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Loans, Liquidity, and Schooling Decisions

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Abstract

During the 1999-2000 school year, students borrowed $36 billion through the federal loan program, double the volume in 1992-93. Despite the large size and rapid growth of the student loan market, it has been the subject of little economic analysis. Does the availability of government loans affect schooling decisions? Identifying the effect of loans is empirically challenging, because eligibility for federal loans is correlated with observed and unobserved determinants of schooling. I exploit variation in loan eligibility induced by the Higher Education Amendments of 1992. I find that loan eligibility has a positive effect on college attendance and a somewhat larger effect on the choice of college, with loans shifting students toward four-year public schools. The school choice results are consistent with a particularly sharp rise in the rate of subsidized borrowing at four-year public schools.

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I. Introduction

During the 1999-2000 school year, students borrowed $36 billion through the federal loan program, double the volume in 1992-93. By contrast, federal grant volume was flat over this period: $8 billion in 1999-2000, the same as seven years earlier.[1] The rise in borrowing is not explained by growth in the student population, which has remained relatively stable during the last decade.[2] Instead, both the average loan and the proportion of students taking loans have risen sharply. In the three years following the Higher Education Act of 1992, which liberalized access to federal loans, the average loan rose from $3,300 to $4,100 and the share of undergraduates borrowing rose from 20 to 26 percent.[3]

Despite the large size and rapid growth of the student loan market, it has been the subject of little economic analysis.[4] This silence is peculiar, since student loans play a key role in economic theory. In the human capital model, individuals borrow against their future earnings in order to fund consumption and schooling costs. But since loans against human capital cannot be collateralized, private markets will not offer students the liquidity they require to attain their optimal level of education. Government can correct this market failure by providing liquidity, thereby shifting individuals closer to their higher, optimal level of schooling.

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[1] Figures are from College Board (2000). Values are inflated by the CPI-U with academic year 1999-2000 as the base year.

[2] See Table 173 in U.S. Department of Education (2000a). Fall enrollment was 14.49 million in 1992 and 14.55 million in 1998.

[3] US Department of Education (1998).

[4] Reyes (1995) analyzes the effect of the passage and repeal of the Middle Income Student Assistance Act of 1978 on schooling decisions. She concludes that expanding eligibility for loans to middle- and upper-income students had a positive effect on attendance rates.

Do federal loans achieve this goal? That is, does the availability of government loans affect schooling decisions? The presence of a large loan market does not, *per se*, shed light on this question. Infra-marginal students may take out loans because they prefer to have available other lines of credit; that is, student loans may have an option value. Or students may wish to maintain a fixed level of available credit, and so borrow more when limits are raised. Gross and Souleles (2001) provide evidence in support of this buffer-stock model by showing that credit card holders borrow more when credit limits are raised, even when they are not at those limits. Last, students may take up federal loans because they are cheaper than other sources of credit. This is a subsidy to schooling costs which should, in theory, increase schooling. In practice, the effect may be on typically unobserved margins, such as hours worked or choice of major, rather than on college entry or school quality.

For all these reasons, it is an empirical question whether loan eligibility affects schooling decisions such as college entry or college choice. Further, determining whether loans affect schooling decisions is of policy relevance. It is not costless for the government to make loans. The government pays the interest on some loans while the student is in college; interest rates may be subsidized; and there are transaction costs. This paper does not focus on calculating these costs, and so cannot provide a cost-benefit accounting of the federal loan program. But by providing insight into whether loans affect schooling decisions, the results move us a step closer to determining whether loans are an efficient use of scarce government resources.

Identifying the effect of loans is empirically challenging, because eligibility for federal loans is correlated with observed and unobserved determinants of schooling. I exploit variation in loan eligibility induced by a shift in loan policy in order to identify the effect of loans. The Higher Education Amendments of 1992 (HEA92) removed home equity from the assets "taxed"

by the federal aid formula. Previous to HEA92, each dollar of home equity reduced by three to six cents the federal aid eligibility of families on the margin of receiving more aid. For most families of college-age children, home equity is the majority of wealth. As a result, this change in the aid rules made many youth suddenly eligible for need-based aid. Since federal grants were level-funded during this period, the bulk of this newly-offered aid took the form of loans.

I find that loan eligibility has a positive effect on college attendance and an even larger effect on the choice of college, with loans shifting students toward four-year public schools. The school choice results are consistent with a particularly sharp rise in the rate of subsidized borrowing at four-year public schools after HEA92. The attendance results suggest that removal of $10,000 in home equity from taxable assets increases college attendance by 0.95 to 1.4 percentage points. This translates into an increase of 1.6 to 2.3 percentage points per $1,000 of loan eligibility.

We cannot interpret the effect of loan eligibility as a pure liquidity effect, since subsidized federal loans decrease the cost of schooling and, by implication, increase the optimal level of schooling. Plausible assumptions about the subsidy value of these loans show that the results are comparable to estimates of the effect of grant aid on college attendance. That is, a dollar delivered as a loan subsidy and a dollar delivered as a grant have roughly the same effect on the probability of college attendance.

II. Overview

The first federal student loan was the National Defense Student Loan (NDSL) of 1958, which aimed to improve science and foreign language education in the aftermath of the Sputnik launch. The NDSL, now known as the Perkins Loan, had a fixed interest rate of three percent and was forgiven if students went into certain professions. This program has never been very large,

with volume of $600 million in 1963-64 and $1.1 billion in 1999-2000. Its current, fixed interest rate is five percent.[5]

The much larger Stafford loan program has its origins in the Guaranteed Student Loan (GSL), introduced in 1965 by opponents of a proposed tuition tax credit. The new loans were aimed at middle-income students whose families faced short-term cash-flow problems in paying college costs. The loan program grew steadily, guaranteeing $12.6 billion in 1977-78. But loan volume surged to $20 billion just three years later as a result of the passage of the Middle Income Student Assistance Act (MISAA) of 1978, which opened GSLs to students of all income levels. GSLs, with their fixed interest rates, were particularly attractive given the high market interest rates of the era. The MISAA was repealed in 1981 and loan volume dropped immediately, though it never returned to its original level.

