Liberty for More: Finance and Educational Opportunities

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ABSTRACT

U.S. banking reforms—which reduced interest rates—boosted college enrollment rates among able students from middle-class families. We define "able" students as those with learning aptitude scores in the top two-thirds of the U.S. population. We define "middle class" as families in which both parents are not highly educated (more than 12 years of education) and that are neither in the bottom fourth nor in the top 10 percent of the family income distribution in the United States. Our findings suggest that credit conditions, the ability of an individual to benefit from college, and a family's financial and educational circumstances combine to shape college decisions. The functioning of the financial system plays a powerful role in shaping the degree to which a child's educational choices—and hence economic opportunities—are defined by parental income.

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1. INTRODUCTION

Consider two observations about education and income: First, there is a positive association between education and income. On average, people who receive more education go on to earn higher incomes. Second, there is a positive association between parents' income and the education of their children. Thus, higher-income parents tend to have children who receive more education, and that additional education is associated with those children earning higher incomes as adults.[1]

Those two observations motivate some hotly debated questions among social scientists: If the returns to education are so high, why do children from lower-income families obtain less education than children from higher-income families? If education boosts future incomes, what constrains children from lower-income families from attaining more education?

One response to those questions focuses on the costs: lower-income families do not have the money—and borrowing is too expensive—to pay for more education for their children (Becker 1975; Kane 1994; Kane and Rouse 1999; Ellwood and Kane 2000; Belley and Lochner 2007; Brown, Scholz, and Seshadri 2012). From that perspective, financial reforms that lower interest rates will boost education, including college enrollment rates, among children from disadvantaged families that were unable to afford college when credit was more expensive.

A second response stresses the benefits. The returns to higher education for children from disadvantaged families are comparatively low, and so those families invest less in education (Cameron and Heckman 1998, 2001; Shea 2000; Keane and Wolpin 2001;

[1] See Manski and Wise (1983); Behrman and Taubman (1990); Hauser (1993); Kane (1994); Mayer (1997); Cameron and Heckman (1998, 2001); Shea (2000); and Carneiro and Heckman (2002).

Carneiro and Heckman 2002; Keane 2002). That is, if children from lower-income families experience family, community, and grade school environments that are less conducive to their cognitive and noncognitive development than children from higher-income families, then their expected benefits from attending college will be correspondingly lower. Consequently, by the time students are deciding whether to work or go to college, their childhood environments have already determined their expected benefits from going to college. From that perspective, financial reforms that reduce the cost of credit will have a minor effect as disadvantaged families accurately view the expected benefits as low.[2]

To assess those responses empirically, numerous empirical studies use indirect methods (that do not rely on directly observing interest rates) to infer whether interest rates materially shape educational choices. They use indirect methods because of the difficulties associated with identifying exogenous changes in interest rates. As we will review in greater detail, a large body of research devoted to estimating the causal effects of schooling on income (see Card 1999, 2001) has found that the instrumental variable estimates of the returns to schooling exceed the ordinary least squares (OLS) estimates. In theory, credit-constrained individuals will have higher returns to education on the margin than less constrained individuals. That theory implies that if the instrumental variables capture the schooling choices of credit-constrained individuals more than others, the instrumental variable estimates will be larger (Lang 1993; Card 1999, 2001). However, it might be inappropriate to interpret the differences between instrumental variable and OLS estimates as reflecting the effect of the cost of credit on educational choices because other factors can produce those differences (Carneiro and Heckman 2002; Cameron and Taber 2004).

A second but much more limited body of research directly assesses the effect of interest rates on education, but this line of inquiry has serious methodological limitations. In an influential study, Card and Lemieux (2001) find that changes in U.S. interest rates do not account for changes in educational choices over the period

[2] Rather than focusing on reducing the cost of credit, this "benefits" view holds that the most efficacious way to boost college graduation rates among lower-income families is through early childhood interventions that enhance cognitive and noncognitive development and thereby boost the returns to education.

1968–1996. But it is inappropriate to treat the United States as an integrated capital market with a single interest rate during the 20th century with corresponding effects on state-level interest rates. State bank regulations differentially influenced the cost of credit in each state. And states reformed their regulations in different years during the second half of the 20th century. Consequently, both the level and dynamics of interest rates differ across states. Furthermore, since many factors might be correlated with both interest rates and education, it is important to use instrumental variables to identify the effect of interest rates on education.

By integrating labor and financial economics, we contribute to the study of the effect of credit conditions on educational choices in several ways. First, we assess whether state-specific banking reforms that intensified competition among banks and reduced state-specific interest rates increased the probability that students from those states attended college. Previous research on education and credit conditions both failed to recognize that U.S. credit markets were highly segmented because of state-specific regulations on banks for virtually the entire 20th century and failed to exploit the cross-state heterogeneity in the timing of banking reforms that lowered interest rates. In one of the largest—if not the largest—financial regulatory reforms in the history of the United States, every state relaxed geographic restrictions on bank branching—intrastate bank branching reform—during the second half of the 20th century. The state-specific timings of those deregulations were independent of interest rates and education. Although those intrastate bank branch deregulations eased credit conditions, researchers have not—to the best of our knowledge—previously assessed the effect of those regulatory reforms on educational choices.

Second, using state-level bank branch deregulation as an instrumental variable for interest rates, we assess whether this component of state-level interest rates affects the probability that students from that state attend college. However, one must be cautious in drawing sharp inferences from the instrumental variable analyses because bank branch deregulation does not necessarily satisfy both conditions for a valid instrument. Although there is no evidence that educational choices or interest rates influence the timing of bank deregulation in a state, the exclusionary condition might not hold. For example, bank deregulation might accelerate economic

activity and boost the demand for skilled workers, encouraging more students to attend college. Put differently, exogenous bank branch deregulation might reduce interest rates and boost college enrollment rates, but it might not boost college enrollments by lowering interest rates; it might boost college enrollments by increasing the demand for skilled workers. Thus, to interpret the instrumental variable analyses as providing information about the effect of an exogenous change in the cost of borrowing on the decision to attend college, we separately evaluate whether demand-side factors are driving the results.

Third, we assess whether an easing of credit conditions triggered by intrastate bank branch deregulation influenced only those particular children within particular families implied by Becker's (1967) model of human capital accumulation. The model suggests that the effect of lowering interest rates on attending college depends in an interactive manner on family income and the ability of the individual child to benefit from college. That framework suggests that a reduction in interest rates will have a larger effect on high-ability students who would benefit materially from college but whose parents were previously unable to afford college than it will on high-ability children from families that are unconstrained when making decisions about college. Thus, the model predicts that the effect of a change in credit conditions will differ depending on the ability of the individual student to benefit from college and on the ability of the family to pay for college. We assess that prediction using both the simple reduced-form analyses of education and bank deregulation and the instrumental variable analyses that use bank deregulation as an instrument for interest rates.

To make those contributions, we primarily use the National Longitudinal Survey of Youth 1979 (NLSY79) because of its unique characteristics. First, the NLSY79 traces individuals through time so that we know the educational attainment of each person. Second, the NLSY79 contains information on learning aptitude. It gave respondents the Armed Forces Qualification Test (AFQT) in 1980 when they were between the ages of 14 and 22. We use the AFQT score as a measure of learning ability, that is, the ability to benefit from education. Third, the NLSY79 has information about each respondent's family, including family income in 1979 and the educational attainment of both the mother and father. Given the tight connection between

education and income and the problems associated with measuring permanent income using only one year of data, we sometimes use parental education as a proxy measure of the permanent income of the family instead of family income in 1979. Fourth, the NLSY79 has information on two psychometric traits as measured in 1980: (a) self-esteem and (b) the degree to which the person believes that chance, fate, and luck control his or her life. Thus, in assessing the effect of changes in credit conditions on educational choices, we can control for many individual and family characteristics.

To complement the NLSY79, we use the Current Population Survey (CPS), which surveys more people than the NLSY79, but it does not contain information on learning ability, parental education, or personality traits. Thus, we use the CPS to make broader assessments about the effect of bank deregulation on interest rates and the Mincerian[3] returns to education, and we use the NLSY79 to assess how changes in credit conditions influence the decision of individuals to attend college.

We find that intrastate bank deregulation substantially increased the probability that individuals with particular learning abilities and family traits attended college. Specifically, bank deregulation had no effect on students in the lower third of the distribution of learning ability as measured by AFQT; for students for whom the expected benefits of college are low, changes in credit conditions have no appreciable effect on the probability of attending college. But bank deregulation did boost the probability that "able" students—students in the upper two-thirds of the AFQT distribution—go to college. For example, five years after a state deregulated, the probability that able students attend college was 13 percent greater than before deregulation. Moreover, and consistent with theory, an easing of credit conditions has the biggest effect on the able students from families in which both parents have a relatively low level of education (fewer than 12 years of completed schooling). Indeed, for able students from families in which both parents have more than 12 years of education, bank deregulation has no effect. To the extent that parental education is an accurate signal of the family's permanent income (or the family's taste for education), changes in credit conditions have little influence on the decisions of highly educated, affluent parents

[3] See Mincer (1974).

to send their children to college. However, in more disadvantaged families (as measured by parental education), bank deregulation has a large effect: five years after deregulation, able students from disadvantaged families have an almost 20 percent greater probability of attending college. The results are consistent with the view that credit conditions materially influence the educational opportunities of a particular segment of society: able students from disadvantaged families.

When dividing the sample by family income instead of parental education, we find that easing intrastate bank deregulation boosted college enrollment rates among able students from middle-class and upper-middle-class families. Even among students in the upper two-thirds of the AFQT distribution, an easing of credit conditions did not influence children from lower-income families (below the 25th percentile of the income distribution) or high-income families (above the 90th percentile). At those income levels, marginal changes in interest rates did not alter decisions about college. However, for able students from families with incomes between the 50th and 75th percentile of the income distribution, bank deregulation materially altered college decisions.

