Estimation of Educational Borrowing Constraints using Returns to Schooling

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March 4, 2003

[1]For helpful comments we thank Joe Altonji, John Cochrane, Tim Conley, Steve Levitt, Lance Lochner, Bruce Meyer, Larry Kenny, Craig Olson, two anonymous referees, and seminar participants at Brigham Young, the Federal Reserve Bank of New York, MIT, Northwestern, Princeton, Rochester, Florida, and North Carolina. We thank Tricia Gladden for superior research assistance and thoughtful comments. We also thank Jeff Kling for providing us with his code. For financial support, Taber acknowledges NSF Grant SBR-97-09-873, and Cameron acknowledges NSF Grant SBR-97-30-657.

Abstract

This paper measures the importance of borrowing constraints on education decisions. Empirical identification of borrowing constraints is secured from the economic prediction that opportunity costs and direct costs of schooling affect borrowing constrained and unconstrained persons differently. Direct costs need to be financed while in school which is when credit constraints bind most strongly. As a result they impose a relatively larger burden on credit constrained students. By contrast, gross forgone earnings do not have this feature. We explore the implications of this idea using least-squares regressions of schooling attainment, instrumental variables wage regressions, and two structural economic models that integrate schooling choices and their returns into a unified framework. None of the methods produces evidence that borrowing constraints generate inefficiencies in the market for schooling in the current policy environment. We conclude that, on the margin, additional policies aimed at improving credit access will have little impact on schooling attainment.

1 Introduction

Does access to credit influence educational outcomes? The answer to this question is fundamental to sensible educational policy design and to our understanding of many economic phenomena. However, as the data to answer this question directly is not available, we are forced to use alternative approaches. The key idea for identification of credit constraints in this paper is that opportunity costs of schooling (i.e. the value of the time students spend learning) and direct costs of schooling (the monetary costs) influence schooling choices differently for credit-constrained and unconstrained individuals. Direct costs need to be financed while in school which is when credit constraints bind most strongly. As a result they impose a relatively larger burden on credit constrained students. By contrast, gross forgone earnings do not have this feature and thus do place a higher burden on credit-restricted persons.[1]

Our approach requires two exclusion restrictions, one of which is a measure of direct costs and the other is a measure of opportunity costs. We follow Card (1995b) by using an indicator variable for the presence of any college (either a two- or four-year college) in an individual's county of residence as a proxy for direct costs of schooling. As a proxy for foregone earnings during schooling, we use measures of average earnings in low-skill industries in the county of residence during high school. We take four different approaches to studying the data.

(1) The first approach follows the logic of Lang (1993) and Card (1995a) using instrumental variables. Since the schooling decisions of borrowing constrained students are relatively more sensitive to changes in direct costs of schooling, the group that attends college in response to a fall in direct college costs will contain a relatively higher concentration of credit-disadvantaged students. These students also require a higher return to college. Since two staged least squares tends to identify the effects of schooling on earnings for individuals who are most strongly affected by the instrument, one would expect the schooling coefficient to be relatively high when schooling is instrumented with direct costs.

[1]By gross foregone earnings we mean income that would have been earned if the student were not attending school. We abstract from earnings that students earned while in college. If we included those earnings in the model they would enter the model in exactly the same way as direct costs of schooling, but with the opposite sign.

Using the language of Lang(1993) the "discount rate bias" would be high in this case. By contrast, when we instrument using opportunity costs there should be a relatively lower concentration of credit disadvantaged students and thus a smaller "discount rate bias." Because estimates using instruments for either direct or opportunity costs are presumably free of ability bias, the presence of borrowing constraints implies that the IV estimator that uses the direct costs of schooling instrument yields estimates larger than those obtained with the forgone earnings instrument. In practice, we find that this approach provides no evidence of educational borrowing constraints. The point estimates using two staged least squares estimates go in the opposite direction as would be predicted if constraints are important.

(2) The main implication of our model is that students who are credit-disadvantaged should respond more strongly to direct costs than other students. While we have no direct evidence on who is borrowing constrained, we can find many proxy variables that we think may be correlated with borrowing constraints. Our second approach is to estimate models of schooling attainment and look for evidence of borrowing constraints using interactions between schooling costs and observables that are likely to be related to credit constraints. These interactions are often of the wrong sign and consistently insignificant, so this method shows no evidence of credit constraints.

(3) We next develop an estimable version of the economic model. Both the interest rate at which students borrow to finance their education and the return to schooling are assumed to vary across individuals in this structural model. In our third approach, heterogeneity in borrowing rates is assumed to depend on individual characteristics observed in the data as in method (2). We estimate the borrowing rate faced by individuals with different observable characteristics. This method provides the strongest evidence against the importance of borrowing constraints for educational decisions. Our estimates of the extent of borrowing constraints are precisely estimated and are shown to be small.

(4) Finally we generalize the structural model to allow for unobserved heterogeneity in educational borrowing rates and returns. Much like the IV approach, identification is secured from the use of direct costs and indirect costs as exclusion restrictions. We show formally that these two types of exclusion restrictions enter the model in different ways which allows us to nonparametrically identify the extent of borrowing constraints.

We then estimate the distribution of borrowing constraints using this model. Our point estimates lie on a corner in which no individuals are borrowing constrained.

Thus our results are consistent across all four approaches in that none find evidence that borrowing constraints are important for schooling decisions.

It is important to underscore the limits of our study. First, we cannot directly observe credit access for individuals. Our identification of the extent of borrowing constraints is indirect. Second, we cannot answer whether all students in all time periods have had adequate access to credit for educational investments. During the period covered by our data, large subsidies to school and college were already in place in the United States. Given the policy regime, we find no evidence of inefficiencies in the schooling market resulting from borrowing constraints. This finding suggests additional subsidies aimed at improving credit access will have little impact on overall schooling attainment.

The paper unfolds as follows. Section 2 provides a review of the literature and Section 3 presents the economic model and frames the discussion of borrowing constraints in the context of the model. Section 4 describes the data. Section 5 presents empirical evidence on the question of borrowing constraints from linear regression and instrumental variable estimates of returns to education and analyses of schooling attainment. Section 6 outlines the structural models and reports empirical findings from the models. The paper concludes with a summary.

2 Previous Work

Evidence favoring the idea that borrowing constraints hinder educational progression, particularly at the college level, is based almost exclusively on the well-documented correlation between schooling attainment and family income or other family characteristics. The step from correlation to causation is precarious, however, as family income is also strongly correlated with completion of elementary and high school and early scholastic test performance that predict eventual college entry.

Recent work by Cameron and Heckman (1998, 2001), Keane and Wolpin (2001), and Shea (2000) has attempted to better understand the determinants of schooling choices. Using very different empirical approaches, these researchers find little evidence that borrowing constraints hamper college-going or any other schooling decision.

The credit constraint question has also surfaced in the recent literature on returns to education. This literature has aimed at recovering returns to schooling from wage regressions purged of "ability bias." Unobserved ability is thought to bias least-squares estimates of returns to schooling upward. Using instrumental variables methods to correct for the bias, researchers have devised a wide variety of different instruments and typically find instrumental variables estimates anomalously larger than least-squares estimates (Card 1999, 2001). The connection between credit access and measured returns to schooling–a link originally clarified by Becker (1975)–has been investigated by Lang (1993) and Card (1995a) as an explanation for these high instrumental variables estimates. Their argument presumes borrowing constraints are important for schooling decisions.[2]

Few empirical studies have integrated educational attainment and schooling returns into a unified framework. An important exception is the pioneering work of Willis and Rosen (1979), upon which our paper builds and is discussed at the beginning of Section 3. Most research relevant to the question of educational financing constraints comes from studies of the determinants of schooling. The literature on returns to schooling has evolved largely in isolation. Both literatures are discussed below.[3]

2.1 Literature on Determinants of Educational Attainment

A ubiquitous empirical regularity that emerges from the literature on determinants of schooling is the strong correlation between family income and schooling attainment. This correlation has been documented in legions of U.S. data sets covering the entire 20th century (see e.g. Mare [1980], Manski and Wise [1983], Hauser [1993], Manski [1993], Kane [1994], Mayer [1997], Cameron and Heckman [1998, 2001], and Levy and Duncan [2000]) and in data from dozens of other countries in many stages of political and economic development (see the studies collected in Blossfeld and Shavit, 1993, for instance).[4] Educational

[2] We use the terms "borrowing constraint" and "credit constraint" synonymously and broadly to include not only a hard constraint, which prevents any borrowing outside of the family, but also interest rates for educational borrowing that are higher than market interest rates. Definitions are clarified in Section 3.

[3] Lochner and Monge (2001) also consider the relationship between borrowing constraints and credit constraints. They present a schooling model in which credit constraints are endogenous.

[4] Tomes (1981) and Mulligan (1997) are related but look at the elasticity of schooling to income. They find higher elasticities for families who are more likely to be borrowing constrained.

financing constraints has been the popular behavioral interpretation of the schooling-family income correlation, particularly in studies of college attendance.

However, credit access is only one of many possible interpretations of this correlation. Family income and other family background measures have been found to be correlated with achievement test performance in elementary and secondary school as well as with schooling continuation choices at all levels of schooling from eighth grade through graduate school. Cameron and Heckman (1998, 2001) adopt a 'life-cycle' view of the importance of family income and other family factors and argue that family income is a prime determinant of the string of early schooling decisions. They conclude that the measured effect of family income on college continuation is largely a proxy for its influence on early achievement.[5]

Shea's (2000) findings support this interpretation of the data. Shea isolates the component of family income variation that could arguably be ascribed to "luck," coming from union status, job loss, and other factors, to estimate the causal effect of income on schooling. He finds little or no correlation between this component of income and children's schooling outcomes.[6] Keane and Wolpin (2001) take a different approach. They estimate a rich discrete dynamic programming model of schooling, work, and savings. Model simulations reveal that relaxing borrowing constraints have almost no effect on schooling but are important determinants of working during school.

2.2 Returns to Education Literature

A large literature in labor economics has been concerned with estimating the causal effect of schooling on earnings. Ordinary least squares regressions of earnings on schooling have long been believed to be biased upward as a result of "ability bias:" individuals who attain higher levels of schooling do so in part because they are smart and earn a return on that characteristic as well as on their additional years of education. Empirical evidence for this idea has been found in virtually every data set with pre-labor-market measures of

[5]Carneiro, Heckman, and Manoli (2002) extend this work using similar methods but different outcomes. They find evidence that suggests that borrowing constraints may effect college completion, college quality, and delay entrance.

[6]Shea studies extracts from the Panel Study of Income Dynamics, and finds no effects in the full sample. He does find modest evidence of a relationship in the low-income subsample, though such a finding is not inconsistent with Cameron and Heckman's (2001) view that family income effects operate at the earliest stages of schooling.

scholastic ability, such as standardized test scores. Including test scores in wage regressions leads to a decline in the estimated effect of schooling. Nevertheless, scholastic test scores are imperfect measures of earning ability and leave substantial scope for bias from other unobserved components of ability.

A good deal of recent work uses instrumental variables or related techniques to address the problems caused by omitted ability measures.[7] Card (2001) provides an extensive survey of this literature. Contrary to the intuition provided by the ability bias story, Card documents that researchers often find that the estimated coefficient on schooling rises rather than falls when instrumental procedures are used. Building on Becker's (1975) Woytinsky model, Lang (1993) and Card (1995a) explore heterogeneity in borrowing rates as an explanation for this pattern, which Lang terms "discount-rate bias." In Becker's model, a student invests in schooling until her return is equal to the interest rate she faces. If borrowing constrained individuals face higher personal interest rates, they will demand higher returns from schooling at the margin. If returns to schooling vary across individuals because of differential credit costs, the pattern of estimates produced by IV estimators may be explained by the fact that many of these estimators identify the causal effect of schooling from the subset of the population that is borrowing-constrained and who receive returns to schooling at the margin that are higher than the population average return. If the returns to schooling were homogeneous, IV would yield consistent estimates of the causal effect of education for the population. If returns are heterogenous, IV estimates must be interpreted with care. Imbens and Angrist (1994) show that instrumental variables estimates measure the treatment effect of schooling (that is, the causal effect) for groups whose schooling decisions are most sensitive to changes in the instrument used in estimation. For instance, schooling choices of borrowing constrained individuals may be most sensitive to changes in college tuition. Because of their higher costs of raising funds for schooling, borrowing constrained individuals also demand the highest returns to continue. Thus, the IV estimate of schooling returns will be an average of returns for this group and will be higher than the true population average return.[8] This argument also helps explain patterns of estimates

[7]Altonji and Dunn (1996) use a somewhat different strategy that is relevant to our study. They look for interactions between the return to schooling and family background. Their results are mixed, but some specifications point to a positive interaction.

[8]Heckman and Vytlacil (1998) present a more complete description of the econometrics behind Card's

obtained from studies of "selection" models of schooling. Selection models take into account ability bias but discount rate bias should not appear. These studies generally report lower estimated returns to education than from OLS (see e.g. Willis and Rosen, 1979, and Taber, 2001).

3 The Model

An economic description of schooling choice is developed in this section. We use the model to show that direct costs of schooling and opportunity costs affect schooling choices in different ways for borrowing constrained students. This difference is essential for identification in all of the empirical approaches that follow. The closest antecedent of our work is Willis and Rosen (1979). They integrate future returns to education into their analysis of schooling decisions to account for self-selection into college attendance. Our framework extends that work in two important respects. First, Willis and Rosen study only the college-entry decision among a sample of high school graduates. They condition on all schooling decisions made before high school graduation. In our empirical work we restrict the analysis to (1) the decision to complete high school, (2) the decision to enter college, and (3) the decision to complete college. Our analysis avoids the sample selection problem in Willis and Rosen's work that arises by conditioning on high school graduation.

Second, and more importantly, Willis and Rosen estimate a first-order linear approximation of their economic model. By so doing, they are unable to disentangle the influences of direct and indirect costs of college-going. Separating these influences is the key to identifying and obtaining estimates of the quantitative importance of borrowing constraints.