The next major shift in loan policy took place in 1992, with the Higher Education Amendments of 1992. These amendments will be discussed in detail later in the paper, as they provide the variation in loan eligibility I will use to identify the effect of loans on schooling decisions. In short, HEA92, like MISAA, opened the student loan program to middle- and upper-income students. Borrowing ticked up sharply immediately upon the implementation of HEA92, with loan volume rising by nearly forty percent in just one year.

How Student Loans Work

Federal loans fall into two categories: subsidized and unsubsidized. Subsidized loans are distributed according to need, as defined by the federal aid formulas. Need determination is discussed in the next section. A subsidized loan is one for which the government pays the

[5] This section draws on the history of student loans in Fossey and Bateman (1998).

interest while a student is in college; as a result, the student graduates owing only the principal. The magnitude of this in-school subsidy depends on the interest rate and the length of time spent in college. Given four years in college and a 5 percent real rate of interest, the in-school subsidy on the freshman year loan is about 20 percent of its face value. If four more years are spent in graduate school, the government ends up paying interest of about one third of the face value of a freshman-year loan.

Unsubsidized loans do not have this feature. Interest on some unsubsidized loans can be deferred until graduation, when it is capitalized into the principal.[6] These "unsubsidized" loans frequently have the same interest rate as subsidized loans. The interest rate on both the unsubsidized and subsidized Stafford Loans is tied to the 91-day Treasury bill, with a nominal cap of 8.25 percent.

III. Empirical Approach

We would like to know how loan eligibility influences schooling decisions. Loans may affect the likelihood of going to college, the type of college attended, or years of schooling completed. In an ideal experiment, we would gather a group of high school seniors, randomly offer loans, and over time observe their schooling decisions. In the absence of such an experiment we might use survey data to estimate the relationship between loan eligibility and schooling decisions:

$$(1)\quad y_i = \beta_0 + \beta_1 loan\_eligibility_i + \varepsilon_i$$

In this equation, $loan\_eligibility_i$ is the dollar amount of loans for which person i is eligible.

[6] Payment of both principal and interest begins immediately on parental loans, called PLUS loans.

To fix ideas, consider the case in which y_i is a dummy indicating whether a person has attended college. We would like to interpret β_1 as the causal effect of a dollar of loan eligibility on college attendance. But we can't reasonably make this interpretation, since loan eligibility is certainly correlated with the excluded determinants of y_i in Equation (1). For example, low-income students are eligible for larger loans, *ceteris paribus*. But low-income youth are relatively unlikely to go to college for reasons that are vigorously debated: their parents did not go to college and so can provide little information about its costs and benefits; they attend schools of poor quality throughout their lives and are unprepared for college; or they can't afford college even if its cost is subsidized.[7] A result of income's correlation with both loan eligibility and attendance is that excluding income from Equation (1) will bias downward our estimate of β_1.

One way to deal with this identification problem is to add to Equation (1) a set of covariates correlated with college attendance and loan eligibility:

$$(2)\quad y_i = \beta_0 + \beta_1 loan\_eligibility_i + \beta_2 X_i + \varepsilon_i$$

We can now give β_1 a causal interpretation if we have correctly modeled the direct effect of X_i on college attendance. For example, including only a linear income term in Equation (2) will falsely attribute to loan eligibility any non-linear effect of income on attendance.

Yet this approach holds promise, as there are sharp non-linearities in the aid formula that allow us to control for smooth functions of X_i and still identify the effect of loan eligibility. This is a type of regression-discontinuity analysis, first discussed in Campbell and Stanley

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[7] For a discussion of the causes of low college attendance among low-income youth, see Cameron and Heckman (1999) and Kane (1999).

(1963).[8] A strength of this approach is that it allows for identification of β_1 in a single cross-section. But, if panel data or multiple cross-sections are available, an additional source of identifying variation is changes over time in the aid formula. It is such variation in aid, arising from shifts in aid policy, that I exploit in this paper. In the next section, I describe the policy change that allows us to identify the effect of loan eligibility on schooling outcomes.

The Higher Education Amendments of 1992

Every six years, the Higher Education Act of 1965 expires and must be re-authorized. This act, as currently amended, is the authorizing legislation for the Pell Grant and student loan programs. During re-authorization, Congress generally alters the rules that determine eligibility for federal student aid. The Higher Education Amendments of 1992 made changes to both loan and grant policy. Its greater impact, by far, was on loan eligibility.

Figure 1 plots loan volume, in real terms, during the 1990s. Federal loan volume was $19 billion during academic year 1992-93, the last before HEA92 took effect. Real loan volume rose dramatically after HEA92, jumping 37 percent in just one year and nearly doubling to $36 billion by 1999-2000.[9] By contrast, federal grant volume was flat over the same period.[10] About half of the growth in loan volume was due to increased borrowing of subsidized loans, which are the subject of the analysis.[11]

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[8] van der Klaauw (1997) uses regression discontinuity analysis to estimate the effect of a private college's scholarship offers on students' decisions to enroll in that college. Other applications to schooling include Angrist and Lavy (1999), Jacob and Lefgren (2001), and Guryan (2001).

[9] See Table 2 in College Board (2000).

[10] In part, this is because grants are limited by appropriations while loans are, essentially, an entitlement.

[11] Unsubsidized borrowing accounts for the other half of the increase in loan volume after HEA92. Since unsubsidized loan volume was at a very low level before HEA92, its relative rate of increase is much

To make clear the eligibility changes brought about by HEA92, I provide here a brief description of how federal aid is determined.[12] Months before they enter college, students complete a financial aid application, indicating the colleges they are considering and providing financial information. Their data is run through an involved algorithm that calculates the amount their families are expected to contribute toward college costs. The algorithm sums income from a variety of sources and subtracts off allowable expenses (taxes, a maintenance allowance based on family size, elementary school tuition and unusually high medical costs). To this figure is added twelve percent of assets above an "asset protection allowance." For 1991-92, the asset protection allowance ranged from zero to $62,400 and rose with the age of the older parent; at age 45, the allowance was $34,200.[13]

The resulting weighted sum of assets, expenses and income is called "adjusted available income." A progressive tax schedule, with rates ranging from 22 percent to 47 percent, is applied to this figure to determine the family's expected contribution. If the family's expected contribution is less than the student's expected schooling costs, the student is offered a package of grants and loans, with the neediest (as deemed by the aid rules) offered grants and the less

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higher. Future versions of this paper will attempt to identify the effect of increased eligibility for unsubsidized loans on schooling decisions. By comparing the effect of unsubsidized and subsidized loans, we can back out the value to students of the in-school subsidy and potentially isolate the liquidity effect of federal loans.