The results are very similar when we use bank deregulation as an instrumental variable for interest rates. Only for able students from middle-class and upper-middle-class families is the reduction in interest rates associated with an increase in the probability of attending college. For lower-income families or high-income families, such changes in the cost of credit do not influence college decisions. And reductions in interest rates do not increase the probability of attending college among students with AFQT scores in the bottom third of the sample. Consistent with theory, changes in the cost of credit influence a particular but meaningful segment of society.

Finally, we show that our results do not simply reflect the effect of intrastate bank deregulation on the demand for skilled labor. Rather, bank deregulatory reforms boosted college enrollment rates among able students from middle-class and upper-middle-class families, partially by lowering the costs of credit. In particular, a legitimate concern with our analyses is that perhaps branch deregulation boosted economic activity (Jayaratne and Strahan 1996) and thereby boosted the demand for skilled labor. Perhaps that "demand-side"

effect is driving the increase in college enrollments, leading us to misinterpret the findings as arising from a reduction in the "cost of credit." Although reasonable conceptually, the demand-side channel is not the only channel through which bank deregulation increased college enrollments. If the results were purely a demand-side effect, then bank deregulation should boost the demand for college-educated workers and the returns to a college education. But we show that bank deregulation reduced the returns to a college education, which is fully consistent with a reduction in costs boosting the supply of college-educated workers. Although we do not rule out the demand-side channel as a contributing factor, the findings suggest that the supply side mattered too, as deregulation eased credit conditions and that boosted the supply of college-educated workers.

The remainder of this paper is organized as follows: Section 2 sketches the theoretical framework and its empirical predictions. Section 3 provides a literature review and details how we propose to contribute to existing research. Section 4 discusses the data on bank deregulation, interest rates, and education. Section 5 presents the results, and Section 6 concludes.

2. THEORETICAL FRAMEWORK AND EMPIRICAL PREDICTIONS

Building directly on Becker (1967) and Mincer (1974), this section first presents a theoretical model of human capital and then details the empirical predictions emerging from the theory. In its simplest form, the Becker (1967) model assumes that each individual i maximizes the discounted present value of lifetime earnings, $W(s_i)$, by choosing the optimal level of investment in human capital, s_i, which we call "schooling" or "education" but which represents all investments in human capital skills that boost earnings.

$$W(s_i) = \int_{t=s}^{\infty} y(s_i) \exp(-(r_i - \theta_i))\, dt \tag{1}$$

where $y(s_i)$ denotes the annual earnings of an individual with a schooling level s_i; r_i is the interest rate facing individual i, which reflects his or her cost of capital and subjective rate of time preference; and θ_i is the individual's preference for schooling over work. For simplicity, we assume an infinite planning horizon.

To complete the model, let earnings reflect the spot market value of a unit of human capital (P) multiplied by the individual's stock of accumulated human capital, $H(s_i)$:

$$y(s_i) = P \times H(s_i) \tag{2}$$

Further, following Griliches (1977) and more recently Card (2001), we define the human capital production function as follows:

$$H(s_i) = \exp(a_i s_i - \frac{\gamma}{2} s_i^2 + \mu_i \tag{3}$$

where individual i's human capital is positively related to his or her schooling (s_i), ability to benefit from schooling (a_i), and initial level of general skills (μ_i). The beneficial effects of additional schooling face diminishing returns (γ), which we assume to be the same across all individuals for simplicity.

Solving, the optimal level of schooling for individual i (s_i^*) is

$$s_i^* = \frac{a_i - r_i + \theta_i}{\gamma} \tag{4}$$

Across individuals, differences in the optimal amount of education reflect differences in the ability to benefit from education (a_i)—modeled as the technological efficiency with which learning time, effort, and resources augment the value of human capital—the cost of credit (r_i), and the (dis)utility from schooling (θ_i). Clearly, if an individual's marginal benefit from education with respect to future income is relatively large (i.e., a large a_i), then that individual will tend to invest relatively more in schooling than a low-ability person. If an individual's preference for education, θ_i, is relatively high, then such an individual will invest more in education than comparable individuals with weaker tastes for schooling. The model is silent about the source of heterogeneity in the "ability to benefit" from education (Ben-Porath 1967) and the "ability to pay." Separating the ability to benefit from education and the ability to pay for it is challenging, especially since family and community environments affect both.

Although it is appropriate to model human capital as a stock and investment in human capital as a flow, schooling—especially higher-level education—is often a discrete choice. Therefore, there are discrete educational choices, such as attending college, for which the effect of easing credit conditions will depend on the initial conditions facing the family and individual student. For example, a high-ability student in a high-income family that has a strong taste for education

might have an optimal level of schooling that includes college and even a postgraduate degree. In this case, lowering interest rates will not affect the decision to attend college. As another example, a low-ability student from a disadvantaged family with weak tastes for education might have an optimal level of schooling that does not even include graduating from high school. In this case, lowering interest rates is unlikely to affect the student's decision to attend college. However, for some high-ability children from families in which initial interest rates cause the expected costs of college to outweigh the expected benefits, an easing of credit conditions could influence the decision to go to college. Thus, the effect of credit conditions on the decision of an individual student in a particular family to attend college may depend materially on the student's ability to benefit from college, the initial financial conditions facing the family, and its taste for education.

3. THE LITERATURE AND OUR CONTRIBUTION

Why don't lower-income families invest more in the education of their children? As we noted in the Introduction, an enormous literature documents large disparities in high school and college graduation rates across family income groups over the 20th century. Since education is so highly correlated with income, those disparities motivate research on the persistence of inter–income group disparities in education.

We have already noted that the model highlights two major—though not mutually exclusive—explanations for why disadvantaged families invest comparatively little in the education of their children. The first emphasizes the costs: Lower-income families do not have the money to pay for more education and their borrowing costs are high. Those costs hinder lower-income families from providing the same level of education to their kids as higher-income families, perpetuating intergenerational income differences. From this perspective, lowering interest rates will lower the costs of education so that high-ability children from lower-income families can better afford college. Thus, improvements in financial systems can reduce inequalities of opportunity and the inefficient persistence of relative income differences.

The second explanation for why lower-income families do not invest more in education stresses the benefits: The children of

disadvantaged families frequently face lower expected returns to additional education; that is, their a_i's are low. Thus, children from disadvantaged families—which tend to provide family, community, and school environments less conducive to the cognitive and noncognitive development of their children—will disproportionately and accurately view college as a relatively low-return activity. From this perspective, lowering interest rates will not induce lower-income families to invest much more in sending their kids to college.

3.1 Existing Evidence

The evidence on whether the credit conditions influence educational choices is mixed and inconclusive. Given the difficulties associated with measuring the credit conditions facing individuals, a large body of research has used indirect methods—which do not require researchers to observe interest rates or other measures of credit conditions—to draw inferences about the influence of credit conditions on educational choices.

Numerous studies have tackled this question by studying the correlation between educational attainment and family income (or other family characteristics). The positive correlation between educational attainment and family income has been widely interpreted as evidence that borrowing constraints hinder educational choices (see, e.g., Kane 1994; Kane and Rouse 1999; Ellwood and Kane 2000; Belley and Lochner 2007; Brown, Scholz, and Seshadri 2012). However, the step from correlation to causation is a precarious one as family income is also strongly correlated with family resources that foster cognitive and noncognitive traits that boost the ability of a student to benefit from more education. The connection between family resources and the nurturing of cognitive and noncognitive traits that increase the productivity of formal schooling has been emphasized by Cameron and Heckman 1998, 2001; Shea 2000; Heckman and Rubinstein 2001; Keane and Wolpin 2001; Carneiro and Heckman 2002; Keane 2002; Cameron and Taber 2004; and Heckman, Stixrud, and Urzua 2006.

As we have already noted, a large literature finds that the instrumental variable estimates of the return to schooling exceed OLS estimates (see Card 1999, 2001). Credit conditions are one possible source of that difference between the estimates, a point first offered by Becker (1967). In particular, instrumental variable estimates can

be interpreted as estimating the return for those randomly assigned to schooling by the instrument. Finding higher returns using an instrumental variable is consistent with the view that those affected by the "instrument" are credit-constrained (Lang 1993; Card 1999, 2001) and is, therefore, consistent with interest rates curtailing the educational opportunities of lower-income families. Similarly, Shea (2000) finds that family income matters for children's human capital investment in a sample of low-income families, but not for the broader population.[4]

Substantial work, however, challenges the methodological efficacy of these indirect methods for drawing inferences about the effect of credit conditions on educational choices. For example, Cameron and Heckman (1998, 2001) and Carneiro and Heckman (2002) suggest that it is inappropriate to interpret the difference between OLS and instrumental variable estimates in the sample of low-income families as signaling the importance of liquidity constraints, criticizing econometrically the use of invalid instruments and pointing economically to alternative explanations, including sorting for schooling on comparative advantage.

Hence, larger coefficients in instrumental variable regressions of income on education might not imply the existence and effect of interest rates on schooling. That is, without directly measuring exogenous changes in interest rates, it is difficult to distinguish between cross-family differences in interest rates (r_i) and attitudes toward education (θ_i). Furthermore, Cameron and Taber (2004) question the robustness of the instrumental variable results to using alternative instruments. Keane and Wolpin (2001) estimate a structural model of schooling behavior and find that relaxing interest rates tends to increase consumption, not investment in education

A much more limited set of papers assesses the direct linkage between interest rates and schooling decisions. As a leading example, Card and Lemieux (2001) find that changes in U.S. interest rates over the period 1968–1996 do not account for changes in educational choices.

However, the direct approaches taken so far have two key limitations. First, it is inappropriate to treat the United States as

[4] Researchers also examine the effect of targeted credit programs on education, such as the CalGrant program in California for college-bound students (Kane 2003) or Head Start.

an integrated capital market with a single interest rate, especially with regard to household loans during the 20th century. Each state exerted a powerful regulatory role over banks until the mid-1990s so that interest rates and their evolution over time differ markedly by state. Second, it is valuable to identify an exogenous source of variation in credit conditions to assess the effect of interest rates on educational choices. Some third factor, such as aggregate economic activity, could affect both interest rates and education decisions, creating a spurious correlation between them.