It should be pointed out that the results we prove in this section can be shown in a more general framework. To avoid inconsistencies across sections we have tried to keep the particular specification in this section as close as possible to the structural model we will estimate below. The main results are still true in more elegant and general forms of the model.[9]

(1995a) model. Angrist and Krueger (2000) also embody the idea of discount rate bias into their econometric framework.

[9]The key result depends on the fact that for borrowing constrained individuals the marginal utility of income is higher while in school than out of school. The basic result would go through if we modelled constraints using hard constraints rather than a separate rate during school or more generally if the

Our model begins with a specification of individual preferences. Individuals derive utility from consumption and tastes for nonpecuniary aspects of schooling. These nonpecuniary tastes could represent either the utility or disutility from school itself or as preferences for the menu of jobs available to individuals at different levels of schooling. Assuming agents have power utility over consumption in each period, lifetime utility for a given level of schooling S is given by

$$V_S = \sum_{t=0}^{\infty} \delta^t \frac{c_t^\gamma}{\gamma} + T(S), \tag{1}$$

where c_t is consumption at time t, $T(S)$ represents tastes for schooling level S, δ is the subjective rate of time preference, and γ is a parameter of utility curvature with a value in $(-\infty, 1)$. Defining the set of possible schooling choices by $\mathcal{S}$, individuals choose S out of this set so that

$$S = \arg\max \{V_S \mid S \in \mathcal{S}\}. \tag{2}$$

Much of the schooling literature–including Becker (1975), Rosen (1977), Willis and Rosen (1979), Willis (1986), Lang (1993) and Card (1995a)–models heterogeneity in credit access by a person-specific rate of interest r at which a person can borrow and save throughout life. A credit constrained person is thought to be an individual who faces a high r, which makes educational financing relatively costly.

This approach to modelling credit cost heterogeneity has the unattractive feature that a high person-specific r implies high returns to savings after labor market entry. This leads to implications of the model that seem inconsistent with credit constraints. For example, in this standard model giving assets to borrowing constrained students prior to college would not affect their schooling decisions.[10] We adopt the simple but novel assumption that individuals borrow at their personal rate r while in school but face a common market interest rate for all borrowing and lending after labor market entry. Confining borrowing-rate het-

borrowing rate is an increasing function of the amount borrowed.

[10] Assuming a single r prevails throughout a person's lifetime gives rise to a separation result that simplifies the model. Given r, individuals choose schooling to maximize the present value of lifetime earnings. Thus, holding the interest-rate effect fixed and ignoring nonpecuniary tastes, neither family income nor income transfers from parents to children have any direct effect on schooling choices. In our setup, an increase in income transfers to children from parents influences schooling choices by raising the value of further schooling.

erogeneity to the schooling years is a natural assumption if one considers the borrowing rate to be determined by the ability to collateralize loans with personal or family assets during school. The specific form of borrowing constraints is not essential to the results in this section, but since we do not have data on consumption or assets we focus on a simple type of borrowing constraint that is straight forward to estimate in our structural model.

Define $R = 1 + r$ to be the borrowing rate during school and let r_m denote the market interest rate, which is normalized for convenience such that $(1 + r_m)^{-1} = \delta$. Students maximize utility subject to the lifetime budget constraint

$$\sum_{t=0}^{S-1} \left(\frac{1}{R}\right)^t c_t + \left(\frac{1}{R}\right)^S \sum_{t=S}^{\infty} \delta^{t-S} c_t \leq I_S, \tag{3}$$

where S is total years of school and I_S is the present value of income net of direct schooling costs. The first-order conditions are,

$$\begin{aligned} c_t &= (\delta R)^{\frac{t}{1-\gamma}} c_0 \qquad t \leq S, \\ c_t &= (\delta R)^{\frac{S}{1-\gamma}} c_0 \qquad t > S. \end{aligned}$$

Plugging these values into the budget constraint yields

$$I_S = \sum_{t=0}^{S-1} R^{\frac{t\gamma}{1-\gamma}} \delta^{\frac{t}{1-\gamma}} c_0 + (R\delta)^{\frac{S\gamma}{1-\gamma}} \sum_{t=S}^{\infty} \delta^t c_0. \tag{4}$$

Finally, solving c_t in terms of I_S and inserting the value into the utility function leaves us with the following expression for lifetime utility of a person choosing S years of school,

$$V_S = \frac{I_S^\gamma \left(\sum_{t=0}^{S-1} R^{\frac{t\gamma}{1-\gamma}} \delta^{\frac{t}{1-\gamma}} + (R\delta)^{\frac{S\gamma}{1-\gamma}} \sum_{t=S}^{\infty} \delta^t\right)^{1-\gamma}}{\gamma} + T(S). \tag{5}$$

Equation (5) represents an indirect lifetime utility function conditional on the choice variable S.

We next solve for the present value of income. To focus on borrowing constraints, we abstract from earnings uncertainty by assuming that earnings streams associated with all levels of S are known with certainty at time 0.[11] Let w_{St} be earnings at time t for an

[11] Uncertainty in future returns to education introduces an option value to further education. For instance, even when predicted returns to college are low, individuals may still graduate college in case realized returns rebound sometime in the future. See Taber (2001).

individual with S years of schooling. Individuals have zero earnings while in school and pay tuition τ_S at time $S-1$ to attend schooling level S. Abstracting from labor supply, we have the following expression for the present value of income discounted to time $t=0$:

$$
\begin{aligned}
I_S &= \left(\frac{1}{R}\right)^S \sum_{t=S}^{T} \delta^{t-S} w_{St} - \sum_{t=0}^{S-1} \left(\frac{1}{R}\right)^t \tau_{t+1} \\
&= \left(\frac{1}{R}\right)^S W_S - \sum_{t=0}^{S-1} \left(\frac{1}{R}\right)^t \tau_{t+1},
\end{aligned} \tag{6}
$$

where W_S is the present value of earnings associated with schooling level S discounted to time S. An essential part of the model is the two separate costs of schooling. The first is the direct cost which is captured by τ_{t+1}. The second is the time cost or opportunity cost which is captured by the fact that students do not enter the labor market until time S so that earnings are discounted by $\left(\frac{1}{R}\right)^S$.

To illustrate the model's main implications, consider how changes in direct costs and opportunity costs affect utility from consumption and their returns in a simple world with only two schooling levels, $S=0$ and $S=1$. Let τ_1 be the direct cost of $S=1$ and assume that there are no direct costs associated with $S=0$. Lifetime utility values for $S=0$ and $S=1$ are given by

$$
V_0 = \frac{W_0^{\gamma}\left(\frac{1}{1-\delta}\right)^{1-\gamma}}{\gamma} + T(0) \tag{7}
$$

$$
V_1 = \frac{(W_1/R - \tau_1)^{\gamma}\left(1 + (R\delta)^{\frac{\gamma}{1-\gamma}}\frac{\delta}{1-\delta}\right)^{1-\gamma}}{\gamma} + T(1). \tag{8}
$$

A person chooses $S=1$ when $V_1 - V_0 > 0$ and $S=0$ otherwise. In this model, the direct cost of schooling is represented by τ_1 and the foregone earnings is represented by W_0.

Notice that

$$
\Pr(S=1 \mid W_1, W_0, R, \tau_1) = \Pr\left(V_1 > V_0 \mid W_1, W_0, R, \tau_1\right) \tag{9}
$$

$$
= \Pr\left(D > T(0) - T(1) \mid W_1, W_0, R, \tau_1\right) \tag{10}
$$

where

$$
D = \frac{(W_1/R - \tau_1)^{\gamma}\left(1 + (R\delta)^{\frac{\gamma}{1-\gamma}}\frac{\delta}{1-\delta}\right)^{1-\gamma}}{\gamma} - \frac{W_0^{\gamma}\left(\frac{1}{1-\delta}\right)^{1-\gamma}}{\gamma}. \tag{11}
$$

Given this simple relationship between D and schooling attendance we will focus our attention on the effects of direct and opportunity costs of schooling (W_0 and τ_1) on D rather than the probability of attending college itself.

Consider two individuals with identical preferences (and for simplicity assume that $T(1) = T(0) = 0$), one of whom is borrowing constrained and one of whom is not. Suppose they are both indifferent between attending and not attending school ($V_0 = V_1$). One person borrows at the market rate $R = 1/\delta$; the other is credit constrained and borrows at $R > 1/\delta$. Next compare each person's reaction at time zero to a dollar increase in the present discounted value of foregone earnings, W_0, and alternatively to a dollar increase in direct schooling costs, τ_1. For the person borrowing at the market rate, a dollar is a dollar. To see this from equations (7) and (8), one can see that $V_0 = V_1$ implies the present value of income from either schooling choice is equal at time 0, $W_0 = W_1\delta - \tau_1$. Hence, a dollar rise in W_0 and a dollar rise in τ_1 have the same effect on the relative value of $S = 1$:

$$\frac{\partial D}{\partial \tau_1} = \frac{-\gamma V_1}{(\delta W_1 - \tau_1)} = \frac{-\gamma V_0}{W_0} = \frac{\partial D}{\partial W_0}. \tag{12}$$

In contrast, for the credit constrained person to be indifferent, from equations (7) and (8) one can see that $W_0 > \frac{1}{R}W_1 - \tau_1$. A dollar rise in direct costs of school has a larger (in absolute value) influence on the value of schooling than a dollar drop in foregone earnings:

$$\frac{\partial V_1}{\partial \tau_1} = \frac{-\gamma V_1}{\left(\frac{1}{R}W_1 - \tau_1\right)} < \frac{-\gamma V_0}{W_0} = \frac{\partial V_0}{\partial W_0}. \tag{13}$$

Put differently, the shadow value of a dollar of income is higher during school than after.

Our three main implications of the model can be stated more precisely.

Note first that a rise in R reduces the likelihood a person chooses $S = 1$ as long as she is not a net saver while in school:[12]

$$\frac{\partial D}{\partial R} = \frac{\partial V_1}{\partial R} = -\frac{\gamma V_1}{R I_1}(c_0 + \tau_1) < 0, \tag{14}$$

where c_0 is optimal time 0 consumption.

Implication 1. *A dollar rise in τ_1 diminishes the value of V_1 more for the individual with higher R as long as that person is not a net saver during school:*

$$\frac{\partial^2 D}{\partial R \partial \tau_1} = -\frac{\gamma V_1}{R I_1^2}[(c_0 + \tau_1)(1-\gamma) + c_0(R\delta)^{\frac{\gamma}{1-\gamma}}\frac{\delta}{1-\delta}] < 0. \tag{15}$$

[12] This is generally true as long as $c_0 > -\tau_1$ and must hold when schooling costs are positive.

Implication 2. *The influence of W_0 on D does not depend on R:*

$$\frac{\partial^2 D}{\partial R \partial W_0} = \frac{\partial W_0^{\gamma-1}\left(\frac{1}{1-\delta}\right)^{1-\gamma}}{\partial R} = 0. \tag{16}$$

Implication 3. *Consider two individuals who are indifferent about attending college and have the same values of utility conditional on college (i.e. V_1), but one faces a higher borrowing rate than the other. The return to education for the more constrained person is higher than it is for the less constrained person. That is, for individuals i and j with $V_{0i} = V_{1i} = V_{0j} = V_{1j}$, $R_i > R_j$ implies $W_{1i}/W_{0i} > W_{1j}/W_{0j}$.*

The last implication of the model follows directly from the fact that among the conditioning group, a person facing a high R must be compensated with a higher W_1 to remain indifferent. That is since $\partial V_1/\partial R < 0$ and $\partial V_1/\partial W_1 > 0$ if R_i is higher than R_j, then W_{1i} must be higher than W_{1j} for both individuals to be indifferent.

The basic economic idea embodied in these implications is used in all four empirical approaches below. The first two approaches use the intuition derived in this section to guide interpretation of the estimated parameters. The second two approaches estimate a version of the model and identification critically depends on the fact that W_0 and τ_1 enter the model in different ways.

4 The Data

Our analysis is based on Black, Hispanic, and White males from the 1979-1994 waves of the NLSY (National Longitudinal Survey of Youth). Because the NLSY collected detailed information on family background, scholastic achievement, labor market outcomes, county of residence, and school attendance and completion starting at relatively young ages, it is ideal for our study. The NLSY data are comprised of four distinct samples: a random sample of the population, a random sample of the Black and Hispanic populations, a sample drawn from the military, and a sample of the economically disadvantaged, non-Black non-Hispanic population.

We limit our sample in a number of ways. First, we exclude from our analysis the military and the non-Black non-Hispanic disadvantaged subsamples because they are not drawn according to exogenously determined characteristics.[13] Second, we use only males because their schooling and labor supply decisions are less complicated by fertility and labor market participation considerations. Third, because information about events occurring before January, 1978 is retrospective and limited, we confine our extract to males between ages 13 and 16 as of that date in order to have reliable information on schooling attendance, parental income, and county of residence. County of residence is used to construct measures of labor market conditions and measures of college proximity. Finally, another thirteen percent of the sample was eliminated because respondents did not complete the test battery (see below) or because of missing data in county of residence during high school (measured at age 17 when available) or family income or one of the family background variables. Final sample sizes are reported in Table 1.

We construct panel data from NLSY annual observations. Annual observations on each respondent begin no later than age 16 and extend through ages 29 to 33. For the analysis of wages, we excluded annual wage observations taken before age 22 because most college-graduates were still in school before that age.[14]

Summary statistics of the main variables used in the analysis are presented in Table 1. Panels A through C of the table show summary statistics of static variables for years of schooling completed, racial-ethnic identity, family background, and geographic location. Details of the variables are provided at the base of the table.