[12] This description of aid determination is for the 1991-92 school year and applies to dependent students. In the empirical analysis, I will be focusing on 18-19-year-olds, who are overwhelmingly defined as dependents, in order to avoid the analytical problems caused by students gaming their dependency status.

[13] In determining Pell Grant eligibility, the rule was slightly different: the flat allowance for non-housing assets was $25,000 and an additional $30,000 was subtracted from home equity.

needy offered subsidized loans. If the family's expected contribution is more than schooling costs, the student is ineligible for grants or subsidized loans but can obtain unsubsidized loans.[14]

Until HEA92, home equity was one of the assets taxed in this process. As a result, up to 5.64 percent of home equity was considered available for paying a given year's schooling costs. Since the tax is applied each year that a child goes to college, the cumulative tax for a child in college for four years is up to 20.72 percent [$=1-(1-0.0564)^4$]. Should two children attend four years of college consecutively, the tax is as high as 37.15 percent.[15]

A key provision of HEA92 was the removal of home equity from the set of taxable assets. Since home equity is a large proportion of household net worth, this had a major impact on families' paper wealth. Among homeowner households with college-age children in the 1990 Panel of the Survey of Income and Program Participation (SIPP), median home equity is $45,000 (nominal), representing 64 percent of net worth.[16] For such a family, the equity rule change would have reduced the expected family contribution by up to $2,400 for each year that a child was in college.[17] The rule change had its greatest impact on those with the highest home equity: for families with equity at the 75th percentile, the expected family contribution would have

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[14] Pre-HEA92, unsubsidized loans for dependent students were limited to $4000 a year in parental PLUS loans. After HEA92, unsubsidized Stafford loans were made available to dependents. For freshmen, the sum of unsubsidized and unsubsidized Stafford loans is limited to $2625.

[15] See Feldstein (1995) and Dick and Edlin (1997) for further discussion of this tax and its effect on parental savings.

[16] Author's calculations. These figures are based on the 97% of homeowner households that have positive net worth. The median is of the distribution of home equity divided by net worth.

[17] The calculation assumes net worth of $76,700, home equity of $45,000 and an asset protection allowance of $34,200. I assume ineligibility for a Pell Grant, since almost no student with family income at the median is eligible.

dropped by up to $4,800. It is this heterogeneous effect of the rule change on financial aid eligibility that I will exploit in estimating the effect of loans on schooling decisions.[18]

Using Equity to Identify the Effect of Loans

By removing home equity from the financial aid calculation, the government swept many people into the pool eligible for student loans.[19] Since the expected family contribution dropped most for those with the highest home equity, this suggests comparing the college attendance of youth from high and low equity homes, before and after HEA92, and interpreting any increase in the attendance of high-equity youth to HEA92. Alternatively, rather than arbitrarily divide the population into high- and low-equity families, we can instead make the analysis continuous by estimating a time-varying effect of a given dollar of equity on college attendance. The effect of the equity rule change is then identified by any shift in the equity-attendance gradient that occurs after HEA92. In other words, we can estimate the smooth relationship between equity and attendance and identify the effect of the equity rule change from a structural break in that relationship after HEA92.

We could estimate this shift with the following equation:

$$(3)\quad y_{it} = \alpha_0 + \alpha_1 after_t + \alpha_2 equity_{it} + \alpha_3 after_t \times equity_{it} + \alpha_4 X_{it} + \mu_{it}$$

[18] Note that the effect of the rule change on loan eligibility is not linear. Some families are so well off that incremental changes in home equity will have no impact on their aid eligibility, and the same is true of families that are quite poor. I address this in the next section.

[19] Some would have become newly eligible for grants because of the rule change. But as was shown with the aggregate spending figures, the increase in loan eligibility dominates. This is because, first, appropriations for grants did not change and, second, the type of student whose aid eligibility was affected by the inclusion of equity was unlikely to receive a Pell Grant. Just 3.7 percent of dependent students from families with income of $40,000 to $60,000 received a Pell in 1995-96.

Here, $after_t$ is a dummy variable indicating the years in which HEA92 is in effect and $equity_{it}$ is a measure of the home equity held by the family of person i. The shift in the relationship between equity and attendance is captured by α_3.

The specification of Equation (3) assumes no pre-existing trends in the relationship between equity and attendance. I will use two approaches to avoid confounding the effect of HEA92 with such trends. First, I will allow the effect of equity to vary by year by replacing $after_t \times equity_{it}$ in Equation (3) with a set of interactions of equity with year dummies. This will let us observe how the effect of equity on attendance has evolved over time. Second, I will take a more parametric strategy by estimating the following equation:

$$(4)\quad y_{jt} = \alpha_0 + \alpha_1 after_t + \alpha_2 equity_{it} + \alpha_3 time_t \times equity_{it} + \alpha_4 time_t \times equity_{it} \times after_t + \alpha_5 equity_{it} \times after_t + \alpha_6 X_{it} + \mu_{it}$$

This specification allows for separate trends in the effect of equity on attendance before and after HEA92. The effect of HEA92 is identified by any discontinuous jump in the magnitude of the effect of equity on attendance, captured by α_5 in Equation (4). In the absence of trends in the effect of equity on attendance, Equation (4) collapses to the simpler specification of Equation (3).

Note that these specifications do *not* identify the effect of HEA92 from the correlation between equity and attendance. Instead, the effect is identified by the *change* in that correlation. A benefit of this approach is that it does not require that I catalog the many plausible mechanisms through which home equity might influence college attendance and attempt to

control for them parametrically.[20] Instead, I allow for a smooth relationship between equity and attendance and identify the effect of HEA92 with deviations from that smooth relationship. It is the magnitude of that break that will isolate the effect of removing home equity from assets taxable by the financial aid formula.