3.2 Our Contribution

We propose to contribute to existing research in the following interrelated ways:

First, we will *directly* examine the relationship between educational choices and credit conditions, as measured by exogenous changes in bank regulations that lowered interest rates. This examination contrasts with the large literature that draws inferences about the importance of credit conditions in explaining educational choices through indirect methods, that is, by examining differences between instrumental variable and OLS coefficient estimates of the relationship between wages and education.

Second, we will assess the effect of the exogenous relaxation of regulatory restrictions on bank branching (which lowered interest rates) on college enrollment rates. As we will describe in greater detail, those deregulations occurred across all states in varying years during the second half of the 20th century. Since those state-level regulatory reforms occurred in different years, we control for all national influences by including year fixed effects. Furthermore, whereas past studies take the United States as an integrated financial system with one interest rate, we allow interest rates to differ at the state-year level. This allowance is crucial for drawing accurate inferences about the relationship between credit conditions and educational choices because state regulations heavily and differentially influenced credit conditions across the U.S. states for much of the 20th century, and those regulations were liberalized in different years in different states.

Third, we assess the relationship between interest rates and college enrollment rates, using exogenous cross-state, cross-year variation in bank deregulation as an instrumental variable for interest rates. Unlike much existing work, using instrumental variables is valuable

because interest rates and schooling choices might be simultaneously determined. In these analyses, we argue that bank deregulation is exogenous to educational choices, but we do not claim that bank deregulation influences schooling only through its effect on interest rates; we do not claim that bank deregulation satisfies the exclusionary restriction. In particular, bank deregulation might boost the demand for skilled workers and thereby induce more people to attend college. Despite these limitations, we present evidence that deregulation boosted college enrollment rates by reducing the cost of college, not simply by increasing the demand for skilled workers.

Fourth, we provide an empirical bridge between those researchers who focus on the costs of education and those who focus on the benefits of education in seeking to explain why the children of lower-income families tend to obtain less education. To do so, we will evaluate the effect of easing credit conditions ("costs of education") on an individual's educational choices while differentiating by proxies for the person's learning aptitude ("benefits of education") and the family's initial conditions as measured by family income and the education of the parents. Thus, we will assess how the costs and benefits of college combine to shape an individual's educational choices.[5]

4. DATA: BANK DEREGULATION, INTEREST RATES, AND EDUCATION

Geographic restrictions on banks have their origins in the U.S. Constitution, which limited states from taxing interstate commerce and issuing fiat money. In turn, states raised revenues by chartering banks and taxing their profits. Since states received no charter fees from banks incorporated in other states, state legislatures prohibited the entry of out-of-state banks through interstate bank regulations. To maximize revenues from selling charters, states also effectively granted local monopolies to banks by restricting banks

[5] Furthermore, by differentiating by each individual's cognitive abilities and the traits of each person's parents, the framework advertises an additional line of inquiry: credit conditions can influence the ability of a person to benefit from college. For example, if a reduction in interest rates allows a family to purchase a home in a better school district and that school district enhances the cognitive and noncognitive capabilities of the children, then interest rates can increase the returns from additional education for those children. A reduction in r_i can boost a_i, with a concomitant increase in education and lifetime earnings. We are pursuing this line of research in a companion paper.

from branching within state borders. The intrastate branching restrictions frequently limited banks to operating in one city.

By protecting inefficient banks from competition, geographic restrictions created a powerful constituency for maintaining those regulations even after the original fiscal motivations receded. Indeed, banks protected by those regulations successfully lobbied both the federal government and state governments to prohibit interstate banking and intrastate branching (White 1982).

In the second half of the 20th century, however, technological, legal, and financial innovations diminished the economic and political power of banks benefiting from geographic restrictions. In particular, a series of innovations lowered the costs of using distant banks. Those lower costs reduced the monopoly power of local banks and weakened their ability and desire to lobby for geographic restrictions. For example, the invention of automated teller machines (ATMs), in conjunction with court rulings that ATMs are not bank branches, weakened the geographical link between banks and their clientele. The creation of checkable money market mutual funds made banking by mail and telephone easier, further weakening the power of local bank monopolies. Finally, the increasing sophistication of credit-scoring techniques, improvements in information processing, and the revolution in telecommunications reduced the informational advantages of local bankers, especially with regard to small and new firms.

Those national developments interacted with preexisting state characteristics to shape the timing of bank deregulation across the states and the District of Columbia, as listed in Table 1. As shown by Kroszner and Strahan (1999), deregulation occurred later in states where potential losers from deregulation (small, monopolistic banks) were financially stronger and had a lot of political power. On the other hand, deregulation occurred earlier in states where potential winners of deregulation (small firms) were relatively numerous. Thus, unlike many types of regulatory reforms that occur at the national level, geographic bank deregulation took place on a state-by-state basis over an extended period.

4.1 Interest Rates

To measure the cost of credit, we use data on mortgage rates at the state-year level. Since consumers frequently use their homes as collateral, those rates provide information on general credit

Table 1
Dates of Intrastate Bank Branch Deregulation, by States

State	Date	State	Date
Alabama	1981	Montana	1990
Alaska	1960	Nebraska	1985
Arizona	1960	Nevada	1960
Arkansas	1994	New Hampshire	1987
California	1960	New Jersey	1977
Colorado	1991	New Mexico	1991
Connecticut	1980	New York	1976
District of Columbia	1960	North Carolina	1960
Florida	1988	North Dakota	1987
Georgia	1983	Ohio	1979
Hawaii	1986	Oklahoma	1988
Idaho	1960	Oregon	1985
Illinois	1988	Pennsylvania	1982
Indiana	1989	Rhode Island	1960
Iowa	1999	South Carolina	1960
Kansas	1987	Tennessee	1985
Kentucky	1990	Texas	1988
Louisiana	1988	Utah	1981
Maine	1975	Vermont	1970
Maryland	1960	Virginia	1978
Massachusetts	1984	Washington	1985
Michigan	1987	West Virginia	1987
Minnesota	1993	Wisconsin	1990
Mississippi	1986	Wyoming	1988
Missouri	1990		

Source: Kroszner and Strahan (1999).

conditions. The mortgage interest rate data are based on a monthly survey of major lenders that are asked to report the terms and conditions on all conventional, single-family, fully amortized, purchase-money loans closed in the last five working days of the month. The data exclude Federal Housing Administration–insured and Veterans Administration–guaranteed mortgages, refinancing loans, and balloon loans. The "effective interest rate" includes the amortization of initial fees, points, and charges over a 10-year period, which is the historical assumption of the average life of a mortgage loan and is computed by the Federal Housing Finance Board. We then deflate by the national consumer price index.

4.2 Education and Other Individual-Level Data

The NLSY79 is a nationally representative sample of 12,686 young men and women who were 14–22 years old when they were first surveyed in 1979. Interviewees were initially surveyed annually, and then on a biennial basis after 1994. The NLSY is made up of three subsamples: (a) a random sample of 6,111 noninstitutionalized civilians; (b) a supplemental sample of 5,295 people designed to oversample civilian Hispanics, blacks, and economically disadvantaged whites; and (c) a sample of 1,280 people who were ages 17–21 as of January 1, 1979, and who were enlisted in the military as of September 30, 1978. We use the random sample and the black and Hispanic oversamples and use the weights provided by the NLSY79 to obtain a representative sample of the U.S. population.

In the analyses, we control for information on family background, including family income and the highest grade completed by a person's mother and father. *Family Income in 1979* measures the income of the individual's household in 1979, computed in 2010 dollars. (In the regressions, we divide *Family Income in 1979* by $100,000.) As shown in Table 2, the mean value is about $62,300, and the median value is about $56,400. Some values are missing for *Family Income in 1979*. Consequently, when we use *Family Income in 1979* as a regressor, we impute the sample mean and include a dummy variable that equals 1 for observations in which *Family Income in 1979* is missing. When we use *Family Income in 1979* to divide the sample, we do not impute a value for missing observations and instead use a smaller sample of individuals. Therefore, the number of observations is not identical in these different specifications.

Table 2
Summary Statistics

	Mean	Median	Standard Deviation
Attended College	0.52	1.00	0.50
Years since Deregulation	3.65	0.00	7.16
AFQT Percentile	51.30	51.88	28.86
External Locus of Control Score	8.48	8.00	2.39
Self-Esteem Score	22.57	22.00	4.07
Mother's Education	11.68	12.00	2.79
Father's Education	11.83	12.00	3.57
Family Income in 1979 (2010 dollars)	$62,302	$56,430	$42,221
Interest Rate	5.29	5.17	2.68

Note: *Attended College* equals 1 if the individual attended college for any period of time and 0 otherwise. *Years since Deregulation* equals the number of years since the state deregulated restrictions on intrastate branch banking and is computed for the year 1979. *AFQT Percentile* is the individual's Armed Forces Qualification Test score as a percentile of the entire National Longitudinal Survey of Youth 1979 (NLSY79) sample, which is measured in 1980, and where 50 is the median of the NLSY79 representative sample. *External Locus of Control Score* (computed in 1980) measures the degree to which individuals believe that external factors such as chance, fate, and luck control their lives relative to the degree that the individual has internal control over his or her life, where values range from 4 to 16—higher values signify more external control; *Self-Esteem Score* (computed in 1980) measures the degree of approval or disapproval of one's self, where values range from 6 to 30— higher values signify greater self-approval. *Mother's* and *Father's Education* measure the number of years of education of the mother and father, respectively. *Family Income in 1979* measures the income of the individual's household in 1979, computed in 2010 dollars. In the regression tables, we divide *Family Income in 1979* by $100,000. *Interest Rate* is the annual real interest rate based on mortgage rates from a monthly survey of major lenders that are asked to report the terms and conditions on all conventional, single-family, fully amortized, purchase-money loans closed in the last five working days of the month. The data exclude FHA-insured and VA-guaranteed mortgages, refinancing loans, and balloon loans. The rate includes the amortization of initial fees, points, and charges over a 10-year period, which is the historical assumption of the average life of a mortgage loan and is computed by the Federal Housing Finance Board. We deflate by the national Consumer Price Index. For variables from the NLSY79, the reported summary statistics use the NLSY79 sample weights.