Panel D shows summary statistics for scholastic test score variables. The scores are from the ten-part Armed Services Vocational Aptitude Battery (ASVAB). The test was administered to NLSY respondents in the summer of 1980, when respondents in our sample were between ages 15 and 18. The set of four variables "Word Score," "Math Score," "Science Score", and "Automotive Score" are raw scores on four of the ASVAB sections. "AFQT Score" is the score on the Armed Forces Qualification Test, which is a weighted sum of scores of ASVAB tests related to literacy and basic mathematics. "AFQT Score" has been widely used in recent empirical work. The other four test scores are used together

[13]Thus, our data include all observations from the random sample and the random Black and Hispanic oversamples. The military and the non-Black non-Hispanic disadvantaged samples are small relative to the data we include. In addition, the military oversample was not followed after the 1984 wave of the NLSY.

[14]Starting the panel at an earlier age creates a data set unrepresentative of the population as it would contain more annual observations for high school dropouts and graduates than for college graduates. The estimates presented below are not sensitive to higher age cutoffs.

in the analysis below as an alternative to "AFQT Score." Taber (2001) contends these four scores represent a parsimonious set of wage predictors that capture more dimensions of ability than AFQT by itself.

Panel E of the table summarizes the instruments used below for endogenous schooling. "Local College" is a binary indicator for the presence of any college (either two-year or four-year) in the county of residence at age 17 (or age 16 for the handful who graduate high school by age 17).[15] College identifiers were merged to NLSY annual county of residence measures from the Department of Education's annual HEGIS and IPEDS "Institutional Characteristics" surveys, which contain annual data on location, type of institution, tuition, and other variables associated with all colleges in the U.S.[16]

Local labor market conditions were created from annual county-level labor market measures of annual earnings (not wages) in industries dominated by unskilled workers.[17] Our proxy for foregone earnings is a static variable created from this series taken in a person's county of residence at age 17 (see "Local Earnings at Age 17" in the table). The variable "Mean Local Earnings over Working Life" is constructed in the following manner. We condition on the county in which a student lived when they were 17. We then take the mean of the labor market measure for this county averaged over the years in which we have wage data on the individual from the NLSY sample. We also studied the local (county) unemployment rate in our schooling choice analyses reported below. The variable was never significant and the results are not reported.

Panel F shows means of annually varying variables used in the wage analysis. "Hourly Wage" is the wage at the current or most recently held job as of the interview date in each year. "Current Local Earnings" is an annual measure constructed from the local annual earnings series just discussed. It is measured for the county in which a person currently

[15]We explored a set of three indicator variables for two-year college, four-year college, and both. Our conclusions below were unchanged, though standard errors of estimates improved slightly in some cases when the three indicators were used together.

[16]A number of specialty colleges, generally with enrollments less than 100, and Federal institutions, such as the Naval Academy, were excluded.

[17]The data are taken from annual Bureau of Economic Analysis (BEA) data. Since the BEA data are reported by industry and not occupation, we use average earnings per job in service, agriculture, and the wholesale and retail trade industries. A number of other labor market measures using a variety of industry aggregates were explored, with little difference to the estimates presented below. This was apparently true because, except for government jobs, average wages across industries in a given county at a show a good deal of correlation with one another across time.

lives and is discussed further below. "Work Experience" is a measure of potential work experience and is constructed as age minus schooling minus six.

5 Regression and Instrumental Variable Results

5.1 Methodological Issues Behind Instrumental Variables

Sections 5.3 and 5.4 present instrumental variables estimates of log wage functions using proxies for both direct costs and opportunity costs as instrumental variables. To see the issues involved in the instrumental variables approach consider the following regression equation

$$\log(w_{it}) = \beta_0 + S_i\gamma_i + \ell_{it}\beta_1 + e_{it}\beta_2 + e_{it}^2\beta_3 + X_{it}'\beta_4 + u_{it}, \tag{17}$$

where for individual i at time t, w_{it} is the hourly wage, S_i is schooling, ℓ_{it} is a measure of the earnings in the county in which individual i lives at time t, e_{it} is experience, X_{it} represents other factors that affect wages, and u_{it} is an error term. The goal in the literature on returns to schooling is to estimate the coefficient on schooling, γ_i, which is interpreted as the causal effect of a year of school. Under some conditions it can be interpreted as the internal rate of return to schooling (see Willis, 1986). Two aspects of (17) complicate this analysis. The first is the much studied potential problem of ability bias that u_{it} may be correlated with S_i. The second complication is that we allow for a random coefficient on schooling, γ_i, that varies across members of the population. If γ_i were constant and u_{it} were orthogonal to S_i, OLS would produce a consistent estimate of the causal effect of a year of schooling on wages. We address these two problems sequentially.

First, abstract from the random coefficient aspect of the model by assuming that γ_i is just a constant γ. It is well known that one can obtain a consistent estimate of γ by two staged least squares using instruments that are correlated with S_i but uncorrelated with u_{it}. The model presented in section 3 contains two college costs which could potentially be used as instruments. The first is τ_1 in equation (6) which represents direct costs of schooling. As mentioned above, we follow Card (1995b) by using presence of a college in the county of residence at age 17 as a measure of the costs of college and assume it is uncorrelated with unobserved ability. For students from families with low and moderate incomes, the opportunity to live at home or have the parental residence close at hand while in college yields a substantial financial advantage. The National Longitudinal Survey of Youth (NLSY) data reveal that the probability of living at home while in college is about 55% for students with a college in their county, and only 34% for others. The second cost of college is the opportunity cost which is embodied in W_0 in equation (7). Earnings in the county in which a person lived at age 17 (see the variable "Local Earnings at Age 17" in Table 1) is a candidate instrument as it should be correlated with W_0 but uncorrelated with u_{it}. The big concern in using this as an instrument is that labor market variables at age 17 are almost certainly correlated with local labor market variables later in life and hence correlated with wages. To deal with this potential problem, it is essential that we include the time-varying measure of local earnings in the wage regression itself (ℓ_{it}). This is the "Current Local Earnings" variable summarized in Table 1. Under this specification, the crucial assumption justifying the instrument is that conditional on current local labor market conditions, local labor market conditions at age 17 are unrelated to the error term in (17).

Unfortunately, including ℓ_{it} in the wage function (17) leads to a new concern. Migration is potentially endogenous and related to schooling outcomes. In particular, college graduates tend to move more often than other groups and also tend to be relatively more likely to move to better local labor markets. As a result, ℓ_{it} is endogenous to schooling choices. Given the importance of controlling for this variable for the justification of local labor markets as an instrument, it is important to obtain a consistent estimate of β_1. This requires an additional instrumental variable that is correlated with ℓ_{it}, but not endogenous itself. The natural choice in this case is the local earnings at time t, but for the county in which person i lived at age 17. Since many individuals do not stray far from their residence at age 17, this instrument should be powerful. In addition, since it is determined by the county in which the student lives at age 17, it does not depend on moving after schooling completion and so is not endogenous.[18]

Another concern which is standard in the returns to schooling literature relates to the endogeneity of experience. Our experience variable is a measure of potential experience, and is equal to age minus schooling minus six. Since education is potentially endogenous

[18]In the tables, the local earnings rate (ℓ_{it}) at time t is called "Current Local Earnings" and the instrument is called "Current Local Earnings in Age 17 County."

and potential experience depends directly on schooling, it would be endogenous as well. To account for this problem, we instrument for experience and experience squared using age and age squared in some specifications.

The second nonstandard feature of (17) is that γ_i varies across individuals. If the return were constant at level γ, then both instruments would yield consistent estimates of γ (assuming they are valid). However, when γ_i is heterogeneous, two stage least squares estimates typically converge to different parameters when different instruments are used. Imbens and Angrist (1994) show that in this type of random coefficient model, IV recovers the average return to schooling for the subset of the population that is induced to change their level of schooling by the change in the instrument.[19] In our situation, the composition of the group induced to alter their schooling status depends on which instrument we use. In the theory section, we show that direct costs have a relatively larger effect on the schooling decisions of credit-disadvantaged students than do opportunity costs (implications 1 and 2). This means that when direct costs is used as the instrument, IV will put relatively more weight on borrowing constrained students than when opportunity costs is used. Implication 3 showed that credit disadvantaged students at the margin of whether to go to college require a higher return to schooling (higher γ_i). Putting these two arguments together, we expect IV estimates of the returns to schooling to be higher when schooling is instrumented with direct costs than when it is instrumented with opportunity costs. Using the language of Lang(1993), the "discount rate bias" will be higher when direct costs instrument schooling than when opportunity costs are used. The main goal of the instrumental variable approach is to test this implication of the model. Note that we have said nothing about OLS. Since OLS estimates contain ability bias while IV estimates do not, we can say nothing about the relationship between IV and OLS. However, we present OLS estimates below for comparison with the literature.

It is important to point out that while this argument may be intuitively appealing, it is not precise for a number of reasons. First, the actual estimated coefficient on schooling depends on the full joint distribution of schooling variables and regressors for individuals

[19]In the language of Imbens and Angrist (1994), instrumental variables estimators converge to the expected treatment effect for those individuals induced to change status by the change in the instrument. In our model the treatment is school and the treatment effect is the return to schooling.

close to the margin. Second, schooling is not just a decision between two options so the Angrist and Imbens (1994) result does not apply directly. Instead the IV estimate will be a weighted average across different levels of schooling. These weights will likely differ across the different instruments. Thus we strongly expect that if borrowing constraints were important, the coefficient on schooling should be higher when we instrument with direct costs, but we have not formally proved this result. A major advantage of the structural econometric model below is that we can use this same basic idea for identification, but identification of borrowing constraints is formally justified.

5.2 First Stage Results

We present the first stage results to demonstrate that the instruments have predicted power in the first stage and that their signs are consistent with the model presented above. Because years of schooling do not change during the period in which individuals are surveyed, it is difficult to interpret a first stage panel regression of schooling on time-varying local labor market variables and other characteristics. However, to convey the content of the first stage regressions, we report a regression of years of schooling on an average of local earnings rate (see "Mean Local Earnings over Working Life" in Table 1). The regression also includes "Local Earnings at Age 17," the indicator for the presence of a college in the county ("Local College"), family background measures, test scores, a set of indicators for age cohort, and a set of geographic control variables that include dummies for Census region of residence and living in an urban residence at age 17 (see Table 1 for details).

Column (1) of Table 2 shows that the effect of "Local College" is large and statistically significant, implying that individuals with a college in their county complete almost one half year more of school on average. The other covariates have estimates with signs and magnitudes consistent with those reported in other work (see e.g. Cameron and Heckman, 2001).

Column (2) presents estimates when "Local Earnings at Age 17" is included in the regression instead of "Local College." The estimated coefficient has the expected sign, but is not statistically significant at conventional levels. This variable apparently reflects both time-series variation in county earnings due to business-cycle effects as well as cross-sectional differences in average earnings and wealth across counties. Adding "Mean Local

Earnings over Working Life" to control for levels of wealth across counties allows us to sort out these two avenues of influence. Column (3) shows that the coefficient on the new variable is significant and positive, indicating that students from wealthier counties are more likely to attend college–perhaps as a result of superior schools or peer effects. In addition, the coefficient on "Local Earnings at Age 17," which proxies opportunity costs of school, is negative as expected and statistically significant. Column (4) shows adding "Local College" to the column (3) specification hardly alters any of the estimates just reported.

There are two reasons why the opportunity cost variable may have a negative sign. First, students could be more likely to attend school during temporary downturns. Second, individuals in counties in which the economy is improving could be more likely to attend college. In practice both avenues seem to have influence.[20] From the point of view of the model, the distinction is irrelevant. What is important to the student is the comparison of the economic conditions during their college years versus later in life. It doesn't matter whether the change in conditions results from business cycle fluctuations or a county specific trend. However, the business cycle avenue may be more intuitively appealing as a source of identification since it does not depend on long-run features of the county which may be correlated with individual ability. Unfortunately, we can not separate out these different sources without losing substantial power. Our evidence presented in Section 5.5 suggests this may not be an important concern. We show the local labor market variable is unrelated to observed measures of ability, so it seems plausible that the variable is not related to unobserved ability differences either.

5.3 IV Excluding Forgone Earnings

Table 3 displays estimated returns to schooling from log-wage equations when schooling is instrumented with the local labor market variable "Local Earnings at Age 17." This instrument, which proxies the opportunity costs of school, is not expected to have a larger impact on borrowing constrained individuals, so we do not expect much "discount rate bias." The

[20] In particular we re-estimated the regressions presented in Table 2 controlling for labor market conditions from 1970 through 1996 rather than just conditions from the time a person entered the labor force. The point estimates on "Local Earnings at 17" fell by about 33%.

estimated schooling return is shown in the first row of the table. The specification also includes "Current Local Earnings," dummy variables for racial ethnic identity, experience and experience squared, family background variables, our set of four test score variables, and cohort and geographic controls (see the base of Table 2 definitions). The test score and family background variables are included to capture as many dimensions of unobserved ability as the data allow. "Current Local Earnings" is instrumented with "Current Earnings in Age 17 County" as discussed above. As a practical matter, not instrumenting for "Current Local Earnings" made little difference to the results shown below.

Column (1) reports the OLS results. Column (2) shows that the IV point estimate of the schooling coefficient is about 50 percent larger than the OLS estimate. Instrumenting for experience and experience squared with age and age squared leaves the results essentially unchanged–see Column (3). In interpreting these findings, one should keep in mind that the standard errors are large enough that we cannot reject the hypothesis that OLS estimates and IV estimates are the same.

Columns (4) to (6) exhibit estimates of the same specifications except that test scores and family income are excluded. The OLS and IV point estimates of the schooling coefficient are higher, but the pattern is the same. Other specifications not reported here, including a more standard one with AFQT score instead of the set of four test scores, all yield similar patterns. These results are also similar to Arkes (1998) who uses state unemployment rates in a similar design and finds IV estimates higher than OLS estimates. The results of this specification are robust in their finding of IV estimates that are higher than the OLS estimates.