One problem with the approach discussed here is that, for families of college-bound children, home equity may be endogenously determined by the financial aid rules. Indeed, there is a small literature that demonstrates that the financial aid rules affect both the level and composition of assets held by families.[21] In the case at hand, HEA92 may have induced families of college-bound youth to shift their savings into home equity in order to protect them from taxation. This would produce a spurious shift in the relationship between equity and college attendance after HEA92. In order to avoid this problem, and because of data constraints described in the next section, I will in most specifications proxy or instrument for home equity with home value or local real estate values.

IV. Data

I use two household surveys, the October School Enrollment Supplement of the Current Population Survey (CPS) and the Survey of Income and Program Participation (SIPP), to estimate the equations described above. I draw on the strengths of the two datasets, using the CPS to establish a structural break in the relationship between attendance and home values after

[20] For example, when real estate values rise, incumbent owners have more equity to borrow against for college costs. Home values may also rise in a booming labor market and draw youth away from college.

[21] Feldstein (1995) and Dick and Edlin (1997) calculate the magnitude of the asset tax and estimate its impact on asset accumulation. Kim (1997) shows that families move their savings into protected assets (e.g., retirement vehicles) in order to avoid having them taxed by the financial aid formula.

HEA92, and the SIPP to confirm that the effect is strongest among those most likely affected by the change in aid policy.

The strength of the CPS is its size and frequency. Because it is an annual survey, we can use it to distinguish sharp breaks from continuous trends, and the large sample allows us to do so with quite small correlations. The drawback of the CPS is that it does not allow us to link all college-age children with their parents; only 74 percent of 18- and 19-year-olds have parental information available. Rather than risk biasing the sample by throwing out the quarter of youth who do not have parental information, I restrict the CPS analysis to specifications that do not require parental data.[22]

A consequence is that in the CPS we cannot narrow in on those youth whose financial situation places them on the margin of receiving more aid. This is a serious drawback, since the effect of equity on aid eligibility is non-linear. Home equity has no impact on the loan eligibility of the poorest and richest families. They are either so poor that they are already eligible for the maximum allotment, or so rich that they are ineligible even with the exclusion of home equity from their assets. By necessity, in the CPS I can identify only the average effect of the equity rule change, calculated over those who are and not on the margin of getting more aid.

The SIPP does not have this flaw, as it is a longitudinal survey. We can identify family groups when children are of high school age and observe later whether those children go to college. The sample used in the paper successfully matches 92 percent of college-age children to

[22] A college-age youth can be linked to her parent's CPS record if she lives with her family or she is away at college, but not if she is living on her own and not attending college. This will produce bias in analyses where college attendance is an outcome of interest and the sample is limited to those with parental information. Cameron and Heckman (1999) discuss this point.

their parents.[23] The weaknesses of the SIPP are the mirror-image of the strengths of the CPS: size and frequency. In the SIPP, we cannot precisely distinguish sharp breaks from continuous trends, since only a year of the necessary data is available on each side of the HEA92 policy change.[24] And the relatively small sample severely limits the degree to which we can narrow in on affected youth with any precision. The SIPP will therefore primarily serve to confirm the CPS findings.

While the SIPP contains information about home equity and value, the CPS does not. In the CPS I use state-year median home values to proxy for individual home values. I calculate these home values as follows. The Federal Home Loan Mortgage Corporation (popularly known as Freddie Mac) maintains a quarterly, state-level index that tracks repeat sales of single-family homes.[25] The index is unitless; for the analysis it has been normed to one in the first quarter of 1990. To anchor the index in dollar terms, I calculate median home values in each state using the 1990 census and multiply these values by the index. The home values are then converted to 2000 dollars using the CPI price index for shelter. This gives me a state-by-year panel of home values.[26]

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[23]However young the sample, some will not have parental information. The figure is five percent for 15-year-olds, as compared to eight percent for the sample used in the analysis. Once we start to move into older cohorts, the proportion rises rapidly: it is twelve percent for those born just one year earlier than the sample cohorts.

[24] The Assets and Liabilities topical module is the limiting factor. It was answered by the 1990 and 1992 panels of the SIPP, which gives us data for children first entering college during the 1991-92 and 1993-94 academic years.

[25] By focusing on this type of sale, the index excludes price variation that is driven by changes over time in housing quality and size.

[26] The index is available by metropolitan area. Changing borders of MSAs over time make it difficult to construct a consistent panel.

Much of the variation in home values is due to fixed differences across states: California is always more expensive than Arkansas. State fixed effects will be included in the regressions to absorb these fixed differences. But there is considerable within-state variation in home values. Figure 2A graphs median state home values in 1984, sorted from lowest to highest. Median home values varied from $78,233 in Mississippi to $383,201 in Hawaii. Figure 2B shows home values in 1999, with the states ordered by their 1984 ranking. As is clear from Figure 2B, there is considerable movement in relative home values during this period.

The CPS sample spans 1984 to 1999 and consists of all 18-19-year-olds. The SIPP sample is drawn from the 1990 and 1992 panels and consists of those born in 1973 and 1975, respectively. These birth cohorts would have been on schedule to enter college for the first time in the falls of 1991 and 1993, respectively. The SIPP sample is limited to the 92 percent of the sample that has parental information available in the January before this potential fall enrollment. I measure family income, home value and home equity in this January survey.

The outcomes of interest are college attendance and the type of college attended. In the CPS, I observe whether a person is enrolled in October and the type of college he attends. In the SIPP, a retrospective question asked in late spring and early summer[27] elicits whether a youth was enrolled in college anytime during the just-ending academic year. The SIPP does not contain information on the type of college attended.

Means of the CPS and SIPP data are in Tables 1 and 2. Sample weights are used throughout the analysis. Standard errors are adjusted for heteroskedasticity due to the binary

[27] Each SIPP panel is divided into four sub-samples. Only one sub-sample is interviewed in a given month, so the entire panel answers a given set of questions over the course of four months.

dependent variable. When state-year median home value is included in the regressions, standard errors are adjusted for correlation within state-year cells.