The major dependent variable is the binary indicator *Attended College*, which equals 1 if the individual attended college and 0 otherwise. This variable equals 1 for individuals who attended college but did not graduate, those who graduated from college, and those who went on to graduate school. As shown in Table 2, about half of the individuals attended college. We focus on whether the person attended college since that focuses on the decision of whether to go to college or work, which is the central decision in the theoretical framework. The results, however, are robust to focusing on whether the individual graduated from college or not.

Key explanatory variables are as follows:

AFQT Percentile is the individual's AFQT score as a percentile of the entire NLSY79 sample, where the *AFQT Percentile* is measured in 1980. A percentile of 50 is the median of the NLSY79 sample, but the median is 51 for our main regression sample because of missing values on parental education. The AFQT is a weighted sum of four tests from the 10-part Armed Services Vocational Aptitude Battery. We use the *AFQT Percentile* as a proxy of an individual's "ability to benefit from additional education." To guarantee that school attendance did not influence AFQT test scores, the AFQT score is standardized by the age of the individual at the time of the test (Cameron and Heckman 1993; Neal and Johnson 1996; and Altonji and Pierret 2001). We exclude observations with missing AFQT scores, parental education, state of residency, and education attainment.

External Locus of Control Score (computed in 1980) measures the degree to which individuals believe that external factors such as chance, fate, and luck control their lives relative to the degree that the individual has internal control over his or her life. Values range from 4 to 16, with higher values signifying perceptions of greater external control. The average value is 8.5.

Self-Esteem Score (computed in 1980) measures the degree of approval or disapproval of one's self. Values range from 6 to 30, with higher values signifying greater self-approval. As shown in Table 2, the mean value of *Self-Esteem Score* is about 22.6, with a standard deviation of 4.

In some specifications, we use the CPS. Specifically, we use the March Annual Demographic Survey files for the sample years 1976–2007, taken from the Integrated Public Use Microdata Series. We use this much larger sample of individuals to compute the Mincerian returns to education as discussed below.

5. RESULTS

We begin by assessing the effect of bank deregulation on interest rates using the following specification:

$$r_{jt} = \alpha D_{jt} + \beta D_{jt}^2 + \delta_j + \delta_t + e_{jt} \tag{5}$$

In the equation, r_{jt} is a measure of real interest rates in state j in year t, δ_j and δ_t are vectors of state and year fixed effects, and e_{jt} is the error term. The variables of interest, D_{jt}, and D_{jt}^2, equal the number of years since state j deregulated (and 0 before state j deregulated) and the square of the number of years since deregulation. In total, we have data for 48 states plus the District of Columbia. Consistent with the literature on bank branch deregulation, we eliminate Delaware and South Dakota because the structure of their banking systems was heavily affected by laws that made them centers for the credit card industry.

Table 3 shows that intrastate bank deregulation was associated with a substantial reduction in interest rates when controlling for year and state effects. The significant negative relationship between interest rates and bank deregulation only emerges when controlling for both year and state effects. This finding is consistent with the view that capital markets were segmented across the states and that one needs to abstract from national fluctuations in credit conditions to identify the independent effect of state-level deregulations on state interest rates.

5.1 Bank Deregulation and College: Reduced Form Results

We next assess the reduced form relationship between the removal of geographic restrictions on banks and college enrollment, where we differentiate individuals by AFQT scores and by parental education or family income. Since family income in one year might be a poor proxy for permanent income and since education is highly correlated with income, parental education might provide a more accurate signal of the family's long-run financial situation.

According to the theoretical framework above, we should consider the linear-in-the-parameters probability model for whether a person attends college (s_{ij}):

$$s_{ij} = \beta_a AFQT_i + \beta_{D1} D_{j,79} + \beta_{D2} D_{j,79}^2 + \beta_X X_{ij} + \varepsilon_{ij} \tag{6}$$

where the subscript i indicates a person and j designates a state. We include one observation per person. We use the AFQT score to proxy

Table 3
Bank Deregulation and Interest Rates

	(1)	(2)	(3)	(4)
Years since Deregulation	−0.007	−0.016	−0.007	−0.025**
	(0.012)	(0.013)	(0.009)	(0.012)
Years since Deregulation Squared	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	5.170***	5.180***	2.020***	2.010***
	(0.068)	(0.091)	(0.052)	(0.071)
Year Effects	No	No	Yes	Yes
State Effects	No	Yes	No	Yes
Observations	1,274	1,274	1,274	1,274
R-Square	0.001	0.009	0.971	0.978

Note: This table presents the results of four regressions, where the dependent variable equals the *Interest Rate*, which is computed at the state-year level. *Interest Rate* is the effective interest on mortgages, which includes amortization of initial fees, points, and changes over a 10-year period and is computed by the Federal Housing Finance Board. Consistent with previous research, the sample includes data on the District of Columbia and all states except Delaware and South Dakota, which are dropped because of large concentrations of credit card banks. The sample covers the period from 1976 through 2002. Regressors: *Years since Deregulation* equals the number of years since the state deregulated restrictions on intrastate branch banking. This number varies at the state-year level. Standard errors are clustered at the state level and corrected using the Huber-White adjustment. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

for an individual's "ability to learn." In many specifications, we split the sample by AFQT score to assess whether the effect of bank deregulation on educational choices differs by a student's "ability to learn." $D_{j,79}$ is the number of years since deregulation for state j in 1979. We choose 1979 because it is the first year of the NLSY79 and because it corresponds to a period in the lives of respondents when interest rates and credit conditions are likely to influence educational choices. Survey respondents are between the ages of 14 and 22 in 1979. By using a quadratic for years since deregulation, we allow for changes in credit conditions to have nonlinear effects on educational choices.

As additional regressors, X_{ij}, we include the following: *External Locus of Control Score* (computed in 1980) measures the degree to which individuals believe that external factors such as chance, fate, and luck control their lives relative to the degree that the individual has internal control over his or her life. *Self-Esteem Score* (computed in 1980) measures the degree of approval or disapproval of one's self. *Mother's Education* and *Father's Education* measure the number of years of education of the mother and father, respectively. *Family Income in 1979* measures the income of the individual's family in 1979, computed in 2010 dollars and divided by $100,000 (as a regressor). In several specifications, we split the sample according to parental education or family income so we can assess whether the effect of deregulation on college enrollment differs by those family characteristics. In all specifications, we control for regional, racial, gender, and year-of-birth effects, and we include a dummy variable that equals 1 if we imputed *Family Income in 1979*.

Tables 4 and 5 provide the regression results, where Table 4 provides the OLS estimates and Table 5 gives the probit results. In both tables, column (1) provides the results for the full sample; columns (2) and (3) provide results splitting the sample between those with *AFQT Percentile* above and below 33, respectively. The NLSY79 sample mean value of *AFQT Percentile* is 50. In columns (4) and (5), we consider only individuals with AFQT scores above 33 and further split the sample by the education of the parents: column (4) includes individuals where either parent has 12 years or fewer of education, and column (5) includes individuals where both parents have more than 12 years of education. Since the dependent variable is binary, we focus on the results using probit regressions. The OLS regressions yield very similar inferences. For the probit analyses, the reported coefficients are the estimated marginal effects, evaluated at the mean values of the regressors. The standard errors are clustered at the state level and corrected for heteroskedasticity using the Huber-White adjustment.

Bank regulation boosted the probability that individuals with particular abilities and family traits attend college. In particular, bank deregulation has no effect on relatively low-ability people, that is, people with AFQT scores lower than 33 (Table 5, column [3]). But deregulation does have a positive effect on "able" students—students with AFQT scores greater than 33 (Table 5, column [2]). For instance,

Table 4

College and Bank Deregulation by Ability and Parents' Education: Ordinary Least Squares

	(1)	(2)	(3)	(4)	(5)
Sample:	**All**	**AFQT >33**	**AFQT <33**	**AFQT >33 and Either Parent ≤12 Years of Education**	**AFQT >33 and Both Parents >12 Years of Education**
Years since Deregulation in 1979	0.025**	0.035***	0.003	0.041***	0.024
	(0.012)	(0.012)	(0.014)	(0.012)	(0.014)
Years since Deregulation in 1979 Squared	−0.001*	−0.002***	0.000	−0.002***	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
AFQT Percentile	0.008***	0.009***	0.009***	0.009***	0.005***
	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)
External Locus of Control Score	−0.001	−0.003	0.002	−0.004	0.002
	(0.002)	(0.003)	(0.003)	(0.003)	(0.005)
Self-Esteem Score	0.009***	0.007***	0.014***	0.007***	0.008**
	(0.002)	(0.002)	(0.003)	(0.002)	(0.004)
Mother's Education	0.014***	0.021***	0.003	0.017***	0.006
	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)
Father's Education	0.015***	0.018***	0.009**	0.015***	0.014**
	(0.002)	(0.002)	(0.004)	(0.002)	(0.006)
Family Income in 1979	0.153***	0.157***	0.021	0.258***	0.021
	(0.046)	(0.049)	(0.104)	(0.072)	(0.071)
Observations	8,534	4,737	3,797	3,836	901
R-Square	0.334	0.245	0.133	0.198	0.184

