of each sample, and approximately 10 percent of that cohort

is in grade 10 at age 15. The remaining AFQT scores are age-adjusted (linear age effects are removed).

B. Data on Schooling Choices

Monthly school attendance is measured beginning in January 1978. Before then, information on schooling attendance is limited. From information on attendance spells, highest grade completed, diplomas, and type of college first attended, a continuous schooling history is constructed. Some data editing is performed to remove obvious inconsistencies.

Age is measured in mid October of each year to approximate age enrollment cutoffs for kindergarten. This convention also facilitates comparison of the NLSY to October CPS school enrollment supplements, which are used as a check on the NLSY data. Summary statistics from the two data sets are remarkably close. The school year is assumed to last from September 1 through May 31 of each year. A person who reports attending for at least one month of this period is considered a part-time attender. A dropout is classified as someone who does not attend at all.

C. Data on Family Background, Family Income, and AFQT

As noted in table 2, highest grade completed of a person's mother and father, number of living siblings, and whether the respondent came from a broken home (i.e., did not live with both biological parents) are measured at age 14. Less than 2 percent of the individuals in each sample have missing information on one of these variables. Information on parental income, which mainly came from parents, was not available for a person who was no longer considered to be dependent on his or her parents. A dependent is defined by the NLSY as a person living at home or not at home but living in a dorm or military barrack. A person living in his or her own apartment or house (even if at college) is deemed to be independent, and no steps are taken to gather parental family income. Thus family income is generally not known for older members of the NLSY, and we drop these people from our samples. For our samples, family income is missing in only about 6 percent of cases, and over 90 percent of the remaining cases have observations on family income from more than one interview. For the usable cases, a two-year average was constructed for family income at ages 15 and 16 if available. Family income at age 14 and age 17 is used if the data are missing at age 15 or 16. This occurs in 6 percent of the cases. The two-year average yields slightly larger and more precise estimated effects of family income in the statistical analyses than a one-year measure. Averaging income attenuates measurement errors. For analyses based on younger subsets of the data, three-year averages differ little from the two-year measure.

The AFQT score is a weighted sum of four tests (focusing on reading skills and numeracy) of the 10-part ASVAB. About 7 percent of our sample did not complete the battery, and so the AFQT score is not available for them. Altogether, missing data on the AFQT, family income, and other variables eliminate about 13 percent of the samples from the multivariate analyses.

D. Data on Local Labor Market Conditions

The NLSY Geocode data have annual unemployment rates of prime-age males in the county of residence. Local labor market conditions are measured at the county level. We supplement our analysis with county-level data from the Bureau of Economic Analysis containing annual measures of labor market conditions by industry category, such as average annual wages and employment. (For a description, see Bureau of Economic Analysis [1999].)

For secondary school transitions (through GED attainment), we use a measure of average earnings per job in the unskilled sector as the opportunity cost of schooling. Since the wage data are given by major industries and not occupations, we use the average earnings per job in the services and retail and wholesale trade industries to proxy what a high school dropout would earn. For analysis of the transition into college, the set of industries for which average opportunity wages are constructed is expanded to include manufacturing, construction, mining and extraction, and transportation and public utilities.

E. Tuition Data and Pell Grant Eligibility

Annual records on tuition, enrollment, and location of all public two- and four-year colleges in the United States were constructed from the Department of Education's annual Higher Education General Information Survey and Integrated Postsecondary Education Data System "Institutional Characteristics" surveys. By matching location with county of residence, we determined the presence of two-year and four-year colleges. Tuition measures are taken as enrollment-weighted averages of all public two- or four-year colleges in a person's county of residence (if available) or at the state level if no college is available (in which case, the indicator for college presence was set to zero). For some of the analysis, the amount of Pell grant funds for which a person is eligible is needed. These variables were imputed using parental family income and number of siblings using the appropriate annual formulas summarized in Mortenson (1988).

Appendix B

The Likelihood Function

Using the notation introduced in Section II, we write out the likelihood function for the model we estimate in this paper. We use the notation $d_{a,j,c}$ for the realized value of $D_{a,j,c}$. For the initial condition, this is abbreviated to $d_{a,c}$ and to $d_{\bar{a},c_{\bar{a}-1},c}$ for the final state. Associated with each choice set $C_{a,j}$, we have a probability over the destination states that can be accessed from initial state a, j , which we sometimes denote by a, j_a to recognize that the educational attainment state at age a is characterized by an age-specific set of origin states. We note that (a, j_a) is equivalent to (a, c_a) as defined in the text, where c_a is the choice made at age a . We may write the probability of the history

$$(D_{a,c} = d_{a,c}, D_{\bar{a}+1,c_{\bar{a}},c} = d_{\bar{a}+1,c_{\bar{a}},c}, \dots, D_{\bar{a},c_{\bar{a}-1},c} = d_{\bar{a},c_{\bar{a}-1},c})$$