A potential problem with our model is that it does not account for influences of economic downturns on schooling choices that operate through the family or through working while in college. Borrowing constrained families may find raising funds for college more difficult during recessions. This possibility can reverse the direction of labor market effects: schooling may increase during booms for children whose parents are borrowing constrained. Thus, the influence of local labor market conditions on schooling attendance is no longer monotonic. The intuition for this is that the income and substitution effects of local labor market conditions go in opposite directions for borrowing constrained families and it is not clear which dominates. However, if families are not borrowing constrained, there is no income effect on schooling in a standard human capital model. Thus, it could be the case that borrowing constrained families send their children to college at higher rates during a boom; while non-borrowing constrained families send their children at higher rates during a bust because foregone earnings are low.

The negative association between county earnings and schooling reported in Table 2 supports the dominance of the second effect. However, this makes our Table 3 IV results even more surprising. If children of borrowing constrained families have higher marginal returns to schooling and decrease their schooling during recessions, the discount rate bias should be *negative*. Thus, our findings are counterintuitive if credit constraints are important. The mechanics of this argument are formalized in Section A of the Appendix (available from the authors' web sites).

The case in which students work while in school is analogous. Students who are borrowing constrained would presumably be more likely to work in college. Furthermore, earnings while in college would be relatively more important for them. Thus, the effect of local labor market conditions would likely have a larger effect on individuals who are not borrowing constrained than others (and could even go in the opposite direction). Once again this makes the results even more surprising.

5.4 IV Excluding Direct Costs of Schooling

Estimates of analogous specifications using presence of a local college as an instrument are presented in Table 4a. Columns (1) to (3) present results that do not control for "Current Local Earnings" in the regression. This specification reveals a large, causal effect of schooling, over 300 percent larger than the OLS point estimate. However, one may be worried that colleges are not randomly assigned to counties in the United States. Thus it makes sense to control for county level earnings and instrument using "Current earnings in age 17 county." This essentially controls for difference in wealth between different counties which hasn't been done in other studies using this instrument (see Card, 1999). Adding "Current Local Earnings" yields a strikingly different pattern: IV and OLS estimates are nearly identical (columns (4)-(6)).

Table 4b confirms the finding using specifications that control for ability with the AFQT score instead of the four test scores (columns (1) to (3)) and specifications with no ability controls (columns (4) to (6)). We present these specifications because they are close to those commonly found in the literature. When schooling and "Current Local Earnings" are instrumented together (columns (2) and (5)) and when schooling, "Current Local Earnings," experience, and experience squared are instrumented together, IV estimates of the schooling coefficient are well below their OLS counterparts.

The main point of this section of the paper is to contrast the IV estimates using direct costs (Table 3) with the IV estimates using opportunity costs (Table 4). Our theory predicted that if borrowing constraints were important for schooling decisions, IV estimates using direct costs should be higher than IV estimates using opportunity costs. However, the results in Tables 3 and 4 go in the opposite direction. We find no evidence for discount rate bias using this approach.

5.5 Validity of the Instruments

In general, without a maintained assumption that one of our instruments is valid, it is impossible to test the validity of any of them. In addition, since the causal effect of schooling is assumed to vary across individuals, standard over-identification tests do not apply. Nevertheless, it is still worthwhile to investigate our concern about correlation between the instruments and the omitted ability component of wage equation residuals. Finding no relationship between observable measures of ability and the instruments cannot prove the instruments are valid as they may still be correlated with unobserved ability components. Nevertheless, such a finding still lends credence to their use.

Column (1) of Table 5 shows the estimated relationship between the indicator variable for presence of a local college and our standard set of regressors: "Local Earnings at 17," "Mean Local Earnings over the Working Life," racial-ethnic indicators, our set of four scholastic test scores, and family background variables. The results are not encouraging; "Math Score" and "Automotive Score" are significantly related to presence of a college in the county. If "Local College" were correlated with unobserved ability in the same way as these observed measures, then the IV schooling estimate would be biased upwards.[21] This makes the results even more surprising as this might imply that the IV estimates should

[21] Altonji, Elder, and Taber (2002) provide a model that formally justifies this type of argument. We do not have enough control variables in this case to use the type of formal analysis they suggest.

be even lower than they actually are.

By contrast, the result that test scores do not predict "Local Earnings at Age 17" (column (2)) is encouraging. This finding supports our use of local labor market variation as an instrument. In fact, the only variable in this regression (other than "Mean Local Earnings over the Working Life") is family income. Given that family income is measured in the early part of the sample when the students were close to age 17, this is to be expected.[22]

The fact that test scores predict presence of a local college is not necessarily a problem, since these variables are included in our regressions. Table 6 presents results that are more favorable to the legitimacy of using "Local College" as an instrument. The table displays estimates of a probit analysis of indicators for college attendance and high school dropping out on the instruments, racial-ethnic identifiers, the four scholastic test scores, family background variables, and cohort and geographic controls. For us to have any faith in the exogeneity of "Local College," it should have a stronger effect on college attendance than high school dropping out.[23] However, if "Local College" picks up a community's "pro-schooling" character rather than the reduction in the total cost of college, the variable should predict high school graduation as well. Indeed, Table 6 shows "Local College" has a large and statistically significant influence on college attendance but an inconsequential and statistically insignificant effect on high school graduation. This result lends support to the validity of this variable as an instrument.

5.6 Interactions between Observables and Direct Costs

The identification argument sketched in Section 3 emphasizes the relationship between credit access and direct schooling costs. Following the logic of that argument, we could quantify the differential effects of college costs and college access if credit constraints were observable. Even though credit constraints are not observed, we can still estimate interactions between "Local College" and variables that we might expect to be related to credit

[22] For the same reason one may question whether family income is a valid control, but the basic results are robust to inclusion of family income.

[23] The variable may have an indirect effect on high school dropping out. If it increases the value of college, it must increase the value of high school graduation because high school graduates have the option of attending college.

availability in a schooling equation.

Table 7 exhibits estimates of years of schooling regressed on our standard set of covariates as well as five sets of variables interacted with the presence of a college. Column (1) of the table shows the interaction with black and Hispanic indicator variables. For a given income level, it is well documented that minorities have less wealth than whites and thus may be credit constrained relative to whites. We find the opposite: the interactions are negative showing that the presence of a college seems to be more important for whites than minorities (although the estimates are statistically insignificant).

Column (2) shows the interaction with father's education is positive but the interaction with mother's education is negative. Again, this pattern is hard to interpret as evidence of credit constraints. Columns (3) and (4) display the interaction with family income and number of siblings. Again, the interactions are insignificant and of the wrong sign. For completeness, column (5) presents interactions with "Math Score" and "Word Score."[24] This exercise produces no evidence of credit constraints.

In related work, both Card (1995b) and Kling (2001) examine similar sets of interactions in the National Longitudinal Survey of Young Men (NLS Young Men). They find evidence that individuals from low family background are affected more by the presence of a college. Kling (2001) also compares estimates from NLSY data and finds the NLSY estimates to be smaller in magnitude and not statistically significant. He reaches essentially the same conclusion that we do.

There are a number of reasons why the NLSY results may differ from those using the NLS Young Men. Chiefly, the NLSY data are from more recent cohorts. Individuals in Card's data made their high school graduation and college entry decisions in the decade between the early-1960's and early-1970's. During this time, few states had significant student aid programs, and the major Federal student grant and guaranteed loan programs that were passed into legislation in the mid-1960's were not funded at significant levels until after 1970.[25] Thus, it is quite possible that earlier generations of young people faced

[24] The column (5) interactions is somewhat difficult to interpret. While "Word Score" is inconsequential, "Math Score" is positive and significant indicating presence of a local college induces more schooling from higher ability students. It is unclear how to interpret this finding. Given the strength of test scores in predicting schooling it is not surprising that the interaction is significant, perhaps as a result of functional form restrictions in the regression.

[25] An exception was the National Direct Student Loan program, which originated in 1958. However, the

credit access problems that later cohorts did not. Of course, the data sets differ in other ways, so further exploration of this hypothesis is an important avenue for future research.

6 Evidence from the Structural Model

This section presents estimates of a structural version of the model developed in Section 3. There are at least three reasons for using the structural approach. First, extending the empirical specifications to an explicit structural model contributes a robustness check to the empirical findings. While many empirical economists debate the relative merits of structural versus reduced form or instrumental variables methods, few dispute that the methods typically depend on different identifying assumptions. It is reassuring when one obtains the same results from different approaches.

A second reason is to obtain the benefits of formal analysis. The argument that guided the regression and instrumental variable analysis could at best suggest patterns of estimates consistent with the presence of borrowing constraints. The framework developed in this section is based on explicit conditions under which borrowing constraints can be identified, tested, and interpreted. In fact, when we compare the regression approach above with a structural alternative, one can see that the regression approach missed a potentially important aspect of the problem that should be controlled for. This extra layer of formality will also aid future analysts in new explorations of borrowing constraints as well as in interpreting our own results. For example we explicitly describe the manner in which unobserved heterogeneity enters the model. This paper also contributes to our understanding of identification in econometric models by formally demonstrating how one can use the same economic idea (the difference between the effects of direct costs and foregone income in our case) for identification in regression, instrumental variable models, and structural estimation.

A third advantage of a structural model is that it provides estimated magnitudes of the parameters of interest. In our case, the model yields explicit estimates of the extent of borrowing constraints rather than just tests for their presence. The estimated model would also allow us to perform and interpret policy simulations, such as predicting the impact of

program was small and targeted science and engineering students.

below market interest rate loans to children from poor families.[26]

6.1 The Econometric Model

This subsection extends the economic model of Section 3 into two formal econometric frameworks. The first mirrors the regression approach presented in Section 5.6 by assuming credit cost heterogeneity is governed by characteristics observable to the economist. The second approach allows borrowing constraints to enter the model as unobserved heterogeneity. The notation below follows that established in Section 3, with the exception that the subscript i is introduced to enumerate individuals.

If person i chooses schooling level S, their wages at time t would be

$$\log(w_{Sit}) = \gamma_S + X_{Wi}'\beta_W + X_{Lit}'\beta_L + X_{Eit}'\beta_E + u_{Sit}, \tag{18}$$

where u_{Sit} represents the error. This latent random variable is defined for all schooling levels, but only observed for the schooling level that was actually chosen. For convenience, observed right-hand-side exogenous variables are parcelled into three vectors: X_{Wi}, a vector of characteristics that do not vary over time; X_{Lit}, time-varying local labor market measures; and X_{Eit}, time-varying measures of work experience (experience and experience-squared in our empirical work below). We assume that u_{Sit} is strictly orthogonal to these right-hand-side variables for all S and t. Schooling effects are captured through the level shifter, γ_S, and the shocks, u_{Sit}.

If individual i were to choose schooling level S, his present value of earnings discounted to the time he leaves school would be

$$\sum_{t=S}^{\infty} \delta^{t-S} e^{\gamma_S + X_{Wi}'\beta_W + X_{Lit}'\beta_L + X_{Eit}'\beta_E + u_{Sit}}. \tag{19}$$

As in Section 3, we abstract from uncertainty about the shocks $\{u_{Sit}\}$ and assume students choose schooling to solve a certainty equivalence problem so random variables are replaced with expected values. Letting E_0 denote future wage expectations at time zero, the certainty equivalent present value of earnings net of direct costs dated at the initial

[26] We do not perform such counterfactuals in this paper because we find no evidence of borrowing constraints.

period is defined as

$$I_{Si} = \left(\frac{1}{R_i}\right)^S e^{\gamma_S + X'_{Wi}\beta_W} \left(\sum_{t=S}^{\infty} \delta^{t-S} e^{X'_{Eit}\beta_E + E_0\left(X'_{Lit}\beta_L\right) + E_0(u_{Sit})}\right) - \sum_{t=0}^{S-1} \left(\frac{1}{R_i}\right)^t X'_{Ci}\beta_C \quad (20)$$

where R_i represents the individual borrowing rate. Costs of schooling (τ_t in section 3) have been parameterized to depend on observables through the index $X'_{Ci}\beta_C$.[27]

For empirical implementation, we make three simplifying assumptions. First, we assume that the wage equation residuals take the form

$$u_{Sit} = \theta_{Si} + \omega_{Sit}, \quad (21)$$

where θ_{Si} is a time-invariant random effect known to the individual when schooling decisions are made but not observed by the econometrician. The component ω_{Sit} is assumed orthogonal to all time 0 information for all t, so $E_0(u_{Sit}) = \theta_{Si}$. Both components of the error terms are maintained to be strictly orthogonal to the observables for all S and t, although θ_{Si} may be arbitrarily correlated with $\theta_{S'i}$ for any S and S'.

Second, we approximate time zero expectations of future local labor market conditions, $E_0(X'_{Lit}\beta_L)$, by a linear function of local labor market variables known to individuals when schooling decisions are made. We let these variables depend on S and denote them by X_{LSi} with associated coefficient vector $\widetilde{\beta}_{LS}$. To reduce notation we incorporate the wage equation intercept γ_S into $\widetilde{\beta}_{LS}$ so that[28]

$$X'_{LSi}\widetilde{\beta}_{LS} \equiv E_0\left(X'_{Lit}\beta_\ell\right) + \gamma_S + \log\left(\sum_{t=S}^{\infty} \delta^{t-S} e^{X'_{Eit}\beta_E}\right). \quad (22)$$

Third, we assume log utility for computational convenience. We see no reason why this simplification should bias the results in any particular direction.

Solving for the value of S and plugging in the present discounted value of earnings for

[27] We have assumed costs depend only on observables for computational convenience. We see no reason why this should bias the findings in either direction.