Note: This table presents the results of five ordinary least squares regressions, where the dependent variable equals 1 if the individual attended college and 0 otherwise. There is one observation per person. Regressors: *Years since Deregulation in 1979* equals the number of years since the state deregulated restrictions on intrastate branch banking and is computed for the year 1979. *AFQT Percentile* is the individual's Armed Forces Qualification Test score as a percentile of the entire National Longitudinal Survey of Youth 1979 (NLSY79) sample, which is measured in 1980, and where 50 is the median of the NLSY79 representative sample. *External Locus of Control Score* (computed in 1980) measures the degree to which individuals believe that external factors such as chance, fate, and luck control their lives relative to the degree that the individual has internal control over his or her life, where values range from 4 to 16—higher values signify more external control. *Self-Esteem Score* (computed in 1980) measures the degree of approval or disapproval of one's self, where values range from 6 to 30—higher values signify greater self-approval. *Mother's* and *Father's Education* measure the number of years of education of the mother and father, respectively. *Family Income in 1979* measures the income of the individual's household in 1979, computed in 2010 dollars and divided by $100,000. The regression includes regional, racial, and gender fixed effects; the individual's year of birth; and a dummy variable that equals 1 if we imputed *Family Income in 1979* with the sample mean because of missing values. Sample: Besides the core NLSY sample, we also include the supplementary data set on blacks and Hispanics. Standard errors are clustered at the state level and corrected using the Huber-White adjustment. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 5

College and Bank Deregulation by Ability and Parents' Education: Probit

	(1)	(2)	(3)	(4)	(5)
Sample:	**All**	**AFQT >33**	**AFQT <33**	**AFQT >33 and Either Parent ≤12 Years of Education**	**AFQT >33 and Both Parents >12 Years of Education**
Years since Deregulation in 1979	0.031 (0.019)	0.038** (0.017)	0.003 (0.016)	0.047*** (0.014)	0.016 (0.012)
Years since Deregulation in 1979 Squared	−0.002 (0.001)	−0. 002** (0.001)	−0.000 (0.001)	−0.002*** (0.001)	−0.001 (0.001)
AFQT Percentile	0.011*** (0.000)	0.010*** (0.000)	0.010*** (0.001)	0.011*** (0.001)	0.003*** (0.000)
External Locus of Control Score	−0.002 (0.003)	−0.004 (0.003)	0.002 (0.003)	−0.005 (0.004)	0.000 (0.003)
Self-Esteem Score	0.013*** (0.002)	0.009*** (0.002)	0.014*** (0.003)	0.008*** (0.002)	0.005*** (0.002)
Mother's Education	0.021*** (0.005)	0.027*** (0.005)	0.004 (0.005)	0.020*** (0.005)	0.004 (0.005)
Father's Education	0.022*** (0.003)	0.023*** (0.003)	0.010*** (0.004)	0.019*** (0.003)	0.011** (0.005)
Family Income in 1979	0.264*** (0.070)	0.264*** (0.070)	0.019 (0.103)	0.319*** (0.090)	0.042 (0.051)
Observations	8,534	4,737	3,797	3,836	901
R-Square	0.29	0.22	0.13	0.16	0.29

Note: This table presents the results of five probit regressions, where the dependent variable equals 1 if the individual attended college and 0 otherwise, and where the reported coefficients are the estimated marginal effects, evaluated at the mean values of the independent variables. There is one observation per person. Regressors: *Years since Deregulation in 1979* equals the number of years since the state deregulated restrictions on intrastate branch banking and is computed for the year 1979. *AFQT Percentile* is the individual's Armed Forces Qualification Test score as a percentile of the entire National Longitudinal Study of Youth 1979 (NLSY79) sample, which is measured in 1980, and where 50 is the median of the NLSY79 representative sample. *External Locus of Control Score* (computed in 1980) measures the degree to which individuals believe that external factors such as chance, fate, and luck control their lives relative to the degree that the individual has internal control over his or her life, where values range from 4 to 16—higher values signify more external control. *Self-Esteem Score* (computed in 1980) measures the degree of approval or disapproval of one's self, where values range from 6 to 30—higher values signify greater self-approval. *Mother's* and *Father's Education* measure the number of years of education of the mother and father, respectively; and *Family Income in 1979* measures the income of the individual's household in 1979, computed in 2010 dollars and divided by $100,000. The regression includes regional, racial, and gender fixed effects; the individual's year of birth; and a dummy variable that equals 1 if we imputed *Family Income in 1979* with the sample mean because of missing values. Sample: Besides the NLSY sample, we also include the supplementary data set on blacks and Hispanics. Standard errors are clustered at the state level and corrected using the Huber-White adjustment. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

1 year after deregulation, the probability of attending college rose by about 3.5 percent; 5 years after deregulation, the probability is 13 percent greater than before deregulation; 10 years after deregulation, the probability is 15 percent greater; and 15 years after, the probability is 8 percent greater than it was before the state deregulated restrictions on intrastate branching.

Table 5 shows that the able students from families in which neither parent has more than 12 years of education experience the biggest boost from bank deregulation in the probability of going to college. In particular, bank deregulation has no effect on attending college for individuals with AFQT scores above 33 but who are from families in which both parents have more than 12 years of education. But bank deregulation has a very large effect on attending college for able students from less well-educated parents. The estimated effects are large. For instance, 1 year after deregulation, the probability of attending college rises by about 4.1 percent; 5 years after deregulation, the probability is 16 percent greater than before deregulation; 10 years after deregulation, the probability is 21 percent greater; and 15 years after, the probability is 17 percent greater than it was before the state deregulated restrictions on intrastate branching.

Besides the main results on bank deregulation, the analyses also show that AFQT and parental education are positively associated with higher college enrollment even when splitting the sample by AFQT and parental income. Self-esteem is also a good predictor of whether an individual attends college, even after conditioning on the array of individual, family, regional, and national factors included in the regressions.

Overall, the results from Tables 4 and 5 are consistent with the theoretical model presented above. To the extent that people with AFQT scores in the bottom third of the distribution are unlikely to benefit from college, it is unsurprising that an improvement in credit conditions does not influence their probability of attending college. Similarly, to the extent that able students from well-educated parents have a high probability of attending college regardless of credit conditions, we would not expect bank deregulation to influence their likelihood of attending college either. Rather, theory and the evidence in Tables 4 and 5 indicate that changes in credit conditions influence a particular—though significant—segment of society: able students from more disadvantaged family backgrounds.

Table 6 continues these analyses by splitting the sample by different levels of *Family Income in 1979*, rather than by the education of the parents. In these analyses, we only consider "able" students, that is, students with AFQT scores above 33. We present five probit regressions of different samples, where we consider families with family income of (a) less than the 25th percentile of family income in our sample, (b) more than the 25th percentile, (c) more than the median, (d) more than the 75th percentile, and (e) more than the 90th percentile of family income in our full sample. In this paper, we simply present the probit regressions; the OLS regressions yield similar results.

We find that easing credit conditions—as proxied by intrastate bank deregulation—boosted college enrollments for able students from middle-class and upper-middle-class families. As shown in Table 6, changing credit conditions had no effect on college enrollment for students with AFQT scores above the 33rd percentile but who came from lower-income families (families with incomes below the 25th percentile). And changing credit conditions had no effect on college enrollment for able students from high-income families (families with incomes above the 90th percentile). When we consider people from the middle and upper-middle of the income distribution, bank deregulation exerted a positive effect on college enrollment rates. For families with incomes above the median, the results from equation (3) indicate that 5 years after bank deregulation, an able person's probability of attending college is on average 20 percent greater; 10 years after deregulation, it is 25 percent greater; even 15 years after deregulation, the probability of an able person attending college is 15 percent greater than it was before deregulation.

5.2 Two-Stage Least Squares

We now employ a two-stage least squares (2SLS) probit estimator to examine the effect of interest rates on the probability of attending college. That is, we estimate the following probit equation of whether a person attends college or not (s_{ij}):

$$(7) \qquad \Pr(s_{ij} = 1 \mid AFQT_i, r_i, X_{ij}) = \Phi(\beta_a AFQT_i + \beta_r r_j + \beta_x X_{ij})$$

where $\Phi(\ )$ is the cumulative distribution function of the unit-normal distribution, r_j is the real interest rate in 1979 in state j, and the other variables (X) are as defined earlier. The excluded instrumental variables for

Table 6

College and Bank Deregulation by Ability and Parents' Income: Probit

	(1)	(2)	(3)	(4)	(5)
Sample:	**AFQT >33 and Family Income <25%**	**AFQT >33 and Family Income >25%**	**AFQT >33 and Family Income >50%**	**AFQT >33 and Family Income >75%**	**AFQT >33 and Family Income >90%**
Years since Deregulation in 1979	0.036 (0.048)	0.043** (0.019)	0.055*** (0.014)	0.048*** (0.006)	0.018 (0.014)
Years since Deregulation in 1979 Squared	−.002 (0.003)	−0.002** (0.001)	−0.003*** (0.001)	−0.002*** (0.000)	−0.001 (0.001)
AFQT Percentile	0.011*** (0.001)	0.010*** (0.001)	0.009*** (0.001)	0.007*** (0.001)	0.005*** (0.001)
External Locus of Control Score	−0.016* (0.009)	−0.005 (0.004)	−0.006 (0.004)	−0.006 (0.006)	−0.007 (0.005)
Self-Esteem Score	0.006 (0.006)	0.009*** (0.002)	0.007** (0.003)	0.005 (0.003)	0.005* (0.003)
Mother's Education	0.015 (0.011)	0.029*** (0.005)	0.024*** (0.008)	0.023** (0.009)	0.018*** (0.006)
Father's Education	0.021*** (0.007)	0.022*** (0.004)	0.020*** (0.004)	0.022*** (0.004)	0.007** (0.003)
Family Income in 1979	−1.238 (1.131)	0.365*** (0.093)	0.410*** (0.117)	0.233 (0.153)	0.089 (0.119)
Observations	867	2,904	1,964	986	409
R-Square	0.23	0.23	0.24	0.27	0.38