conditional on $\mathbf{Z}_{\bar{a}}, \mathbf{Z}_{\bar{a}+1,c_{\bar{a}}}, \dots, \mathbf{Z}_{\bar{a},c_{\bar{a}-1}}$ and η as

$$\prod_{c \in C_{\underline{a}}} [\Pr(D_{\underline{a},c} = d_{\underline{a},c} | \mathbf{Z}_{\underline{a}}, \eta)]^{d_{\underline{a},c}} \cdot \prod_{c \in C_{\underline{a}+1, c_{\underline{a}}}} [\Pr(D_{\underline{a}+1, c_{\underline{a}}, c} = d_{\underline{a}+1, c_{\underline{a}}, c} | \mathbf{Z}_{\underline{a}+1}, \eta)]^{d_{\underline{a}+1, c_{\underline{a}}, c}} \cdots \prod_{c \in C_{\underline{a}, c_{\underline{a}-1}}} [\Pr(D_{\underline{a}, c_{\underline{a}-1}, c} = d_{\underline{a}, c_{\underline{a}-1}, c} | \mathbf{Z}_{\underline{a}, c_{\underline{a}-1}}, \eta)]^{d_{\underline{a}, c_{\underline{a}-1}, c}}, \quad (\text{B1})$$

where

$$\begin{aligned} c_{\underline{a}} &= \arg \max_c d_{\underline{a},c}, \quad c \in C_{\underline{a}}, \\ c_{\underline{a}+1} &= \arg \max_c d_{\underline{a}+1, c_{\underline{a}}, c}, \quad c \in C_{\underline{a}+1, c_{\underline{a}}}, \\ &\vdots \\ c_{\underline{a}} &= \arg \max_c d_{\underline{a}, c_{\underline{a}-1}, c}, \quad c \in C_{\underline{a}, c_{\underline{a}-1}}, \end{aligned}$$

where $\arg \max$ is applied to the final subscript of $d_{a,b}$ or $d_{a,b,c}$. This selects the element in each choice set that is chosen at each age because $d_{a,b} = 1$ if b is selected; $d_{a,b} = 0$ otherwise for each age and each choice set. The random effects likelihood integrates out η with respect to the distribution $F(\eta)$. The functional forms for the probabilities are given by equation (3) for the transition probability and by equation (4) for the initial condition.

Following Heckman and Singer (1984), we approximate $F(\eta)$ by a discrete distribution with mass points:

$$(P_i, \eta_i)_{i=1}^I, \quad (\text{B2})$$

where $P_i \geq 0$ is the probability associated with the mass point η_i , and $\sum_{i=1}^I P_i = 1$. This is the representation for the nonparametric maximum likelihood estimator of $F(\eta)$ (Lindsay 1995), and $I \rightarrow \infty$ as the sample size (N) gets large ($N \rightarrow \infty$). Chen et al. (1998) present sufficient conditions for the nonparametric maximum likelihood estimator to produce \sqrt{N} consistent estimates of the parametric portion of the model. Alternatively, we may assume that the true model for F is a finite mixture (I is fixed and bounded $I \leq \bar{I}$). Under the latter assumption, we can produce conventional ($N^{1/2}$) errors for the estimated parameters using the information matrix. Heckman and Taber (1994), Chen et al. (1998), Aakvik, Heckman, and Vytalil (1999), and Cameron and Taber (1999) present identification results for this class of models. Cameron and Heckman (1989) introduced a general framework for econometric models with discrete factor structures. Eckstein and Wolpin (1999) apply these methods to study the high school graduation and dropout decision.

Following the suggestions in Heckman (1981), we can estimate the model recursively determining (B2) off the probability for the initial conditions. Alternatively, we can estimate the model jointly for initial conditions and subsequent transitions. In this paper, after considerable experimentation, we find that $I = 2$ describes the data. This low dimensionality has been found in many studies of mixture models (see, e.g., Heckman and Singer 1984; Heckman and Walker 1990). Setting $\eta_1 = 0$ and $\eta_2 = 1$, we estimate P_1 (and $P_2 = 1 - P_1$). To obtain a prespecified variance for η , we multiply by constant k . We pick k so that $\text{Var}(\eta) = 1$, a normalization needed to identify the factor structure and slope coefficients. For each choice probability structure associated with the choice set C_{a,j_a} , we need to normalize one $\beta_{a,j_a,c}$ to some value to identify the remaining parameters. Let c_a^* be the normalization choice; then β_{a,j_a,c^*} and α_{a,j_a,c^*} are constrained to zero within each choice set. This is equivalent to defining all coef-

ficients relative to those for state c^* . However, ν_{a,c^*} is assumed to be Weibull. Specific normalizations used in the empirical work in this paper are presented in table C-10 in Appendix C (available on request).

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