[28] The term $\log\left(\sum_{t=S}^{\infty} \delta^{t-S} e^{X'_{Eit}\beta_E}\right)$ is a constant that depends on S but not i, so it can be incorporated into the intercept.

individual i, associated with schooling level S yields,

$$\begin{aligned} V_{Si} &= \left(\frac{1}{1-\delta}\right)(\log(I_{Si}) + \log(1-\delta)) + \left[\sum_{t=0}^{S-1}\delta^t t + \left(\frac{\delta^S}{1-\delta}\right)S\right]\log(\delta R_i) + X_{Ti}^{'}\beta_{TS} + \nu_{Si} \\ &\equiv \alpha_1 \log\left(\left(\frac{1}{R_i}\right)^S e^{X_{Wi}^{'}\beta_W + X_{LSi}^{'}\widetilde{\beta}_{LS} + \theta_{Si}} - \sum_{t=0}^{S-1}\left(\frac{1}{R_i}\right)^t X_{Ci}^{'}\beta_C\right) \qquad (23) \\ &\quad + \alpha_{2S} + \alpha_{3S}\log(R_i) + X_{Ti}^{'}\beta_{TS} + \nu_{Si}, \end{aligned}$$

where the error term and observable index $X_{Ti}^{'}\beta_{TS} + \nu_{Si}$ are introduced to parameterize nonpecuniary benefits or costs associated with schooling level S (this is the $T(S)$ function in equation (5)). The terms α_1, α_{2S}, and α_{3S} are parameters that are implicitly defined in (23) and do not vary across individuals.

6.2 Sketch of Identification

To convey the basic mechanism through which identification is achieved, again assume two levels of schooling and no direct cost for $S = 0$.

The value functions of schooling for person i as derived in equation (23) are

$$\begin{aligned} V_{0i} &= \alpha_1\left(X_{Wi}^{'}\beta_W + X_{L0i}^{'}\widetilde{\beta}_{L0} + \theta_{0i}\right) + a_1 \qquad (24) \\ &\quad + X_{Ti}^{'}\beta_{T0} + \nu_{0i} \\ V_{1i} &= \alpha_1 \log(e^{-\log(R_i) + X_{Wi}^{'}\beta_W + X_{L1i}^{'}\widetilde{\beta}_{L1} + \theta_{1i}} - X_{Ci}^{'}\beta_C) + a_2 \qquad (25) \\ &\quad + a_3\log(R_i) + X_{Ti}^{'}\beta_{T1} + \nu_{1i}, \end{aligned}$$

where a_1, a_2, and a_3 are scalars (as defined in (23)) that do not vary across individuals.

Though (24) and (25) are complicated, these expression are close to a familiar linear index model. The only nonlinearity arises inside the logarithm in the first term of (25) which yields an interaction between the interest rate, R_i, and schooling costs $X_{Ci}^{'}\beta_C$. No such interaction exists between interest rates and foregone earnings which are represented by $X_{Wi}^{'}\beta_W + X_{L0i}^{'}\widetilde{\beta}_{L0} + \theta_{0i}$. This feature of the model delivers identification of the parameters of interest.

We estimate two versions of the model. The first restricts variation in R_i to be determined through particular observables, such as race or family income. The second version treats R_i as a variable known to the individual but not observed by the econometrician. Identification of each case is discussed separately.

6.2.1 Case I: R_i Determined by Observed Characteristics

Our first approach assumes that there is no persistent, unobserved individual influences on wages ($\theta_{Si} = 0$) and that R_i only varies with observables. The empirical content of this version of the model is very closely related to the regression approach presented in Section 5.6. To see why, suppose that local labor market and wage variables were not determinants of schooling value ($X'_{Wi}\beta_W + X'_{LSi}\widetilde{\beta}_{SL} = 0$ for $S = 0$ and $S = 1$). Let R_i be parameterized by the index $X'_{Ri}\beta_R$, and assume schooling costs, $X'_{Ci}\beta_C$, vary only with the presence of a local college. Equation (25) shows that $\log(R_i)$ enters the value function through the terms $\log(e^{-\log(R_i)} - X'_{Ci}\beta_C)$ and $a_3 \log(R_i)$. Since the observable variables that determine R_i are included in X_{Ti} and since they are all dummy variables, identification of $\log(R_i)$ comes completely from the first term (i.e. from $\log(e^{-\log(R_i)} - X'_{Ci}\beta_C)$). Also, since X_{Ci} is composed of a dummy variable, testing for interest rate heterogeneity in this specification ($\beta_R = 0$) is identical to a test for interactions between the presence of a local college and the variables determining the borrowing rate. Testing for this type of interaction is almost equivalent to the ad-hoc interactions estimated in the schooling regressions of Table 7. The only difference is that the dependent variables in this section is modelled as discrete. However, there is no reason to restrict $X'_{Wi}\beta_W + X'_{LSi}\widetilde{\beta}_{SL} = 0$ and doing so may well be misleading. This is an example of the advantages of precisely specifying the econometric model. Equations (24) and (25) make it clear that one can test for borrowing constraints by looking for an interaction between schooling costs, but when doing so one should control for the influence of future earnings in an appropriate manner (i.e. incorporating $X'_{Wi}\beta_W + X'_{LSi}\widetilde{\beta}_{SL}$ into the model).

Because $\theta_{Si} = 0$ in this version of the model, there is no selection bias from schooling choices so the wage equation parameters β_W and $\widetilde{\beta}_{L1}$ can be consistently estimated from the wage equation alone. Thus the existence of heterogeneity of borrowing rates in the structural model is identified by the interaction between X_{Ri} and the presence of a college once we control for wage effects through $X'_{Wi}\beta_W + X'_{LSi}\widetilde{\beta}_{LS}$. A major advantage of specifying this model is that we can also identify the magnitude of the borrowing constraint. The level of $\log(R_i)$ is not identified separately from the intercept in $\widetilde{\beta}_{LS}$. However, the relative value of $\log(R_i)$ between groups is identified which is absolutely crucial for this exercise. The magnitude of the difference in $\log(R_i)$ across groups is identified by comparing the size of the interaction between $\log(R_i)$ and $X_{Wi}^{'}\beta_W + X_{LSi}^{'}\widetilde{\beta}_{LS}$ (foregone earnings) relative to the interaction between $\log(R_i)$ and $X_{Ci}^{'}\beta_C$ (direct costs). That is suppose there are two groups a and b who face interest rates R^a and R^b respectively. The level $\log(R^a)$ is not identified, but the difference $\log(R^b) - \log(R^a)$ can be identified. In the empirical work below we choose a control group and assume that they are not borrowing constrained and then identify the borrowing rate for the constrained group.

6.2.2 Case II: R_i Determined by Unobserved Characteristics

Next consider the more complicated case in which R_i is an unobserved random variable. Taking the difference in schooling values from equations (24) and (25) and defining $X_i = (X_{Wi}, X_{L0}, X_{L1})$ to simplify the exposition yields the latent variable representation

$$V_{1i} - V_{0i} = \alpha_1 \log(e^{X_i'\Gamma_1 + \varepsilon_{1i}} - X_{Ci}^{'}\beta_C) + X_i\Gamma_2 + \varepsilon_{2i}, \tag{26}$$

where,

$$\begin{aligned} X_i\Gamma_1 &\equiv X_{Wi}^{'}\beta_W + X_{L1i}^{'}\widetilde{\beta}_{L1} &(27)\\ \varepsilon_{1i} &\equiv \theta_{1i} - \log(R_i) &(28)\\ X_i\Gamma_2 &\equiv X_{Ti}^{'}\beta_{T1} - \alpha_1\left(X_{Wi}^{'}\beta_W + X_{L0i}^{'}\widetilde{\beta}_{L0}\right) + a_2 - a_1 - X_{Ti}^{'}\beta_{T0} &(29)\\ \varepsilon_{2i} &\equiv a_3\log(R_i) + \nu_{1i} - \alpha_1\theta_{0i} - \nu_{0i}, &(30) \end{aligned}$$

so that Γ_1 and Γ_2 are defined appropriately. Individuals choose schooling option 1 if $V_{1i} - V_{0i} > 0$, and option 0 otherwise. Note that ε_{1i} enters the expression inside $\log(\cdot)$ in equation (26) and is correlated with the residual ε_{2i} both through the correlation between θ_{0i} and θ_{1i} and because $\log(R_i)$ enters both residuals.

The vector (X_i, X_{Ci}) is observable to the analyst but $(\theta_{1i}, \theta_{0i}, \log(R_i), \nu_{1i}, \nu_{0i})$ is not. We assume that (X_i, X_{Ci}) is independent of $(\theta_{1i}, \theta_{0i}, \log(R_i), \nu_{1i}, \nu_{0i})$. The selection equation (26) has three indices,

$$\left(X_i'\Gamma_1, X_i'\Gamma_2, X_{Ci}^{'}\beta_C\right),$$

and two error terms $(\varepsilon_{1i}, \varepsilon_{2i})$. The three degrees of freedom represented by these index functions are sufficient to trace out this joint distribution of $(\varepsilon_{1i}, \varepsilon_{2i})$.

However, from equations (28) and (30), one can see that the schooling decision alone is not sufficient for identification of the distribution of R_i as one could never hope to separate the distribution of R_i from the distribution of θ_{1i}. Fortunately we also have data on wages as given by equation (18). Since wages depends on θ_{1i} but not on R_i we can identify the distribution of $\log(R_i)$ up to a location normalization.

Theorem 1 is contained in the appendix (available from the authors' web sites). It proves nonparametric identification of the joint distribution of $(\varepsilon_{1i}, \varepsilon_{2i}, u_{1it})$ using the two types of exclusion restrictions, one for opportunity costs and one for direct costs.[29] Given this joint distribution we can identify the conditional expectation of $E\left(u_{1it} \mid \varepsilon_{1i}\right) = E(\theta_{1i} \mid \theta_{1i} - \log(R_i))$. We also show in the appendix that we can use this conditional expectation to estimate the distribution of R_i when R_i is independent of θ_{1i}. Thus we can identify the entire distribution of interest rates faced by students up to a normalization.

Intuition for identification comes from the fact that the form of the selection bias

$$E\left(\theta_{1i} \mid \alpha_1 \log(e^{X_i'\Gamma_1 + \theta_{1i} - \log(R_i)} - X_{Ci}'\beta_C) + X_i'\Gamma_2 + \varepsilon_{2i} > 0, X_i'\Gamma_1, X_{Ci}'\beta_C, X_i'\Gamma_2\right), \quad (31)$$

depends on the realized value of college costs. This expression is complicated and depends on the full distribution of $(\theta_{1i}, R_i, \varepsilon_{2i})$. Following the same logic discussed in Section 3, the term $\theta_{1i} - \log(R_i)$ will be relatively more important when costs of schooling are high because the $\log(\cdot)$ term in equation (31) is nonlinear. Suppose borrowing constraints were not important. Then there would be more selection bias in counties that do not have a college since θ_{1i} is relatively more important when college is costly. However, if borrowing constraints are very important, when costs are high schooling decisions would be dictated primarily by the extent of borrowing constraints. This would imply substantially less selection bias when no college is present.

6.3 Evidence when R_i is Determined by Observed Characteristics

We parameterize interest rates by a function $R(\cdot)$ and an index so that

[29] Using the notation above, the first type of exclusion restriction affects $X_i'\Gamma_2$, but not $X_i'\Gamma_1$ or $X_{Ci}'\beta_C$, and the second enters $X_{Ci}'\beta_C$ but not $X_i'\Gamma_2$ or $X_i'\Gamma_1$.

$$R\left(X_{Ri}'\beta_R\right) \geq 1 \text{ and } R\left(0\right) = \frac{1}{\delta}$$

where X_{Ri} are variables that determine the borrowing rate.[30] Thus, R_i can not be less than one but can be arbitrarily large. This form facilitates comparison between borrowing constrained individuals and a control group that is not constrained. Since we do not include an intercept in X_{Ri}, any population group with the value of X_{Ri} equal to zero will borrow for education at the market rate. For example, let X_{Ri} consist of dummy variables for black and Hispanic status. For whites $X_{Ri}'\beta_R$ is zero so $R_i = \frac{1}{\delta}$. A positive coefficient on the black or Hispanic dummy would imply that this subgroup borrows at an interest rate greater than the market rate.

In this section, we estimate the parameters in the model given by equations (18) and (23). We ignore selection by setting $\theta_{Si} = 0$, which implies that the schooling choice residuals (ν_{Si}) are independent of the wage residuals (u_{Sit}). We relax this restriction in the next subsection. The β_W parameters are estimated by OLS in a standard Mincer style log wage regression and the estimated values are inserted into the value function (the standard errors of the value-function parameters are adjusted appropriately). The cost index, $X_{Ci}'\beta_C$, consists of only the variable "Local College" and a constant. The vector of taste variables, X_{Ti}, contains family background variables, AFQT Score, minority dummies, and dummies for urban residence and region of residence.[31] Local labor market variables enter through the index $X_{LSi}'\widetilde{\beta}_{LS}$. We incorporate two measures of local labor markets. The first is the "Local Earnings" variable used above. It is restricted to affect only the wages of high school graduates and high school dropouts. The second variable is the long run average income in the county of residence at age 17 ("Mean Earnings over Working Life") and affects wages at all schooling levels.

We allow for four levels of schooling: high school dropout(S=0), high school graduate(S=2), some college(S=4), and college graduate(S=6).[32] During high school there are no direct costs of schooling, but costs must be incurred to attend some college and to graduate college.

[30]The exact form of $R\left(X_{Ri}'\beta_R\right)$ is $\exp\left[\exp\left(X_{Ri}'\beta_R + \log\left[-\log(\delta)\right]\right)\right]$.

[31]Most of these variables entered the schooling regressions in Table 2 except that the college in county indicator and the local labor market variables. Also, AFQT is used instead of the set of four test scores.

[32]GED recipients who attend no college are counted among dropouts. See Cameron and Heckman (1993).