Note: This table presents the results of five probit regressions, where the dependent variable equals 1 if the individual attended college and 0 otherwise, and where the reported coefficients are the estimated marginal effects, evaluated at the mean values of the independent variables. There is one observation per person. When designating the sample, *Family Income > X%* signifies the sample is restricted to individuals for which their family income levels in 1979 were above the X percentile of the sample. Regressors: *Years since Deregulation in 1979* equals the number of years since the state deregulated restrictions on intrastate branch banking and is computed for the year 1979. *AFQT Percentile* is the individual's Armed Forces Qualification Test score as a percentile of the entire National Longitudinal Study of Youth 1979 (NLSY79) sample, which is measured in 1980, and where 50 is the median of the NLSY79 representative sample. *External Locus of Control Score* (computed in 1980) measures the degree to which individuals believe that external factors such as chance, fate, and luck control their lives relative to the degree that the individual has internal control over his or her life, where values range from 4 to 16—higher values signify more external control. *Self-Esteem Score* (computed in 1980) measures the degree of approval or disapproval of one's self, where values range from 6 to 30—higher values signify greater self-approval. *Mother's* and *Father's Education* measure the number of years of education of the mother and father, respectively. *Family Income in 1979* measures the income of the individual's household in 1979, computed in 2010 dollars and divided by $100,000. The regression includes regional, racial, and gender fixed effects; the individual's year of birth; and a dummy variable that equals 1 if we imputed *Family Income in 1979* with the sample mean because of missing values. Sample: Besides the core NLSY sample, we also include the supplementary data set on blacks and Hispanics. Standard errors are clustered at the state level and corrected using the Huber-White adjustment. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

r_j are $D_{j,79}$ and $D^2_{j,79}$. Recall from Table 3 that bank deregulation helps account for cross-state, cross-time variation in interest rates. The first stage here is different from Table 3 because there is no time variation; there is one observation per person. However, the coefficient estimates from the first stage correspond with those presented in Table 3. In presenting the 2SLS probit estimates, Table 7 uses a structure similar to Table 6, that is, we split the sample by an individual's AFQT scores and family income.

For able students from middle-class and upper-middle-class families, we find that a reduction in interest rates from bank deregulation during an individual's formative years is associated with an increase in the probability of attending college. For the full sample of individuals, there is no significant relationship between attending college and interest rates (Table 7, column [1]). There is no significant relationship between interest rates and attending college when we only examine able students from lower-income families (families with incomes below the 25th percentile) or when we only examine able students from high-income families (families with incomes above the 90th percentile). Only when we consider able students from middle-class or upper-middle-class families—that is, when we restrict the sample to able students from families with incomes above the median or above the 75th percentile—do we find that changes in interest rates triggered by bank deregulation are negatively and significantly associated with college attendance.

The economic magnitude of the relationship between interest rates and college attendance is large for the subsample of students with AFQT scores above the 33rd percentile who are from middle-class and upper-middle-class families. The coefficient estimate for the sample of able students from families with incomes above the median indicates that a reduction in real interest rates of 1 percentage point is associated with an increase of almost 40 percent in the probability of attending college. For a not-inconsequential segment of society, credit conditions are importantly linked with college attendance.

As noted earlier, we must interpret the 2SLS estimates cautiously because the instruments are unlikely to satisfy the exclusion restriction. Although there is no evidence that educational choices influence the timing of intrastate branch deregulation, there is evidence that intrastate branch deregulation influenced many features of

Table 7

College and Interest Rates by Ability and Parents' Income: Instrumental Variable Probit

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	**All**	**AFQT >33 and Family Income <25%**	**AFQT >33 and Family Income >25%**	**AFQT >33 and Family Income >50%**	**AFQT >33 and Family Income >75%**	**AFQT >33 and Family Income >90%**
Interest Rate	−0.184	−0.049	−0.258	−0.385**	−0.465***	−0.151
	(0.153)	(0.384)	(0.171)	(0.150)	(0.079)	(0.275)
AFQT Percentile	0.027***	0.030***	0.028***	0.030***	0.030***	0.038***
	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.005)
External Locus of Control Score	−0.007	−0.043*	−0.015	−0.021	−0.029	−0.062
	(0.007)	(0.025)	(0.010)	(0.013)	(0.025)	(0.048)
Self-Esteem Score	0.032***	0.016	0.026***	0.024**	0.025	0.047*
	(0.005)	(0.017)	(0.007)	(0.012)	(0.016)	(0.026)
Mother's Education	0.050***	0.043	0.085***	0.078***	0.097**	0.146**
	(0.014)	(0.029)	(0.015)	(0.024)	(0.040)	(0.064)
Father's Education	0.054***	0.055***	0.065***	0.068***	0.100***	0.064**
	(0.007)	(0.020)	(0.011)	(0.014)	(0.016)	(0.029)

(continued)

Table 7
(continued)

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	**All**	**AFQT >33 and Family Income <25%**	**AFQT >33 and Family Income >25%**	**AFQT >33 and Family Income >50%**	**AFQT >33 and Family Income >75%**	**AFQT >33 and Family Income >90%**
Family Income in 1979	0.674***	−3.341	1.036***	1.309***	0.998	0.609
	(0.180)	(3.060)	(0.269)	(0.392)	(0.668)	(0.908)
Observations	6,909	867	2,904	1,964	986	409

Note: This table presents the results of six probit regressions, where the dependent variable equals 1 if the individual attended college and 0 otherwise, and where the reported coefficients are the estimated marginal effects, evaluated at the mean values of the independent variables. There is one observation per person. When designating the sample, *Family Income* > X% signifies the sample is restricted to individuals for which their family income levels in 1979 were above the X percentile of the sample. Regressors: *Years since Deregulation in 1979* equals the number of years since the state in which the individual was living in 1979 deregulated restrictions on intrastate branch banking and is computed for the year 1979. *AFQT Percentile* is the individual's Armed Forces Qualification Test score as a percentile of the entire National Longitudinal Survey of Youth 1979 (NLSY79) sample, which is measured in 1980, and where 50 is the median of the NLSY79 representative sample. *External Locus of Control Score* (computed in 1980) measures the degree to which individuals believe that external factors such as chance, fate, and luck control their lives relative to the degree that the individual has internal control over his or her life, where values range from 4 to 16—higher values signify more external control; *Self-Esteem Score* (computed in 1980) measures the degree of approval or disapproval of one's self, where values range from 6 to 30—higher values signify greater self-approval. *Mother's* and *Father's Education* measure the number of years of education of the mother and father, respectively. *Family Income in 1979* measures the income of the individual's household in 1979, computed in 2010 dollars and divided by $100,000. The regression includes regional, racial, and gender fixed effects; the individual's year of birth; and a dummy variable that equals 1 if we imputed *Family Income in 1979* with the sample mean because of missing values. Sample: Besides the core NLSY sample, we also include the supplementary data set on blacks and Hispanics. Standard errors are clustered at the state level and corrected using the Huber-White adjustment. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

the economy. Branch deregulation accelerated economic activity (Jayaratne and Strahan 1996), reduced income inequality by disproportionately helping the poor (Beck, Levine, and Levkov 2010), and reduced discrimination against black workers (Levine, Levkov, and Rubinstein 2014). Thus, bank deregulation might influence college enrollment rates through a variety of channels beyond its effect on interest rates. It could very well be that deregulation boosted the demand for skilled workers and that it is this "demand-side" effect that drives the increase in college enrollment, not the "cost-side" effect associated with the drop in interest rates.

Although the exclusionary restriction might not hold, two observations suggest that bank deregulation is pushing up college enrollment rates by reducing the cost of college, not just by increasing the demand for skilled workers. First, Beck, Levine, and Levkov (2010) show that bank deregulation increased the demand for unskilled workers, not skilled workers. Thus, bank deregulation does not seem to have increased the demand for skilled workers, which is the starting point of the demand-side story.

Second, we now assess whether bank deregulation increased or decreased the returns to education. According to the demand-side story, bank deregulation should increase the demand for skilled labor and hence the returns to education. In contrast, the supply-side view suggests that bank deregulation lowered the costs of a college education, boosted the supply of college-educated workers, and thereby lowered the returns to education. We assess which prediction holds in the data.

In Table 8, we regress the Mincerian returns to education on bank deregulation over the period 1976–2002 using the following specification:

$$MR_{jt} = \alpha D_{jt} + \beta D_{jt}^2 + \delta_j + \delta_t + e_{jt} \tag{8}$$

The dependent variable, MR_{jt}, equals the Mincerian returns to education in state j during year t and is computed by regressing—by state and year—log wages on years of education and a quartic in potential work experience and by collecting the estimated coefficient on years of education. To compute MR_{jt}, we use the CPS March Supplements and make the computations over the years 1976–2002 for the sample of full-time, full-year, white males between the ages of 25 and 55 while we exclude people living in group quarters and working in either the military or agriculture. We use

Table 8
Bank Deregulation and the Returns to Education

	(1)	(2)	(3)	(4)
Years since Deregulation	0.0021***	0.0035***	0.0004	−0.0010**
	(0.0006)	(0.0003)	(0.0005)	(0.0004)
Years since Deregulation Squared	0.0000**	0.0000**	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Constant	0.0733***	0.0618***	0.0592***	0.0657***
	(0.0027)	(0.0016)	(0.0032)	(0.0025)
Year Effects	No	No	Yes	Yes
State Effects	No	Yes	No	Yes
Observations	1,274	1,274	1,274	1,274
R-Square	0.134	0.522	0.370	0.602

Note: This table presents the results of four regressions, where the dependent variable is *Returns to Education*. For each state-year, *Returns to Education* is computed by regressing log wages on years of education and a quartic in potential work experience and by collecting the estimated coefficient on years of education, as we use data from the CPS March Supplement, over the years 1976–2002, for the sample of full-time, full-year, white males between the ages of 25 and 55, and as we exclude people living in group quarters and working in either the military or agriculture. Consistent with previous research, the sample includes data on the District of Columbia and all states except Delaware and South Dakota, which are dropped because of large concentrations of credit card banks. Regressors: *Years since Deregulation* equals the number of years since the state deregulated restrictions on intrastate branch banking and is computed at the state-year level. Standard errors are clustered at the state level and corrected using the Huber-White adjustment. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

the CPS, rather than the NLSY79, because the CPS samples a much larger cross section of individuals, and we do not need the longitudinal nature of the NLSY79 to compute the returns to education at the state-year level.