V. Results

To get a sense of home values and college attendance are related, I start by examining their bivariate relationship in the CPS (see Table 2). There is a small, positive correlation between home values and college attendance: for each $10,000 rise in home value, the college attendance rate rises 0.20 percentage points (with a standard error of 0.05 percentage points). This correlation may be driven by differences across states in income and demographics. In Column (2) I add to the regression dummies for an individual's age, race and ethnicity. I also include year dummies and the state-year mean and median of household income of 16-17-year-olds.[28] The relationship between home values and attendance is substantively unchanged at 0.27 percentage points with a standard error of 0.04 percentage points. Rising home values in a state may reflect a tight labor market, which will increase the opportunity cost of college and divert high school graduates from college. I therefore add to the regression the state's September unemployment rate, as well as the change in the unemployment rate since the previous year.[29] The coefficient is unaffected.

There are likely fixed differences across states that are correlated with both attendance and home values not captured by these regressions. Adding state fixed effect to the regression

[28] Within this younger group, we are more reliably capturing parental income with the household income variable. The income variable is categorical and top-coded. I assign the midpoint of each category except for top-coded households, which are assigned the highest coded value for that year. Adding to the regressions a variable measuring the share of the state's households that are top-coded in a given year does not affect the results.

[29] September unemployment plausibly captures the conditions facing a youth deciding whether to enter college in the fall. Using annual unemployment does not change the results.

has a substantial impact: as shown in Column (4), the coefficient doubles in magnitude and flips sign. Each $10,000 rise in home value is now associated with a *decrease* in college attendance of 0.56 percentage points, with a standard error of 0.11 percentage points. One possible channel for this negative correlation is financial aid: families with higher home values are eligible for smaller financial aid packages. Other explanations are plausible and I do not argue that financial aid explains all or even most of this negative correlation.

Recall that we are looking for a break in the relationship between home values and college attendance rates as of the academic year that begins in 1993. In order to examine the evolution of the effect of home values on college attendance, I take the most fully controlled specification of Column (4) and replace home value with its interactions with a set of year dummies. I also include in the regression the interactions of all of the covariates with the after dummy. This will capture any change in the effect of covariates over time. Last, since much of the variation in home values is across regions, I also include a set of nine region dummies interacted with the after dummy to absorb any regional trends in attendance rates.

The coefficients on these interactions are shown in Table 4 and plotted in Figure 3. In the figure, the 1992 interaction is normed to zero. The effect of $10,000 in home value on attendance is becoming less negative throughout this period, rising from –1.02 percentage points in 1984 to –0.73 percentage points in 1992. In 1993, however, there is a sharp, discontinuous break in this series, with a one-year rise of 0.32 percentage points. This is the largest jump in the series. This sharp structural break in the relationship between equity and attendance in the first year that HEA92 took effect is strong support for the empirical strategy of the paper.

In order to parameterize the magnitude of the 1993 jump, I fit a new model to the data. I estimate Equation (4), which allows for separate time trends in the effect of home values before

and after HEA92, as well as an intercept shift. Results are in the first column of Table 5. Before HEA92, the effect of home values on attendance grows more positive at the rate of 0.04 percentage points per year (with a standard error of 0.02). In 1993, there is a sharp rise of 1.04 percentage points in the effect of $10,000 of home value on the attendance rate. Based on the pre-existing time trend, the increase in the effect in 1993 was 25 times larger than expected. Again, this is strong evidence of a break in the series.

We might be concerned that changes in CPS or Freddie Mac survey methodology might be driving the observed discontinuity. However, I can find no change in the methodology of either survey that would explain this jump. Further, as can be seen in Figures 4A and 4B, there is no similar jump in the time series of home values or college attendance rates. The break is only in the relationship between these two series, and not in the series themselves.

These results indicate that the removal of equity from the assets taxable by financial aid had a positive effect on attendance rates. It is possible that the policy change also had an impact on other aspects of schooling decisions, such as the type of school attended. In order to address this question, I take the specification of Column (1) in Table 5 and run three regressions in which the dependent variables are dummies for attendance at three types of colleges: two-year public, four-year private and two-year public.[30]

As is clear from Table 5, the effect is concentrated in four-year public colleges. After HEA92, there is a sharp, significant rise in the effect of $10,000 in equity on attendance at these schools of 1.53 percentage points (with a standard error of 0.45 percentage points). By

[30] Only one percent of this age group attends a two-year private school, so I do not include these results. For this type of college, the coefficient of interest is small, negative and statistically insignificant.

comparison, the effects for attendance at four-year private and two-year public colleges are small, negative and insignificant.[31] A plausible explanation for this set of coefficients is that increased access to loans induced some youth who would not have attended college at all into two-year colleges, but induced even more who would have otherwise attended two-year college into four-year public schools. That is, the school-choice effect of loans appears to be larger, in absolute terms, than the attendance effect.

The CPS results are strong evidence of a structural break in the relationship between home values and schooling decisions after HEA92. Figure 3 and Table 4 make clear that there was a positive, discontinuous change in the effect of home values in the first year after HEA92 was effective. With this evidence in hand, I turn to the SIPP in order to confirm that the effect is concentrated among those youth whose eligibility for loans was most likely increased by HEA92.

SIPP: Focusing on the Affected Population

Table 6 replicates the attendance analysis of the previous section using the SIPP; there are no data on type of college attended in the SIPP. There are just two years of data in this sample, so no analysis of pre-existing trends is possible. The covariates used in the previous section are used, except income and home value data are now measured at the household level. In Column (1), I estimate the change in the effect of home equity on attendance after HEA92. The sample is limited to homeowners, since own home value and equity are now the explanatory variables of interest. This ability to focus on homeowners is an advantage of the SIPP, since, in

[31] We may see the zero effect for private schools because they respond to more generous federal aid by cutting their own scholarships to students.

the CPS, we could not identify which college-age youth were from homeowner families. The estimate is positive but insignificant: 0.26 percentage points, with a standard error of 0.42 percentage points.

In response to HEA92, homeowners with college-bound children may have shifted assets into their now-protected home equity. Further, home equity may be measured with error. The use of state values in the CPS skirted both of these problems. In order to deal with these sources of bias in the SIPP, I use an instrumental variables strategy. As instruments for home equity and its interaction with the after dummy, I use home value and its interaction with the after dummy.