We assume ν_{Si} has a Generalized Extreme Value distribution (GEV) so the model can be estimated as a nested logit. We use two levels of nesting. High school graduates are nested together, and college attenders are nested. Observable components of the utilities take the form,

$$
\begin{aligned}
\mu_{0i} &= \alpha_1 \left(X_{Wi}^{'}\beta_W + X_{L0i}^{'}\widetilde{\beta}_{L0} \right) + \alpha_{20} \\
\mu_{2i} &= \alpha_1 \left(-2\log(R_i) + X_{Wi}^{'}\beta_W + X_{L2i}^{'}\widetilde{\beta}_{L2} \right) + \alpha_{22} + \alpha_{32}\log(R_i) + X_{Ti}^{'}\beta_{T2} \\
\mu_{4i} &= \alpha_1 \log \left(\left(\frac{1}{R_i} \right)^4 e^{X_{Wi}^{'}\beta_W + X_{L4i}^{'}\widetilde{\beta}_{L4}} - \sum_{t=2}^{3} \left(\frac{1}{R_i} \right)^t X_{Ci}^{'}\beta_C \right) + \alpha_{24} + \alpha_{34}\log(R_i) + X_{Ti}^{'}\beta_{T4} \\
\mu_{6i} &= \alpha_1 \log \left(\left(\frac{1}{R_i} \right)^6 e^{X_{Wi}^{'}\beta_W + X_{L6i}^{'}\widetilde{\beta}_{L6}} - \sum_{t=2}^{5} \left(\frac{1}{R_i} \right)^t X_{Ci}^{'}\beta_C \right) + \alpha_{26} + \alpha_{36}\log(R_i) + X_{Ti}^{'}\beta_{T6},
\end{aligned}
$$

where the α_{jS} are defined in equation (23).

Nesting yields the following schooling probabilities,

$$
\begin{aligned}
\Pr(S &= 6 \mid S > 2, \mu_{0i,\dots,}\mu_{6i}) = \frac{\exp\left(\frac{\mu_{6i}}{\rho_c}\right)}{\exp\left(\frac{\mu_{6i}}{\rho_c}\right) + \exp\left(\frac{\mu_{4i}}{\rho_c}\right)} \\
\Pr(S &= 2 \mid S > 0, \mu_{0i,\dots,}\mu_{6i}) = \frac{\exp\left(\frac{\mu_{2i}}{\rho_h}\right)}{\exp\left(\frac{\mu_{2i}}{\rho_h}\right) + \left[\exp\left(\frac{\mu_{4i}}{\rho_c}\right) + \exp\left(\frac{\mu_{6i}}{\rho_c}\right)\right]^{\frac{\rho_c}{\rho_h}}} \\
\Pr(S &= 0 \mid \mu_{0i,\dots,}\mu_{6i}) = \frac{\exp\left(\mu_{0i}\right)}{\exp\left(\mu_{0i}\right) + \left(\exp\left(\frac{\mu_{2i}}{\rho_h}\right) + \left[\exp\left(\frac{\mu_{4i}}{\rho_c}\right) + \exp\left(\frac{\mu_{6i}}{\rho_c}\right)\right]^{\frac{\rho_c}{\rho_h}}\right)^{\rho_h}}.
\end{aligned}
$$

where ρ_h and ρ_c are parameters for the nesting of high school graduates and college attenders respectively.[33] The model is estimated by maximum likelihood, restricting ρ_h and ρ_c to lie between zero and one. This nested logit can also have the interpretation of a forward-looking, discrete-choice dynamic programming model (see Taber, 2000).

There are a number of normalizations necessary for identification. We assume β_{TS} is zero for dropouts, which normalizes the location as in a standard polychotomous choice model. In the high school equations, we cannot separately identify the intercepts in β_{TS} from the intercept in β_{LS} so we set the latter to zero. We normalize β_C, the coefficient on

[33] The probability for $S = 4$ can be constructed from these three probabilities.

"Local College," to one in the equations for college. This normalization is needed because we cannot separately identify the scale of the indices from the intercept in the tastes index.[34]

We found the intercept in the cost equation to be difficult to identify numerically, so we fixed it to two in the simulations.[35] We experimented extensively with other values for this parameter and found the results insensitive to this choice. In practice, we also had trouble identifying the coefficient on the local earnings variable in the college states. A reduced form regression yielded a value of approximately 0.3, so we fixed the coefficient to this value in the two college equations. Again, our results were not sensitive to this restriction. Finally, we assume $\delta = 0.97$, which yields a market interest rate of $R = \frac{1}{\delta} \approx 1.031$.

Table 8 exhibits parameter estimates for the base model in which there is no interest rate heterogeneity ($X'_{Ri}\beta_R = 0$). Panel A shows estimates of taste parameters, panel B estimates of local labor market variables, and panel C estimates of the GEV scale and nesting parameters. For the sake of brevity, we do not provide a general discussion of the parameters, but they appear reasonable and broadly consistent with the results in the educational attainment literature discussed in section 2.1.

Column (1) of Table 9 exhibits estimated educational borrowing rates using four alternative sets of observed characteristics. In column (2) we present negative log-likelihood values for each specification.

Panel B reports implied borrowing rates when dummy variables for black and Hispanic determine the borrowing rate. Unexpectedly, point estimates of the borrowing rate come in at the lower boundary value of 1.0, below the assumed market interest rate.

Panel C exhibits the case when borrowing rates depend on parental education through four dummy variables for interactions between father and mother having a college education or a high school education. The control group is comprised of individuals with two college-educated parents who are assumed to borrow at the market rate. Estimated borrowing rates show little deviation from the market rate.

In panel D results, borrowing rates depend on whether parental family income, falls into the top, middle, or bottom third of the population distribution. The top third is

[34] To see this, note that in equation (23) one can multiply β_C by any positive number γ, increase the intercept in $\widetilde{\beta}_{LS}$ by $\log(\gamma)$, and decrease the intercept in β_{TS} by $\alpha_1 \log(\gamma)$ without changing the value of V_{Si}. Normalizing the scale of β_C in this way performed better computationally than the alternatives.

[35] This means that living at home cuts the cost of schooling in half.

the control group. Estimated interest rates for the middle and bottom third are barely distinguishable from the market rate, and again the test fails to reject the hypothesis of no credit limitations. One can also see that the interest rates are precisely estimated so our failure to reject the null hypothesis results primarily from a small difference in the parameter values not lack of statistical power. Finally, panel E shows the case for family size, where one might expect to find the strongest evidence for heterogeneity in credit access. However, the borrowing rate turns out to be insensitive to the number of children in the family ("Number of Siblings"). Once again, confidence intervals are tight, indicating that our failure to reject the null is due to small coefficients rather than lack of power.

6.4 Evidence when R_i is not Observed

Failing to find evidence of heterogeneity in borrowing rates in the observables, we extend the structural model to capture borrowing rate heterogeneity in the unobservables. We assume that individuals come in one of two types: those borrowing at the market rate $1/\delta$ and those who are "constrained" and borrow at rate $R^c > 1/\delta$. We estimate the borrowing rate, R^c, and the fraction of the population that is constrained, P_c. We restrict the distribution of borrowing rates to be independent of the observables and the other error terms.

Unobserved ability enters the model through a single standard normal factor θ_i, which is known at the time schooling choices are made and is independent of observables. The error term in the wage equation is defined as

$$u_{Sit} = \phi_{WS}\theta_i + \omega_{Sit}, \tag{32}$$

where ϕ_{WS} is the wage-equation factor loading term, and ω_{Sit} is i.i.d. over time and orthogonal to θ_i for all t. Ability may also be correlated with unobserved tastes for schooling. The taste residual for school level S is now defined as

$$\nu_{Si} = \phi_{TS}\theta_i + \widetilde{\nu}_{Si}, \tag{33}$$

where ϕ_{TS} is the tastes-equation factor loading for school level S, and $\widetilde{\nu}_{Si}$ has a GEV distribution, which yields the nested logit model from the previous section.

A major goal of our approach in this section is to identify the extent of borrowing constraints from the economic considerations about the behavioral interaction between interest rates and costs rather than from functional form assumptions on the distribution of the error terms. This goal has led us to two nonstandard strategies. First, because we have no presumption about the form of the selection bias, we retain flexibility in modelling it. Ideally, we would place no restrictions on the joint distribution of the taste and wage errors, $(v_{2i}, v_{4i}, v_{6i}, u_{0it}, u_{2it}, u_{4it}, u_{6it})$. However, full nonparametric estimation of this distribution is not computationally feasible. Even after restricting the distribution of the error terms to specific functional forms with a one factor representation, there are still seven factor loading terms on θ_i that determine the form of the selection bias. We have *purposely* chosen to over-parameterize the form of the selection bias to force identification to come from the interaction between the selection terms and the college costs index rather than from the functional form of the error term. As a result of the flexibility we can not obtain standard errors for the parameters because the Hessian is singular. Nevertheless, consistent estimation of the R_i distribution is still possible, and we can use a likelihood ratio statistic to test for borrowing constraints so we do not view this as a serious concern.

We first tried to estimate the model using maximum likelihood, but estimates appeared sensitive to restrictions on the factor loading terms. In particular, we could not relax the distribution of ω_{Si} sufficiently to distinguish the distribution of R_i from a flexible functional from for the school choice equation. Identifying the parameters in this way is certainly not in the spirit of our nonparametric identification results. This led us to a second nonstandard approach. We used an iterative procedure to force identification of the distribution of R_i and other parameters to come from particular variation in the data.

In particular, we partition the parameters of our model into those that the identification results suggest should be identified primarily from the selection equation (Ψ_1) and those that require the wage equation (Ψ_2). Redefine X_i to denote all observables and let $\pounds_i(\Psi_1, \Psi_2; X_i)$ be the log likelihood function for the schooling decision only (derived as a nested logit with unobserved heterogeneity). The model is estimated using the following iterative procedure,

1. Fix Ψ_2 and solve for the value of Ψ_1 that maximizes the log likelihood function.

2. Fix Ψ_1 and solve for the value of Ψ_2 that minimizes nonlinear least squares,

$$\sum_{i=1}^{N}\sum_{t=1}^{T_i}\left(\log(w_{S_iit}) - E(\log(w_{S_iit}) \mid X_i, S_i; \Psi_1, \Psi_2)\right)^2, \tag{34}$$

where S_i represents the realized value of S for person i.

We iterate until convergence.[36] This procedure is equivalent to GMM with moment conditions

$$E\left[\begin{array}{c}\frac{\partial \pounds_i(\Psi_1,\Psi_2;X_i)}{\partial \Psi_1} \\ \frac{\partial E(\log(w_{S_iit})|X_i,S_i;\Psi_1,\Psi_2)}{\partial \Psi_2}\left(\log(w_{S_iit}) - E(\log(w_{S_iit}) \mid X_i, S_i; \Psi_1, \Psi_2)\right)\end{array}\right] = 0. \tag{35}$$

Following the logic behind identification, we want the borrowing rates identified by the form of selection bias in the wage equation, so the distribution of R_i was estimated with the parameters in Ψ_2. To keep the form of the selection bias flexible, we estimate the factor loading terms on θ_i in the wage equation also. We use the following partition,

$$\begin{aligned}\Psi_1 &= \left\{\alpha_1, \rho_h, \rho_c, \left(\beta_{TS}, \widetilde{\beta}_{LS} : S \in \{0,2,4,6\}\right)\right\} \\ \Psi_2 &= \left\{\beta_W, \beta_L, \beta_E, P_c, R^c, \left(\gamma_S, \phi_{LS}, \phi_{WS} : S \in \{0,2,4,6\}\right)\right\}.\end{aligned}$$

Panel A of Table 10 shows the estimates from this approach.[37] The model produces an estimated borrowing rate of 1.071 for the borrowing constrained group (compared to 1.030 for the unconstrained group) but converges to a boundary value, where the fraction of the population borrowing at the higher rate is zero. Both because of the reason mentioned above and because the estimate is on the boundary, standard errors of the parameters could not be calculated. We also experimented with a large number of alternative starting values and specifications which produced no evidence of heterogeneity in borrowing rates. Thus this final approach like the previous ones provides no evidence that borrowing constraints are important for schooling decisions.

[36] In practice we use a damping parameter to aid convergence.

[37] Experimenting with a wide variety of specifications and starting values showed the model did not typically converge when no restrictions were placed on the ϕ_{ST} parameters (factor loading on heterogeneity in tastes for schooling). Consequently, we restricted the factor loadings in the "high school dropout" and "high school graduation" equations to be equal ($\phi_{T0} = \phi_{T2}$) and the "some college" and "graduate college" factor loadings to be equal also ($\phi_{T4} = \phi_{T6}$). These restriction did not appear to affect estimates but aided convergence substantially.

7 Summary and Conclusions

This paper develops the economic reasoning that direct costs of schooling affect borrowing constrained individuals in a fundamentally different way than opportunity costs. Using variation in direct costs and opportunity costs, we test for educational borrowing constraints in four different ways: 1) instrumental variable wage regressions, 2) years of schooling regressions with interactions between college costs and various observables likely to be correlated with borrowing constraints, 3) a structural econometric model in which borrowing rates depend on observables, and 4) a structural model that allows for unobservable heterogeneity in borrowing rates. We find no evidence of borrowing constraints using any of the methods.

An issue that inevitably arises with this type of finding is statistical power. Have we found no evidence of borrowing constraints because borrowing constraints are not problematic or because we cannot measure the relevant parameters with enough precision? It is never possible to precisely separate these two claims. In the end, the answer chosen depends on prior beliefs about the size of the effect and the empirical content of the data. Our view is that power is less of a concern in the second method (the schooling regressions) and the third method (the structural model that restricts borrowing rate heterogeneity to vary with observables). The college in county variable is powerful; if interactions with it were important, we would expect to be able to detect an interaction. The small standard errors on the interest rates in Table 9 support our view. Estimates are precise, and show little heterogeneity in borrowing constraints across families of different sizes and income levels.