As shown in Table 8, bank deregulation reduced the returns to education, after controlling for state and year effects. That reduction is consistent with the cost-side channel playing an important role, whereby bank deregulation lowered the costs of education, shifted out the supply curve of skilled workers, and reduced the returns to education. The findings are inconsistent with a purely demand-side

story in which bank deregulation only boosted the demand for skilled labor, shifted out the demand curve, and increased the returns to education.

The results presented in Tables 7 and 8 suggest that by lowering the costs of a college education, bank deregulation boosted college enrollment rates among able students from middle-class and upper-middle-class families. Although those results do not indicate that bank deregulation increased college enrollment *only* by lowering costs, the results do suggest that bank deregulation increased college enrollments by lowering costs. Although we focus on the effect of deregulation on interest rates, it is possible that deregulation boosted college enrollment rates by increasing family incomes and thereby reducing the costs of funding a child's college education. Although the income channel is feasible, Beck, Levine, and Levkov (2010) show that deregulation boosted the incomes of families in the lower third of the income distribution, and we find that the major effect of deregulation on college enrollment rates occurs in families between the median and 90th percentiles of the income distribution. Thus, even though we cannot nail down the interest rate channel per se, the evidence indicates that improvements in credit conditions triggered by bank deregulation increased college enrollment rates.

6. CONCLUSIONS

In this paper, we find that intrastate bank branch deregulation, which lowered interest rates, boosted college enrollments among able students from middle-class and upper-middle-class families. Our findings suggest that credit conditions, the ability of an individual to benefit from college, and a family's financial circumstances combine to shape decisions about attending college. Banking reforms that ease credit conditions boost college enrollment rates among a significant portion of the population.

The analyses suggest that the functioning of the financial system exerts a powerful influence on shaping economic opportunities, as emphasized by Levine (2005). Although many factors shape the economic opportunities available to a child, affording a good education is one of them. The results presented in this paper indicate that improvements in the functioning of the financial system help make education affordable to more students.

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Comment

Raquel Fernández

Starting with the well-known observation that family income and education are positively correlated, this paper asks an important question: to what extent does that correlation reflect the effect of credit constraints? In particular, the paper seeks to examine the link between college attendance and credit constraints. The obstacle to answering that question lies in the difficulty of reliably identifying the credit channel.

The authors' contribution is the use of the variation in the timing of intrastate bank deregulation as a way to obtain "exogenous" variation in the competitiveness of the banking system and hence in the interest rates it charges. This route is potentially promising, and the authors do a good job of dealing with various possible objections. Nonetheless, a few potentially important problems lie in interpreting their results. I discuss those below.

As a first step, the authors show that, after controlling for year and state fixed effects, the (mortgage) interest rate is a negative function of the number of years a state has been deregulated (Table 3).[1] This result is necessary in order for the analysis that follows to make sense—ceteris paribus, states had lower interest rates after deregulation. I would have liked to have seen plots of the data using time windows of varying length to have a feel for the variation and size of the effect. In general, the entire paper would have been improved by presenting graphs showing the variation in the data used to support the main results.

The authors do not justify their choice of the mortgage interest rate as the relevant rate facing individuals. What is the marginal interest rate for loans for most individuals regarding college decisions?

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[1] Although they include a quadratic specification in *Years since Deregulation*, the estimated relationship turns out to be linear.

To the extent that such loans are mainly federal student loans, the rate will not vary by state. If, on the other hand, the ability of individuals to prosper from college relies on a good primary and secondary education, then where one lives is the main determinant of education quality at this level. In that case, the mortgage interest may be the best indicator of how expensive families find it to live in communities with good schools. I would urge the authors to explore that possibility in future research.

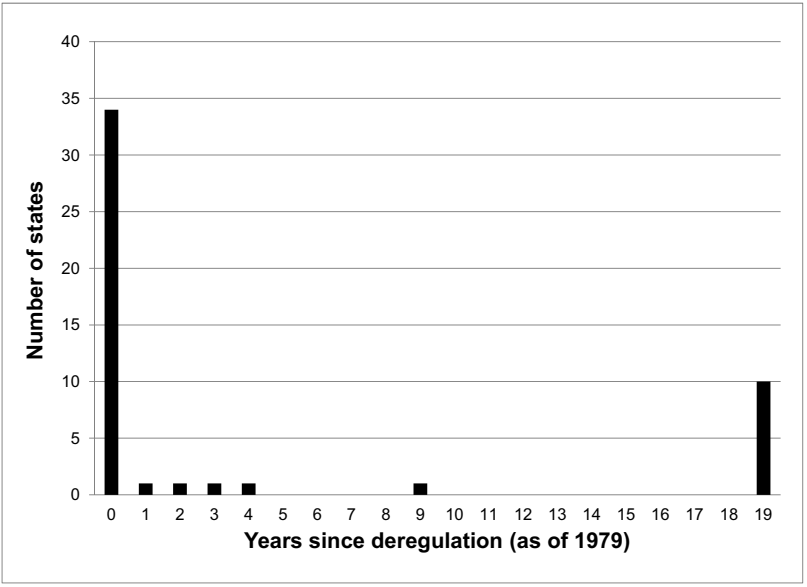
The main analysis consists of ordinary least squares and probit regressions in which the dependent variable is whether an individual i from state j attended college. The data are from the National Longitudinal Survey of Youth 1979, which follows a sample of individuals who were between the ages of 14 and 22 in 1979. The regressions control for year of birth, gender, parental income in 1979 (or parental education), Armed Forces Qualification Test score (age normalized), and some individual attitudes (self-esteem and locus of control).[2] The main variable is the number of years since deregulation for state j (henceforth $d_{j,79}$) as of 1979 if deregulation occurred; otherwise, it takes the value of 0.

Although in principle the variable $d_{j,79}$ is sensible, a quick look at the years in which deregulation occurred (provided in the paper's Table 1) raises serious doubts. A large group of states (10) deregulated in 1960 (hence they would be coded 19), and an even larger set of states (over 30) deregulated in 1979 or after (hence they would be coded 0). As shown in my Figure 1, this variable leaves very little variation across states to "explain" the variation in college outcomes. There appears to be no good reason to code the variable in this way.

A better approach would be to conduct a difference-in-difference analysis by examining the difference in college outcomes for the cohort of individuals too old (say, age 22 or older) to be affected by the state's deregulation in a given year relative to the potentially affected younger cohort in that same state and compare it with the equivalent cohort difference in college attendance in a state that did not deregulate. Unfortunately, the National Longitudinal Survey of Youth 1979 has data for individuals born in a narrow age range. Nonetheless, given that a sizable number of states deregulated between 1975 and 1985 (15), such an analysis may well be feasible.

[2] Why these attitudes should be included in the controls is unclear, and I would have preferred to have seen the regressions without them.

Figure 1
Distribution of $D_{j,79}$



Quantitatively, the results are rather suspect, raising further doubts about the main dependent variable. As indicated previously, Table 3 (column [4]) regressed the mortgage interest rate on the number of years the state had deregulated. The coefficient on this variable indicates that after 10 years the real interest rate is expected, on average, to decrease by 0.2. Given that the real interest rate has a mean of 5.2, it is difficult to believe that college decisions are so sensitive to those small changes. In fact, Table 5 (which contains the main probit results) indicates that one would expect a 21 percent increase in the probability of attending college by high-ability individuals from less educated (hence presumably lower-income) families. That is a very large response to a small change in interest rates!

One objection I raised to an earlier draft of this paper was the possibility that the demand side could be playing a large role. That is, intrastate bank deregulation has been previously shown to have been associated with more startups and increased entry and exit of

firms (Kerr and Nanda 2009); more entrepreneurs (Black and Strahan 2002); lower barriers to entry, especially in bank-dependent industries (Cetorelli and Strahan 2006); and a smaller black–white wage gap (Levine, Levkov, and Rubinstein 2011). If bank deregulation increased the demand for skilled workers, it would also affect the attractiveness of college. The authors now address that concern by showing (Table 8, column [4]) that the return to years of schooling (for full-time-employed white men) is negative once (and only once) they include year and state fixed effects. It would have been interesting to see whether those results could have been replicated using an indicator for some college and above. In any case, as the authors acknowledge, this analysis cannot eliminate the possibility that demand played an important role but that it was overridden by the supply response, resulting in lower wages for college-educated workers.

Next, I turn to some comments regarding the analytical framework for the results. It is interesting to note that the authors never define what it means to be credit-constrained. Is it an inability to borrow on the part of lower-income individuals? Is it their facing higher interest rates than others face? Note that a decrease in interest rates should, ceteris paribus, make investing in human capital more attractive for everyone. Hence, simply observing a positive response does not allow one to conclude that individuals are credit-constrained. For the purpose of their paper—especially given their finding that deregulation increases the probability of attending college for individuals in the upper two-thirds of the Armed Forces Qualification Test distribution with at least one parent with fewer than 12 years of education—I think that a model that distinguishes between the interest rate on borrowing versus the one on saving and that models college as a discrete choice from the outset may be more useful. Below I sketch a very simple model that delivers results consistent with the authors' empirical results.

Consider the college decision of an individual with parental income y and endowed with ability a. Suppose that by going to college the individual earns ay_H, and by not going to college he or she earns y_L. The cost of college is the same for everyone: $c > 0$. Next, assume that individuals can borrow at rate r_b and save at rate r_s, where $r_b > r_s$. Lastly, assume for simplicity that individuals maximize household consumption. That maximized household consumption takes place in the second period, after the individual has attended college. It is simply the sum of earnings plus savings minus debt repayment.