Results are in Column (3). The coefficient rises substantially from 0.26 percentage points to 0.95 percentage points, with a standard error of 0.55 percentage points. This is similar in magnitude to the CPS estimate of 1.04 in Table 5, though that was an average obtained across owners and renters. To calculate a comparable estimate from the CPS, assume the effect is zero for renters and note that that 72 percent of this age group is from a home-owner family (see SIPP means in Table 2). The implied effect in the CPS for owners is then 1.45 percentage points per $10,000 of equity (=1.04/0.72), which is within a standard error of the SIPP instrumental variables estimate of 0.95.

Finally, I look at whether the effect varies by income group. I split the SIPP sample by median household income and run the instrumental variables specification. The effect is concentrated in the upper-income families, with a coefficient of 1.34 percentage points that is significant at the ten percent level. The effect for low-income owners is 0.41 percentage points and insignificant. These results suggests that the removal of equity from taxable assets widened the gap in attendance between low- and high-income youth. The implied distributional effect is

actually stronger than these coefficients would indicate, since renters tend to have lower incomes than owners and did not benefit from the removal of equity from taxable assets.

VI. Discussion

The results suggest that removing $10,000 in home equity from assets taxable by the aid formula increases college attendance by 0.95 to 1.4 percentage points. This translates into 1.6 to 2.3 percentage points per $1,000 of loan eligibility, or 4.2 to 6.1 percentage points for the maximum freshman-year loan of $2,625.

If we assume for a moment that the marginal borrower is not liquidity constrained, and instead takes out loans because of their subsidy value, we can calculate an elasticity of attendance with respect to the loan subsidy. One cannot know the true value of this subsidy until the loan is paid off, as it depends on the spread between market and subsidized interest rates over the life of the loan.[32] Rough estimates of the subsidy value of Stafford Loans range up to 60 percent of a loan's face value.[33] A lower bound on a loan's subsidy value is the interest paid by the government while the student is in school, which represents a fifth to a third of the face value. Using a compromise of 0.4, this suggests that the attendance response to the loan subsidy is 4.0 to 5.7 percentage points (=1/0.4 multiplied by 1.6 to 2.3 percentage points) per $1,000 of eligibility. This falls into the high range of estimates of the effect of grant aid or tuition prices on college attendance, which are generally three to five percentage points per $1,000.[34] That is, a

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[32] Further, it is not clear what the "market rate" is, since there is no private market for this type of loan. Private lenders do provide student loans, but they are not comparable to federal loans, as they require either a good credit history (which almost no young college entrant will have) or a credit-worthy co-signer. They are therefore less risky to the lender.

[33] This estimate is from Feldstein (1995) and is based on prevailing rates in 1985.

[34] See Dynarski (1999, 2000), Kane (1994) and the literature review in Leslie and Brinkman (1988).

dollar delivered as a loan subsidy and a dollar delivered as a grant have roughly the same effect on the probability of college attendance. Of course, if the marginal borrower is liquidity constrained, then part of the response is attributable to loosening those constraints and the implied elasticity with respect to the loan subsidy is lower.

The school choice results are consistent with data that show that subsidized borrowing at four-year public schools rose by nearly half after HEA92, from 21 percent of dependent students in 1992-93 to 29 percent in 1995-96.[35] Part of the increase in four-year public borrowing is among students who would have gone to these schools anyway, who are borrowing more either because public university tuition rose sharply during this period or because they have shifted their borrowing from other sources toward student loans. But the results of Table 5 suggest that a portion of the growth in borrowing at these schools is from new entrants.

VII. Conclusion

The results suggest that eligibility for federal student loans has a positive effect on college attendance, and a somewhat larger impact on school choice. Access to federal loans shifts students from private colleges and two-year public colleges into four-year public colleges. The movement away from private colleges might be explained by these schools reducing their own aid as federal aid becomes more generous. The school choice results are consistent with a particularly sharp rise in the rate of subsidized borrowing at four-year public schools. Rough calculations indicate that a dollar delivered as a loan subsidy and a dollar delivered as a grant have about the same effect on the probability of college attendance.

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[35] Borrowing also rose among private four-year college students, but at a slower pace, from 38 percent to 44 percent. Subsidized borrowing actually dropped at two-year public colleges. See Tables 2.2 , 3.2 and 4.2 in NCES (2000b). Two-year private college students are not examined in this study.

It should be emphasized that the paper's estimates are for the marginal eligible youth. Those who gained eligibility due to the removal of equity from assets taxable by the financial aid formula are from higher-income households than those who were already eligible. It is not obvious whether the estimates provide a lower or upper bound for the effect of loans on lower-income youth, because while they are more likely to be liquidity constrained they may also be more debt averse.

Table 1:
CPS Means
October, 1984-99

College Attendance in October	0.4300
Two-Year Public College	0.1337
Four-Year Public College	0.2036
Four-Year Private College	0.0792
State Median Home Value/$10K	15.03 (8.714)
Black	0.1510
Hispanic	0.1163
Age	18.50 (0.5000)
State Median Income/$10K	4.178 (0.0037)
State Mean Income/$10K	4.513 (0.0026)
September Unemployment Rate	0.0573 (0.0001)
N	60,593

Notes:
Dollar amounts are in constant 2000 dollars. Income is the median or mean in a given state and year of household income for 16-17-year-olds.

Table 2:
SIPP Means
1990 and 1992 Panels

	All	Owners	Renters
College Attendance	0.4211	0.4609	0.3209
Own Home Equity/$10K		8.363 (9.094)	
Own Home Value/$10K		13.90 (10.05)	
Black	0.1433	0.1065	0.2362
Hispanic	0.1120	0.0757	0.2033
Age	18.32 (0.2825)	18.35 (0.2882)	18.31 (0.2798)
Household Income/$10K	5.721 (4.147)	6.621 (4.263)	3.453 (2.750)
Home Owner	0.7160		
N	1284	932	352

Notes:
Dollar amounts are in constant 2000 dollars. Income is measured in the January preceding the academic year in which college attendance is measured.