By contrast, it is less clear how much power we have with the imprecisely estimated instrumental variable estimates of schooling returns. Nevertheless, in interpreting the IV results, there are a number of things to keep in mind. First, evidence supporting discount rate bias is weak. There are a number of studies that find IV estimates larger than OLS estimates, but, consistently, IV standard errors in these studies are large. Second, a few recent studies instrument schooling with a variable like presence of a local college. Our results show these estimates are sensitive to inclusion of local labor market measures. Proper inclusion of local labor market controls weakens or eliminates the pattern found by us, so it seems possible that if other researchers had also included controls for local labor markets they may find IV estimates of schooling returns near or below OLS estimates. If so, Card's (1999) finding that IV estimates are consistently higher than OLS estimates would weaken considerably. Third, our conclusions are not based on standard errors alone. Point estimates consistently turn up with the wrong sign. Fourth, bias arising from potential problems with the instruments in the IV wage specifications generally goes in the wrong direction. First, Table 5 suggests return to schooling instrumented with presence of a local college may be biased upwards because of potential correlation with unobserved ability. Second, if family contributions to borrowing constrained students diminish during economic recessions, IV estimates will be biased downward and will come in below OLS estimates. The IV results per se may not be convincing evidence that borrowing constraints do not exist, but at the very least, they cast doubt on the discount rate bias story that predicts IV estimates of schooling returns are biased upward. Evidence from other studies support our doubts about discount rate bias.[38]

When standard errors could be calculated in the structural selection model, confidence intervals were found to be small. When the parameters of the heterogeneity distribution converged to boundary values, standard errors could not be calculated and statistical power could not be determined. In these cases, though, point estimates showed no evidence of heterogeneity in credit access. We believe our basic approach is useful and hope it leads to additional studies on this topic.

All told, our results are consistent with Cameron and Heckman (2001), Keane and Wolpin (2001), and Shea (2000). All four methods we employ produce no evidence of borrowing constraints. When interpreting our findings, however, two caveats must be kept in mind. First, none of our evidence is direct as we do not observe actual rates at which individuals in our data borrow for school. Second, our results do not imply credit market constraints would not exist in the absence of private and government programs currently available. Instead, our findings show that given the large range of subsidies to education that currently exist, there is no evidence of underinvestment in schooling resulting from borrowing constraints.

[38]Ashenfelter,Harmon, and Osterbeek (1999) have an alternative argument for the IV findings in the literature. They argue that the pattern may be due to a troubling specification or publication bias. Since IV standard errors are often high, if researchers prefer specifications in which the t-statistic is greater than two, there will be a bias towards publishing studies with high IV estimates.

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Table 1
Details and Summary Statistics of the Primary Variables

Variable	Mean	Standard Deviation	Sample Size
A. Schooling and Minority Status			
Years of School[a]	12.8	2.5	2404
Black	.31	.46	2404
Hispanic	.19	.39	2404
B. Family Background Variables[b]			
Highest Grade Father	10.50	4.10	2404
Highest Grade Mother	10.78	3.21	2404
Number of Siblings	3.73	2.63	2404
Broken Home	.24	.42	2404
Family Income[c] (1999 $'s)	$39,601	2,444	2404
C. Geographic Controls–measured at age 17			
Urban Residence	.79	.40	2404
Residence in Northeast	.20	.40	2404
Residence in South	.36	.48	2404
Residence in West	.19	.39	2404
Residence in Northcentral	.25	.43	2404
D. Test Score Variables[d]			
AFQT Score	61.1	22.3	2404
Math Score	11.9	6.3	2404
Word Score	21.9	8.5	2404
Science Score	14.2	5.4	2404
Automotive Score	13.4	5.5	2404
E. Instruments for Schooling			
Local College[e]	.87	.34	2404
Local Earnings at Age 17 (1999 $'s)[f]	$22,620	4,209	2404
Mean Local Earnings over Working Life (1999 dollars)[f]	$23,073	4,333	2404
F. Time-Varying Labor Market Variables[g]			
Hourly Wage (1999 dollars)	$10.01	3.479	13,762
Current Local Earnings (1999 $'s)	$24,519	6,122	13,762
Work Experience (in years)	7.536	3.169	13,762

[a]Years of schooling completed by the 1994 interview date.

[b]Except for "Family Income," these variables are measured at age 14. "Broken Home" is a binary variable indicating whether a person was living with both biological parents.

[c]Parental Family income is available annually only for individual who are dependents of his or her parents. Our measure takes parental family income as close to age 17 as possible. For about 16 percent of our sample, the measure was unavailable at age 17 and was taken at age 16 or age 18.

[d]Test scores are from the ASVAB tests; see the text for details.

[e]An indicator variable for presence of a college (two- or four-year) in the county of residence at age 17.

[f]Average earnings in the county in which an individual resides at age 17 among persons employed in industries predominated by unskilled workers. "Mean Local Earnings over Working Life" is the earnings rate averaged from 1980 through 1996 in the county in which an individual resides at age 17. See the text for more details.

[g]These are annual measures. "Current Local Earnings" is an annual measure of the same data series as "Local Earnings at Age 17" and is taken in the county in which an individual resided at age 17. "Hourly Wage" is hourly earnings at an individual's current or most recent job at the time of each annual interview. "Work Experience" is defined as age-schooling-6.

SOURCE.–The National Longitudinal Survey of Youth. County Average Earnings is from the Bureau of Economic Analysis' Regional Economic Information System data and constructed from annual average earnings in retail, wholesale, and construction industries. Information on college location and college costs is from the Department of Education's Higher Education General Information System and Integrated Postsecondary Education Database surveys of institutional characteristics.

Table 2
First Stage Regression of Years of Schooling[a]
(Standard Errors in Parentheses[b])

Variable	(1)	(2)	(3)	(4)
Local College	.417			.435
	(.152)			(.148)
Local Earnings at 17/1000		-.023	-.182	-.183
		(.021)	(.074)	(.074)
Mean Local Earnings over			.130	.123
Working Life/1000			(.058)	(.058)
Black	.688	.723	.691	.677
	(.124)	(.124)	(.123)	(.124)
Hispanic	.334	.359	.348	.345
	(.138)	(.143)	(.141)	(.141)
Math Score	.167	.168	.167	.166
	(.010)	(.010)	(.010	(.010)
Word Score	.050	.050	.050	.050
	(.010)	(.009)	(.010)	(.010)
Science Score	.065	.066	.066	.065
	(.015)	(.015)	(.015)	(.015)
Automotive Score	-.065	-.069	-.065	-.066
	(.011)	(.010)	(.011)	(.011)
Highest Grade Father	.051	.053	.052	.050
	(.016)	(.016)	(.016)	(.016)
Highest Grade Mother	.038	.039	.039	.039
	(.022)	(.022)	(.022)	(.022)
Number of Siblings	-.044	-.047	-.047	-.046
	(.016)	(.016)	(.016)	(.016)
Family Income/10,000	.090	.094	.095	.095
	(.022)	(.022)	(.022)	(.022)
Intercept	11.320	11.710	11.993	11.886
	(.273)	(.335)	(.371)	(.321)
Geographic Controls[c]	yes	yes	yes	yes
Cohort Controls[d]	yes	yes	yes	yes
Sample Size	2404	2404	2404	2404

[a]Dependent variable is years of schooling as defined in Table 1.

[b]Because "Local College" and the local labor market variables are measured at the county level, the standard errors used in this paper allow for arbitrary correlation among individuals living in the same county at age 17 (i.e., clustering). See Moulton (1986) for the definition of the adjusted standard errors.

[c]Geographic controls include an indicator variable for urban residence at age 17 and indicators for residence in the four standard Census regions at age 17. See Table 1.

[d]Cohort controls are a set of four indicator variables for age, which ranges from 13 to 16 in 1978.

Table 3

OLS and IV Estimates of Log Hourly Wage Function[a]
Using Foregone Earnings as the Instrument for Schooling
(Standard Errors in Parentheses[b])

	OLS (1)	IV1 (2)	IV2 (3)	OLS (4)	IV1 (5)	IV2 (6)
Schooling	.058	.083	.110	.074	.107	.134
	(.004)	(.042)	(.086)	(.004)	(.034)	(.061)
Current Local Earnings/1000	.027	.035	.036	.025	.031	.034
	(.003)	(.006)	(.005)	(.003)	(.005)	(.005)
Work Experience	.054	.065	.091	.055	.074	.083
	(.006)	(.018)	(.023)	(.006)	(.019)	(.022)
Experience Squared	-.002	-.002	-.004	-.002	-.002	-.004
	(.000)	(.000)	(.001)	(.000)	(.001)	(.001)
Black	-.063	-.085	-.115	-.148	-.162	-.178
	(.024)	(.031)	(.061)	(.022)	(.022)	(.028)
Hispanic	-.020	-.033	-.041	-.061	-.074	-.085
	(.030)	(.029)	(.034)	(.030)	(.029)	(.032)
Highest Grade Father	-.003	-.005	-.008	-.001	-.005	-.013
	(.004)	(.005)	(.010)	(.004)	(.006)	(.014)
Highest Grade Mother	-.005	-.008	-.012	-.001	-.007	-.015
	(.005)	(.006)	(.011)	(.005)	(.007)	(.014)
Number of Siblings	.002	.003	.005	-.001	.001	.005
	(.003)	(.004)	(.005)	(.005)	(.004)	(.007)
Family Income/10,000	.036	.034	.031	.042	.039	.034
	(.005)	(.005)	(.007)	(.005)	(.006)	(.005)
Math Score	.010	.007	.002			
	(.002)	(.005)	(.014)			
Word Score	.002	.001	-.000			
	(.002)	(.002)	(.004)			
Science Score	-.004	-.005	-.006			
	(.003)	(.003)	(.006)			
Automotive Score	.012	.014	.016			
	(.002)	(.003)	(.007)			
Geographic Controls[c]	yes	yes	yes	yes	yes	yes
Cohort Controls[c]	yes	yes	yes	yes	yes	yes
Instrument for Schooling:						
Local Earnings at 17	na	yes	yes	na	yes	yes
Instrument for Current Local Earnings:						
Current Earnings in Age 17 County	na	yes	yes	na	yes	yes
Instruments for Work Experience and Work Experience Squared:						
Age and Age Squared	na	no	yes	na	no	yes
Number of Individuals	2225	2225	2225	2225	2225	2225
Total Observations	13762	13762	13762	13762	13762	13762

na = Not applicable.

[a]The dependent variable is log hourly wage. IV1 treats experience and experience squared as exogenous; IV2 instruments them with age and age squared. "Current Local Earnings" is instrumented with "Current Local Earnings in Age 17 County" in all IV specifications.

[b]Standard errors are robust with respect to arbitrary correlation across time for observations on the same person and across persons who live in the same county at age 17(i.e. clustering). See the footnote at the base of Table 2.

[c]See the base of Table 2 for definitions of these variables.

Table 4a
OLS and IV Estimates of the Log Hourly Wage Function[a]
Using Presence of Local College as an Instrument
(Standard Errors in Parentheses[b])

	OLS (1)	IV1 (2)	IV2 (3)	OLS (4)	IV1 (5)	IV2 (6)
Schooling	.062	.228	.193	.058	.057	.061
	(.004)	(.109)	(.084)	(.004)	(.115)	(.076)
Current Local Earnings/1000				.027	.035	.035
				(.002)	(.006)	(.005)
Experience	.054	.124	.075	.054	.054	.102
	(.006)	(.047)	(.020)	(.006)	(.049)	(.019)
Experience Squared	-.002	-.003	-.004	-.002	-.002	-.005
	(.000)	(.001)	(.001)	(.000)	(.001)	(.001)
Black	-.029	-.111	-.119	-.063	-.073	-.082
	(.026)	(.066)	(.067)	(.023)	(.063)	(.059)
Hispanic	.009	-.023	-.020	-.020	-.028	-.029
	(.035)	(.045)	(.044)	(.026)	(.037)	(.036)
Highest Grade Father	-.003	-.014	-.015	-.003	-.003	-.003
	(.004)	(.009)	(.010)	(.004)	(.008)	(.008)
Highest Grade Mother	-.002	-.016	-.017	-.005	-.006	-.006
	(.005)	(.010)	(.011)	(.005)	(.010)	(.010)
Number of Siblings	-.001	.003	.005	.002	.003	.003
	(.004)	(.005)	(.005)	(.003)	(.005)	(.005)
Family Income	.039	.033	.032	.036	.035	.034
	(.005)	(.007)	(.007)	(.005)	(.006)	(.007)
Math Score	.010	-.008	-.010	.010	.010	.010
	(.002)	(.013)	(.014)	(.002)	(.013)	(.013)
Word Score	.003	-.001	-.002	.002	.002	.002
	(.002)	(.004)	(.004)	(.002)	(.003)	(.003)
Science Score	-.005	-.012	-.013	-.004	-.003	-.003
	(.003)	(.006)	(.006)	(.003)	(.006)	(.005)
Automotive Score	.009	.017	.018	.012	.012	.012
	(.002)	(.005)	(.006)	(.002)	(.005)	(.005)
Geographic Controls[c]	yes	yes	yes	yes	yes	yes
Cohort Controls[c]	yes	yes	yes	yes	yes	yes
Instruments for Schooling:						
College in County	na	yes	yes	na	yes	yes
Instrument for Current Local Earnings:						
Current Earnings in Age 17 County	na	no	no	na	yes	yes
Instruments for Work Experience and Work Experience Squared:						
Age and Age Squared	na	no	yes	na	no	yes
Number of Individuals	2225	2225	2225	2225	2225	2225
Total Wage Observations	13762	13762	13762	13762	13762	13762

na = Not applicable.

[a]The dependent variable is log hourly wage. IV1 treats experience and experience squared as exogenous; IV2 instruments them with age and age squared. "Current Local Earnings" is instrumented with "Current Local Earnings in Age 17 County" in the Column (4)-(6) specifications.

[b]See footnote *b* at the base of Table 3 for notes on the standard errors.

[c]See the base of Table 2 for definitions of these variables.