A first observation is that $r_b > r_s$ implies that individuals will always finance college from parental income rather than by borrowing, to the extent possible. It follows that for individuals with parental income high enough ($y \geq c$), the individual will attend college if and only if $ay_H - y_L \geq c\,(1 + r_s)$.[3] For individuals with lower incomes ($y < c$), those individuals will attend college if and only if $ay_H - y_L \geq c\,(1 + r_b) - y\,(r_b - r_s)$.

It follows then that the model predicts:

- Changes in r_b do not affect their college attendance.
- Family income does not play a role in college attendance.
- Only individuals of sufficiently high ability attend college.

Those results are in accordance with what the authors find (see their Tables 4–7). For individuals from lower-income families, on the other hand, the model predicts the following:

- Decreases in r_b will increase college attendance.
- Higher family income increases college attendance.
- A higher minimum ability is required for such students to attend college than for students from wealthy families.

Those predictions are also in line with the empirical results in Levine and Rubinstein's Tables 4–7.

Let me conclude by stating that this paper raises interesting and provocative questions about the ways in which access to financing impinges on education decisions. I hope that the authors continue to work on this important topic.

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[3] An individual with family income y and ability a who attends college consumes $(y - c)\,(1 + r_s) + ay_H$, whereas consumption from not attending college is $y\,(1 + r_s) + y_L$. The inequality follows immediately.

Comment

Erica Field

In their paper, Ross Levine and Yona Rubinstein explore an extremely important question in the literature on the economics of education: what are the major barriers to college attendance in developed countries? Despite high returns to higher education in the United States, rates of college enrollment remain low among many segments of the population and were low in absolute terms throughout much of history. Although those low rates have many possible explanations, the bulk of them can be classified into either cost constraints (affordability combined with credit constraints) or low returns (essentially poor preparation of those educated in lower-income school districts).

Here, the authors attempt to isolate the role of cost constraints by using the natural experiment provided by interstate banking deregulation, which occurred between 1960 and 1999, and was associated with a sharp reduction in interest rates on consumer lending that resulted from greater competition in the banking sector. Although this instrument has been used numerous times in the past to look at the effect of interest rates on local economic activity, Levine and Rubinstein are the first to use it to isolate the causal effect of lending rates on schooling decisions. The results are also novel insofar as they link large-scale financial reforms to changes in schooling opportunity across income groups, and they demonstrate clear heterogeneity with regard to impact of reforms across household types (low-income) and individuals (high-ability).

The fact that the natural experiment is not new in the literature in no way detracts from the importance of the contribution of looking at its effect on human capital. Sources of arguably exogenous variation

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in factors influencing schooling choices are hard to come by, and they should be taken advantage of whenever appropriate to delve deeper into the topic. However, it does imply that we already have hard evidence from existing publications that the timing of interstate banking deregulation does not work well as a means of isolating changes in the cost of schooling on account of the myriad other economic effects of deregulation that have been documented in the literature. In particular, the banking reforms have been associated with increases in rates of economic growth (Jayaratne and Strahan 1996; Huang 2008), higher rates of entrepreneurship (Black and Strahan 2002; Kerr and Nanda 2009), and increases in economic volatility (Morgan, Rime, and Strahan 2004; Demyanyk, Ostergaard, and Sørensen 2007), among other effects. As such, there is no way to be sure that the effects documented in Levine and Rubinstein's paper operate exclusively through changes in the cost of borrowing to pay for college.

The authors are well aware of that fact and do not push the instrumental variables specification particularly hard, but this feature of the identification strategy nonetheless muddies our interpretation of the associations they uncover between banking deregulation and college attendance and calls into question some of the paper's claims. If one assumes that the timing of deregulation is indeed independent of other time trends in economic activity, there is still a multitude of ways in which interest rates could indirectly give rise to changes in schooling choices beyond their potential direct price effects. Are parents' wages (immediate or anticipated future wage profiles) increasing because of greater local economic activity? Is the local economic stimulus associated with an increase in public spending on education or with a convergence in economic opportunities that generate convergence in schooling outcomes (which could show up as an increase in enrollment rates among those outside of the top decile)? Or does an improvement in the local economy motivate kids to enroll in college or parents to invest in kids' college education? Yet another category of explanation unaccounted for in the paper is taste for higher education, which varies with family background and can change with local economic conditions through changes in social norms or peer effects. This channel would boost the effect of any reform that lowered direct cost constraints, for instance.

The possibility of multiple channels of influence is also potentially relevant for our interpretation of the heterogeneity result.

For instance, might it be the case that lower interest rates made it possible for *wealthier* kids to attend private or out-of-state colleges, thereby decreasing competition for spots at lower-cost schools, which led to higher enrollment among the middle class? Although still a relaxation of the cost constraint, if that were the case, it would be incorrect to attribute changes in the behavior of middle-class students to a direct response to relaxed credit constraints.

Given all of those possibilities, two-stage least squares (2SLS) estimates are impossible to interpret, and a reduced form association between deregulation and college enrollment is the only specification that makes sense to use in the paper. It is also the case that the 2SLS estimates are implausibly large, which is hardly surprising given the many potential avenues of influence that are being attributed to interest rate changes in this specification. In this instance, the magnitudes of the estimates should offer guidance as to the plausibility of the assumptions required of the 2SLS specification, and here the numbers make it seem particularly unlikely that the documented effects operate only through the cost of borrowing for college.

The authors certainly acknowledge the fact that many possible channels of influence exist and cite the precedent papers that make the indirect mechanisms impossible to ignore. However, they also argue in favor of the role of credit constraints and college affordability by presenting evidence against alternative pathways. Unfortunately, the evidence is not comprehensive enough to rule out all competing channels, and it even increases the plausibility of certain channels. For instance, they argue that evidence from previous work shows that deregulation was associated with an increase in the demand for unskilled workers, and therefore it is unlikely that enrollment rates are increasing in response to an increase in returns to college. Although this argument makes sense, the existing evidence on demand for unskilled workers makes it even more likely that the effects found by Levine and Rubinstein reflect an increase in the wages of low-skilled parents. Alternatively, an increase in the availability of low-skilled jobs may have made it easier for students to work their way through college.

If one is to gauge the potential for direct responses to the availability of credit, direct evidence would be useful here too. Were enough students from middle-income families actually taking out loans to finance higher education in 1980 for this explanation to be plausible?

As researchers gather direct evidence that college affordability has increased, focusing on the extensive margin would be particularly useful. That is, it is hard to believe that households are very sensitive to small changes in the interest rate on borrowing. A more intuitive story is that the response was driven by changes in the availability of college loans, so it would be particularly helpful to show that availability changed significantly for the group that exhibits the largest change in behavior.

Because the responses documented in the paper are highly lagged, it is particularly difficult to rule out indirect effects of interest rates on schooling decisions that operate through changes in local economic activity. This particular interpretation would gain credence by showing that the response pattern in fact exhibits a discrete jump in schooling enrollment at the point of (or soon after) deregulation rather than assuming a gradual linear increase in college enrollment likelihoods since year of deregulation. In particular, if students enroll in college once college loans become available or affordable, one would expect a sudden change to the drop in interest rates that levels off regularly and quickly after deregulation. To show that change, the authors should use a more flexible specification that better maps the timing of interest rate adjustments to support their reduced form specification. A discontinuity following reform would be reassuring: if education jumps in response to an immediate change in interest rates, it is far more convincing that the channel of influence is credit constraints rather than broader changes to the local economic environment.

Aside from various channels through which interest rate changes may influence schooling behavior, a separate, more basic concern is the potential endogeneity of the instrumental variable. That is, is the timing of interest rate deregulation correlated with other trends in local economic or schooling activity? Given that the instrument has been used numerous times in the past, previous papers have presumably dealt extensively with precisely this issue. Nonetheless, it is important to go through the usual exercise of verifying that the timing of banking deregulation *as it varies in this particular analysis* is not correlated with other characteristics of the local economic or schooling environment. In reality, given the age of the sample (respondents were between the ages of 14 and 22 in 1979), this particular analysis makes use of only a small fraction of the total cross-state

variation in deregulation timing, so the variation used to identify schooling responses may be less clean than that used in previous work. Furthermore, only 15 of 48 states plus the District of Columbia had undergone reforms by 1979, and almost all of them did so in 1960, so a continuous variable (years since deregulation took place) is a little misleading. In practice, the variation in timing of interest rate deregulation in 1979 looks much more like a binary variable.

Given those considerations, it would be reassuring to see a direct comparison between "before" and "after" states using variables like local economic conditions and trends in migration and employment around 1979. Although the paper currently relies on previous work to justify the critical assumption that the reforms are unrelated to other economic trends, given that nature of the variation that is specific to this paper, it is important to show that there are no key differences at this particular point in time (1979) between "after" states—those that did not undergo reforms until after 1979—and "before" states—those that underwent reforms in 1960 (nine cases, excluding the District of Columbia) or during the 1970s (six cases, spread over the decade).

With regard to the econometrics, the specification employed by the authors is otherwise very straightforward, but the analysis would benefit from a handful of sensitivity checks. In particular, it would be helpful to show that results are robust to the inclusion of high school dropouts who attain their General Educational Development certificates (GEDs), whose schooling decisions (and possibly the decision to acquire a GED) should also be sensitive to cost considerations. The heterogeneity analysis on which many of the conclusions rest is also subject to the same concern that plagues all subgroup analyses: it may be that differences across income groups proxy for some other characteristic of students that has nothing to do with interest rate sensitivity. To address that concern, household fixed effects would provide a useful check on any potential family background characteristics of this nature. Finding a pattern of results of similar magnitude based on a specification that contains household fixed effects would rule out certain possibilities, such as households with high-ability kids also having more creditworthy parents. It would also be reassuring to verify that the results are robust to the inclusion of any right-hand-side variables that are potentially endogenous to schooling choices, including self-esteem and locus of control scores, which surely vary (within person) with increases in schooling attainment.

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