Table 3:
October CPS, 1984-99
Home Values and College Attendance
OLS Regressions

	(1)	(2)	(3)	(4)
Home Value/$10K	0.0020 (0.0005)	0.0027 (0.0004)	0.0027 (0.0004)	-0.0056 (0.0011)
Year Effects		Y	Y	Y
Race, Age, Ethnicity Dummies & Income		Y	Y	Y
Unemployment Rate			Y	Y
State Fixed Effects				Y
R^2	0.00	0.04	0.04	0.05
N	60593	60593	60593	60593

Note:
Regressions are weighted by CPS sample weights. Standard errors are adjusted for heteroskedasticity and correlation within state-year cells. "Income" is state-year mean and median of household income of 16-17-year-olds. Dollar amounts are inflated to 2000 values.

Table 4: The Effect of Home Values on College Attendance, by Year

Home Value*1999	-0.0045
	(0.0020)
Home Value*1998	-0.0034
	(0.0021)
Home Value*1997	-0.0016
	(0.0025)
Home Value*1996	-0.0038
	(0.0020)
Home Value*1995	-0.0040
	(0.0020)
Home Value*1994	-0.0026
	(0.0020)
Home Value*1993	-0.0041
	(0.0017)
Home Value*1992	-0.0073
	(0.0013)
Home Value*1991	-0.0079
	(0.0012)
Home Value*1990	-0.0070
	(0.0013)
Home Value*1989	-0.0080
	(0.0015)
Home Value*1988	-0.0089
	(0.0016)
Home Value*1987	-0.0090
	(0.0018)
Home Value*1986	-0.0094
	(0.0022)
Home Value*1985	-0.0112
	(0.0021)
Home Value*1984	-0.0102
	(0.0023)
Year & State Effects	Y
Race, Age, Ethnicity, State Income, Unemployment Rate	Y
Region*1993+; all covariates interacted w/ 1993+	Y
R^2	0.05

Notes: Regressions are weighted by CPS sample weights. Income is state-year mean and median of household income of 16-17-year-olds. Dollar amounts are inflated to 2000 values. Standard errors are adjusted for heteroskedasticity and correlation within state-year cells.

Table 5:
College Attendance and College Choice
CPS 1984-99

	(1) College Attendance	(2) Two-Year Public	(3) Four-Year Public	(4) Four-Year Private
Home Value*1993+	0.0104	-0.0018	0.0153	-0.0029
	(0.0048)	(0.0036)	(0.0045)	(0.0032)
Home Value	-0.0137	-0.0038	-0.0128	0.0053
	(0.0036)	(0.0030)	(0.0035)	(0.0026)
Home Value*Time*1993+	-0.0004	0.0002	-0.0009	0.0003
	(0.0003)	(0.0002)	(0.0003)	(0.0002)
Home Value*Time	0.0004	0.0001	0.0006	-0.0003
	(0.0002)	(0.0001)	(0.0002)	(0.0001)
Year & State Effects, Race, Age & Ethnicity, Unemployment, State Income, Region*1993+, All covariates interacted w/ 1993+	Y	Y	Y	Y
R^2	0.05	0.04	0.03	0.04
N	60593	60593	60593	60593

Note: See Table 4

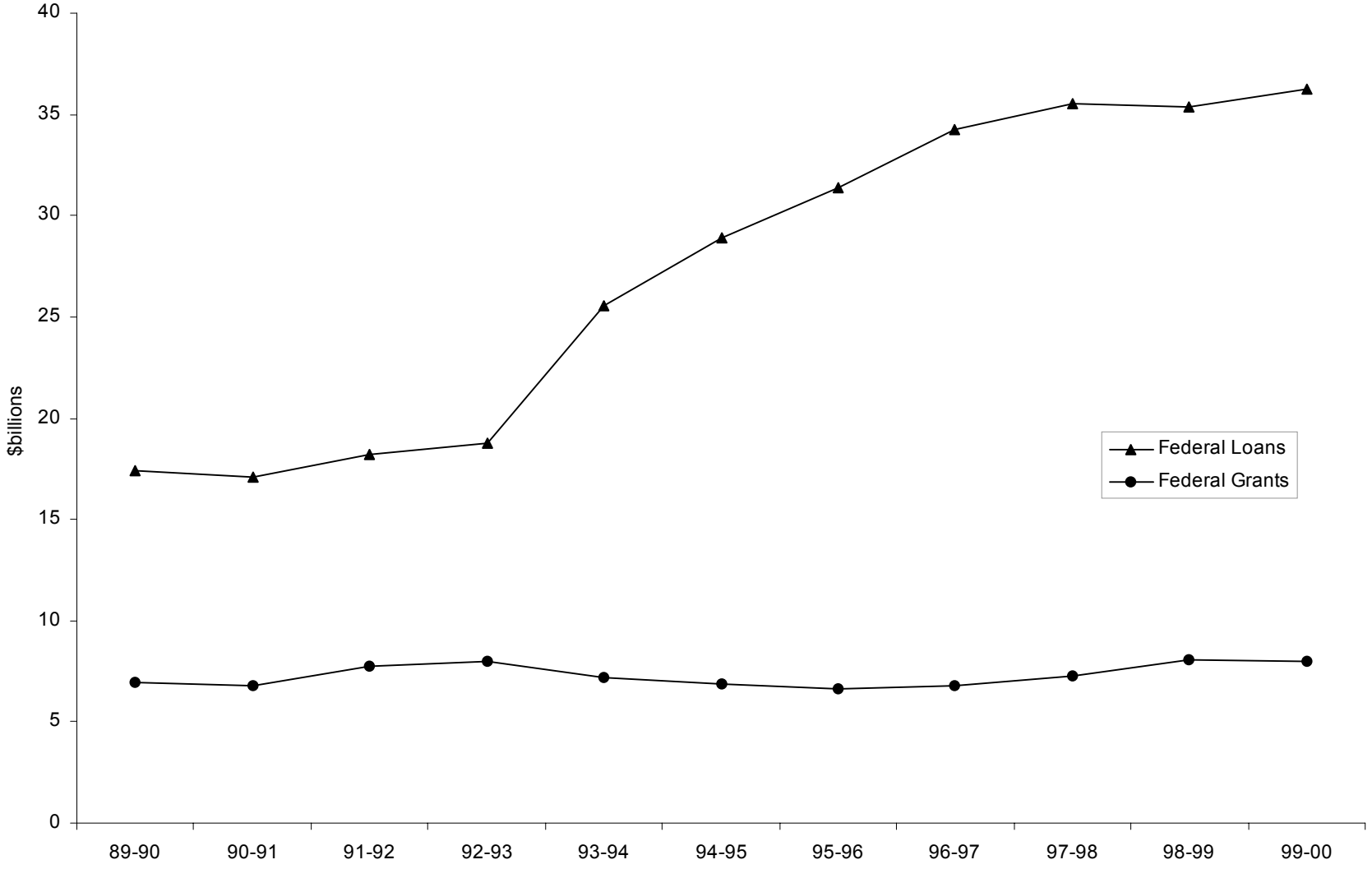
Table 6:
SIPP
Home Values and College Attendance
OLS Regressions