Table 4b

OLS and IV Estimates of the Log Hourly Wage Function[a]
Using Presence of Local College as an Instrument
(Standard Errors in Parentheses[b])

	OLS (1)	IV1 (2)	IV2 (3)	OLS (4)	IV1 (5)	IV2 (6)
Schooling	.054	.027	.042	.074	.052	.065
	(.004)	(.095)	(.060)	(.004)	(.078)	(.046)
Local Earnings/1000	.026	.035	.034	.025	.031	.031
	(.002)	(.006)	(.006)	(.002)	(.006)	(.006)
Experience	.054	.042	.106	.055	.043	.104
	(.006)	(.046)	(.017)	(.006)	(.044)	(.016)
Experience Squared	-.002	-.002	-.005	-.002	-.002	-.005
	(.000)	(.001)	(.001)	(.000)	(.001)	(.001)
Black	-.081	-.077	-.087	-.147	-.154	-.160
	(.021)	(.066)	(.062)	(.021)	(.026)	(.026)
Hispanic	-.040	-.043	-.043	-.061	-.065	-.064
	(.026)	(.040)	(.040)	(.026)	(.032)	(.033)
Highest Grade Father	-.004	-.002	-.001	-.001	.001	.002
	(.004)	(.009)	(.009)	(.004)	(.010)	(.010)
Highest Grade Mother	-.006	-.005	-.005	-.001	.001	.000
	(.005)	(.008)	(.008)	(.005)	(.011)	(.010)
Number of Siblings	.002	.003	.003	-.001	-.001	-.001
	(.003)	(.004)	(.004)	(.003)	(.006)	(.006)
Family Income/10,000	.036	.036	.036	.042	.044	.043
	(.005)	(.006)	(.006)	(.005)	(.009)	(.009)
AFQT Score	.005	.006	.006			
	(.001)	(.004)	(.003)			
Geographic Controls[c]	yes	yes	yes	yes	yes	yes
Cohort Controls[c]	yes	yes	yes	yes	yes	yes
Instrument for Schooling:						
Local College	na	yes	yes	na	yes	yes
Instrument for Current Local Earnings:						
Current Earnings in Age 17 County	na	yes	yes	na	yes	yes
Instruments for Work Experience and Work Experience Squared:						
Age and Age Squared	na	no	yes	na	no	yes
Number of Individuals	2225	2225	2225	2225	2225	2225
Total Observations	13762	13762	13762	13762	13762	13762

[a]The dependent variable is log hourly wage. IV1 treats experience and experience squared as exogenous; IV2 instruments them with age and age squared. "Current Local Earnings" is instrumented with "Current Local Earnings in Age 17 County" in all IV specifications.

[b]See footnote *b* at the base of Table 3 for notes on the standard errors.

[c]See the base of Table 2 for definitions of these variables.

Table 5

Regression of the Instruments on Schooling and Wage Determinants
(Standard Errors in Parentheses[a])

	Dependent Variable	
Covariate	Local College (1)	Local Earnings at Age 17 (2)
College in County		.038
		(.091)
Local Earnings at 17/1000	.006	
	(.014)	
Mean Local Earnings over	.013	.748
Working Life/10,000	(.011)	(.017)
Black	.032	-.094
	(.035)	(.070)
Hispanic	.005	-.007
	(.037)	(.078)
Math Score/10	.030	-.052
	(.016)	(.033)
Word Score/10	.014	.022
	(.014)	(.054)
Science Score/10	.023	.022
	(.024)	(.054)
Automotive Score	-.053	-.006
	(.017)	(.043)
Highest Grade Father	.006	-.004
	(.002)	(.007)
Highest Grade Mother	-.001	-.001
	(.003)	(.007)
Number of Siblings	-.002	.007
	(.003)	(.007)
Family Income/10,000	-.001	.035
	(.003)	(.012)
Geographic Controls[b]	yes	yes
Cohort Controls[b]	yes	yes
Sample Size	2404	2404

[a]Standard errors are robust with respect to arbitrary correlation across persons who live in the same county at age 17 (i.e., clustering). See the footnote at the base of Table 2.

[b]See the base of Table 2 for definitions of these variables.

NOTE.–See Table 2 for variable definitions.

Table 6

Determinants of College Attendance and High School Dropping Out
Average Derivatives from Probit Model are Reported
(Standard Errors in Parentheses[a])

	Dependent Variable	
	College Attendance	High School Dropping Out
Covariate	(1)	(2)
College in County	.166	-.035
	(.036)	(.030)
Local Earnings at 17	-.043	.028
	(.022)	(.016)
Mean Local Earnings	.035	-.009
over Working Life	(.017)	(.013)
Black	.170	-.109
	(.037)	(.023)
Hispanic	.144	-.034
	(.041)	(.024)
Math Score	.032	-.020
	(.003)	(.002)
Word Score	.017	-.007
	(.003)	(.002)
Science Score	.010	-.006
	(.004)	(.003)
Automotive Score	-.013	.002
	(.003)	(.002)
Highest Grade Father	.013	-.003
	(.004)	(.003)
Highest Grade Mother	.010	-.004
	(.007)	(.004)
Number of Siblings	-.016	.004
	(.005)	(.003)
Family Income	.011	-.023
	(.007)	(.005)
Sample Size	2404	2404
Geographic Controls[b]	yes	yes
Cohort Controls[b]	yes	yes

[a]Standard errors of the probit estimates are robust with respect to arbitrary clustering correlation across persons who live in the same county at age 17.

[b]See the base of Table 2 for definitions of these variables.

NOTE.–Average derivatives are formed by calculating the derivative for each individual in the sample with respect to each right-hand side variable and then averaging.

Table 7

Regression of Years of Schooling on Presence of Local College
Interacted with Alternative Covariates
(Standard Errors in Parentheses[a])

	(1)	(2)	(3)	(4)	(5)
Main Effects:					
College in County	.596	3.149	.103	.592	.491
	(.166)	(.072)	(.236)	(.229)	(.15)
Local Earnings at 17	-.102	-.074	-.107	-.104	-.109
	(.059)	(.038)	(.059)	(.058)	(.058)
Mean Local Earnings over	.067	.038	.068	.065	.070
Working Life	(.046)	(.031)	(.046)	(.046)	(.045)
Black	.968	.333	.690	.689	.709
	(.211)	(.086)	(.125)	(.125)	(.125)
Hispanic	.914	.018	.347	.344	.361
	(.379)	(.105)	(.142)	(.142)	(.142)
Math Score	.165	.077	.166	.166	.104
	(.010)	(.006)	(.010)	(.010)	(.03)
Word Score	.050	.013	.050	.050	.057
	(.010)	(.007)	(.010)	(.010)	(.022)
Science Score	.067	.042	.065	.066	.065
	(.015)	(.012)	(.015)	(.015)	(.015)
Automotive Score	-.067	-.031	-.066	-.066	-.066
	(.011)	(.007)	(.011)	(.011)	(.011)
Highest Grade Father	.051	-.040	.051	.051	.052
	(.016)	(.028)	(.016)	(.016)	(.016)
Highest Grade Mother	.038	.074	.038	.038	.038
	(.022)	(.053)	(.022)	(.022)	(.022)
Number of Siblings	-.046	-.013	-.046	-.012	-.046
	(.016)	(.011)	(.016)	(.038)	(.016)
Family Income	.092	.068	-.008	.095	.093
	(.022)	(.017)	(.068)	(.022)	(.022)
Geographic Controls[b]	yes	yes	yes	yes	yes
Cohort Controls[b]	yes	yes	yes	yes	yes

(Continued on Following Page)

Table 7 (continued)

Interactions:					
Black× Local College	-.330				
	(.220)				
Hispanic× Local College	-.635				
	(.390)				
Highest Grade Father× Local College		.071			
		(.030)			
Highest Grade Mother× Local College		-.065			
		(.055)			
Family Income× Local College			.111		
			(.067)		
Number of Siblings× Local College				-.039	
				(.041)	
Math Score× Local College					.069
					(.031)
Word Score× Local College					-.007
					(.021)
Sample Size	2404	2404	2404	2404	2404

[a]Standard errors are robust with respect to arbitrary correlation across persons who live in the same county at age 17. See the footnote at the base of Table 2.

[b]See the base of Table 2 for definitions of these variables.

Table 8
Estimated Structural Schooling Model I
Heterogeneity in Borrowing Rates Depends on Observed Characteristics[a]
without Borrowing Constraint Restrictions
(Standard Errors in Parentheses)[b]

A. Taste parameters (β_T)

	Graduate High School	Attend College	Graduate College
Black	.855	1.215	1.570
	(.169)	(.202)	(.252)
Hispanic	.136	.488	.539
	(.187)	(.212)	(.279)
AFQT Score	.045	.054	.101
	(.005)	(.008)	(.019)
Father Dropout	-.055	-.123	-.028
	(.144)	(.158)	(.198)
Father Some Coll	.076	.233	.603
	(.258)	(.273)	(.293)
Father Coll Grad	.445	.618	1.322
	(.349)	(.359)	(.357)
Mother Dropout	-.142	-.227	-.392
	(.140)	(.157)	(.209)
Mother Some Coll	.077	-.162	.376
	(.295)	(.318)	(.319)
Mother Coll Grad	.200	.332	.907
	(.396)	(.406)	(.417)
Number of Siblings	-.029	-.092	-.124
	(.042)	(.047)	(.055)
Family Income	.162	.121	.136
	(.043)	(.045)	(.054)
Geographic Controls[c]	yes	yes	yes
Cohort Controls[c]	yes	yes	yes

B. Local Labor Market Parameters (β_l)

	Attend High School	Graduate High School	Attend College	Graduate College
Intercept			5.689	5.236
			(.417)	(.338)
Local Earnings at Age 17	.044	.009		
	(.047)	(.022)		
Mean Local Earnings over	.302	.293	.300	.300
Working Life	(.017)	(.008)	(-)	(-)

C. Additional Parameters

Scale	1.281
	(.140)
High School Nesting (ρ_H)	.452
	(.146)
College Nesting (ρ_C)	1.000

[a]In the estimates shown here, estimates of borrowing constraint parameters are not estimated. This is the base model. See panel A of Table 9.

[b]Since the model is over-parameterized the standard errors are likely to be misleading.

[c]See the base of table 2 for definitions of these variables.

The equations for schooling choice and wages are specified as

$$\begin{aligned} V_s &= \alpha_1 \log\left(\left(\frac{1}{R_i}\right)^S e^{\tilde{\gamma}_S + X_{Wi}'\beta_W + X_{L0i}'\tilde{\beta}_{LS}} - \sum_{t=0}^{S}\left(\frac{1}{R_i}\right)^t X_{Ci}'\beta_C\right) \\ &\quad + \alpha_2(S) + \alpha_3(S)\log(R_i) + X_{Ti}'\beta_{TS} + u_{TSi}; \\ \log(w_{Sit}) &= \gamma_S + X_{Wi}'\beta_W + X_{Lit}'\beta_L + E_{it}\beta_E + \alpha_S\theta + \nu_{Sit}. \end{aligned}$$

In this set of estimates the market interest rate is fixed such that $1/(1+r) = \delta = .97$.

Table 9

Tests for the Presence of Borrowing Constraints
Calculated from Structural Model I (Estimates Reported in Table 8)
(Standard Errors in Parentheses[a])

Specification	Borrowing Rate[b] (1)	Negative Log-Likelihood (2)
A. Unrestricted:		2547.69
Everyone	1.031	
B. Racial Groups:		2547.34
Whites	1.031	
Blacks	1.000	
Hispanics	1.000	
C. Parents Education:		2547.42
Both College Educated	1.031	
Father 12, Mother 12	1.000	
Father 12, Mother Coll	1.000	
Father Coll, Mother 12	1.033	
D. Family Income:		2546.91
Top Third	1.031	
	(-)	
Middle Third	1.032	
	(0.006)	
Bottom Third	1.038	
	(0.008)	
E. Number of Siblings[c]:		2547.69
Zero	1.031	
	(-)	
Two	1.031	
	(0.003)	
Four	1.031	
	(0.006)	

[a]The standard errors are presented only for the family income variables and number of siblings because the model converged to a corner solution in the other cases.

[b]The "market rate of return" is fixed to be 1.031.

[c] In the results shown in panels B through D, the borrowing rate is interacted with the dummy variables shown. Here it is interacted with the number of siblings so the comparison group is the set of individuals with no siblings.

Table 10

Estimates of Structural Model II

Allowing For Unobserved Heterogeneity in Borrowing Rates[a]

A. Parameters of Borrowing Rate (R_i) Distribution			
Borrowing Rate[b]	1.031	1.071	
Probability	1.00	.00	
B. Wage Equation			
Black	-.095		
Hispanic	-.047		
Experience	.064		
Experience Squared	-.002		
AFQT Score	.007		
Current Local Earnings	.027		
Intercept-Dropout	1.046		
Intercept-HS Grad	1.139		
Intercept-Some Coll	1.031		
Intercept-Coll Grad	1.324		
Factor Loading-Dropout(α_0)	.176		
Factor Loading-HS Grad(α_1)	.155		
Factor Loading-Some Coll(α_2)	.153		
Factor Loading-Coll Grad(α_3)	.069		
Family Background Controls[c]	yes		
Geographic Controls[c]	yes		
Cohort Controls[c]	yes		
C. Taste Parameters (β_T)			
	Graduate High School	Attend College	Graduate College
Black	.632	1.725	1.873
Hispanic	.057	.838	.793
AFQT Score	.043	.088	.128
Father Dropout	-.144	-.235	-.108
Father Some Coll	.051	.678	.900
Father Coll Grad	.317	1.243	1.683
Mother Dropout	-.166	-.482	-.662
Mother Some Coll	.144	-.271	.336
Mother Coll Grad	.198	.650	1.244
Number of Siblings	-.007	-.113	-.099
Factor Loading (δ_T)	.000	2.130	2.130
Geographic Controls[c]	yes	yes	yes

(Continued on Following Page)

Table 10 (Continued)

D. Local Labor Market Parameters (β_l)

	Attend High School	Graduate High School	Attend College	Graduate College
Intercept			2.746	3.225
Local Earnings at Age 17	.408	.258		
Mean Local Earnings over Life	.245	.172	.300	.300
E. Additional Parameters				
Scale	.231			
High School Nesting (ρ_H)	.452			
College Nesting (ρ_C)	1.000			

[a]See the base of Table 8 for a description of the model equations.

[b]The "market rate of return" is fixed to be 1.031.

[c]Cohort and geographic controls are defined at the base of Table 2. Family background controls are the same as those used in the wage regressions reported in Tables 3.