	(1) Own Home Equity	(2) IV for Equity w/ Own Home Value	(3) IV	(4) IV
	Owners	*Owners*	*Low-Income Owners*	*High-Income Owners*
Home Equity*1993	0.0026	0.0095	0.0041	0.0134
	(0.0042)	(0.0055)	(0.0085)	(0.0074)
Home Equity/$10K	0.0004	0.0021	0.0038	-0.0001
	(0.0031)	(0.0038)	(0.0053)	(0.0054)
Year & State Effects, Race, Ethnicity, Unemployment, Household Income, Region*1993, All covariates interacted w/ 1993	Y	Y	Y	Y
R^2	0.11			
N	932	932	467	465

Notes:
Attendance is a dummy for having attended college anytime in the academic year. Income is parental income and is measured the year before attendance is measured. In Column (4), equity and the interaction of equity with the after dummy are instrumented for with own home value and its interaction with the after dummy. Standard errors are adjusted for heteroskedasticity.

Figure 1: Loan and Grant Volume

Loan provisions of HEA92 first effective AY93-94



Note: Figures are from College Board (2000). Values are inflated by the CPI-U with academic year 1999-2000 as the base year.

Figure 2A: Median Home Values in 1984

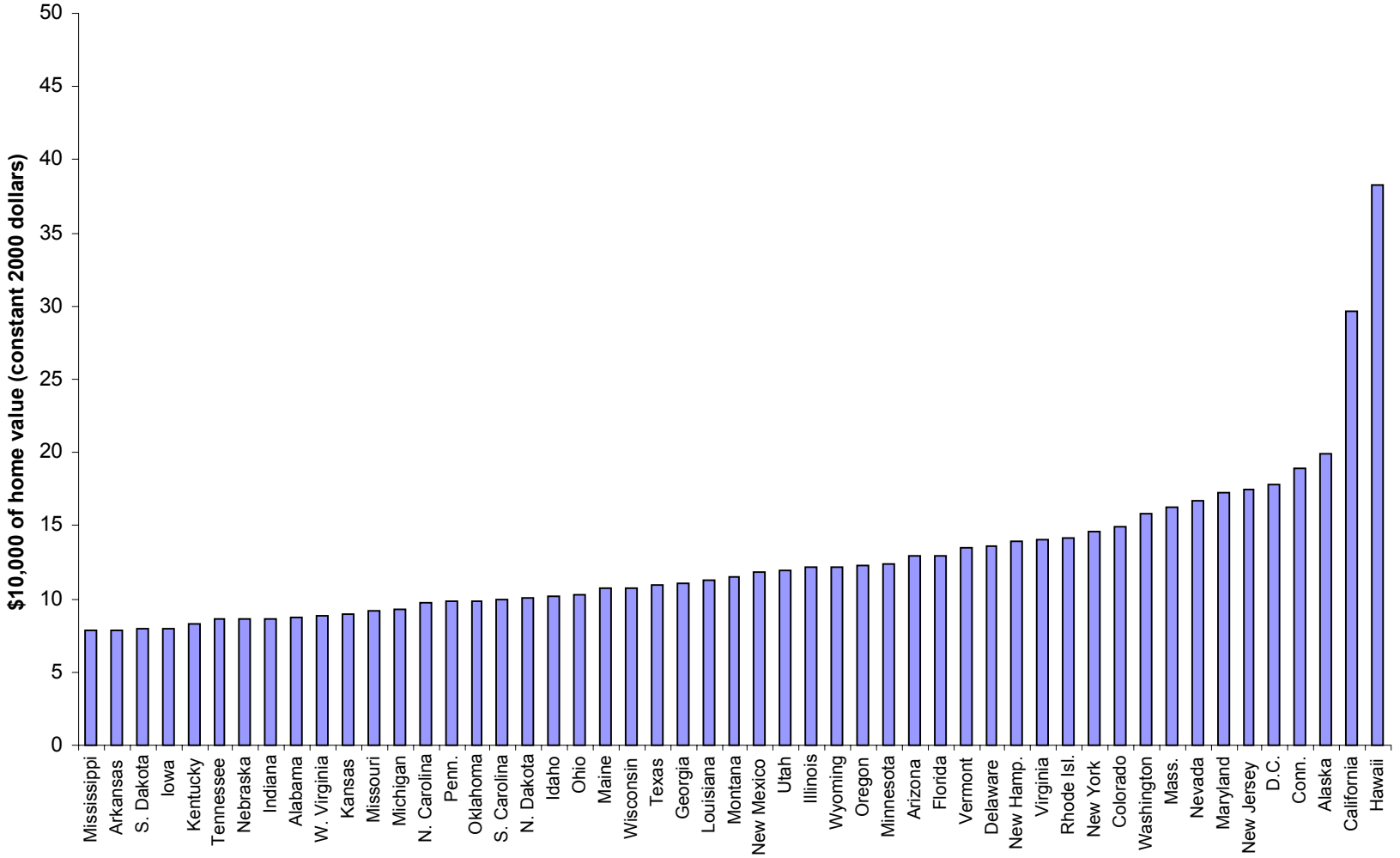


Figure 2B: Median Home Values in 1999

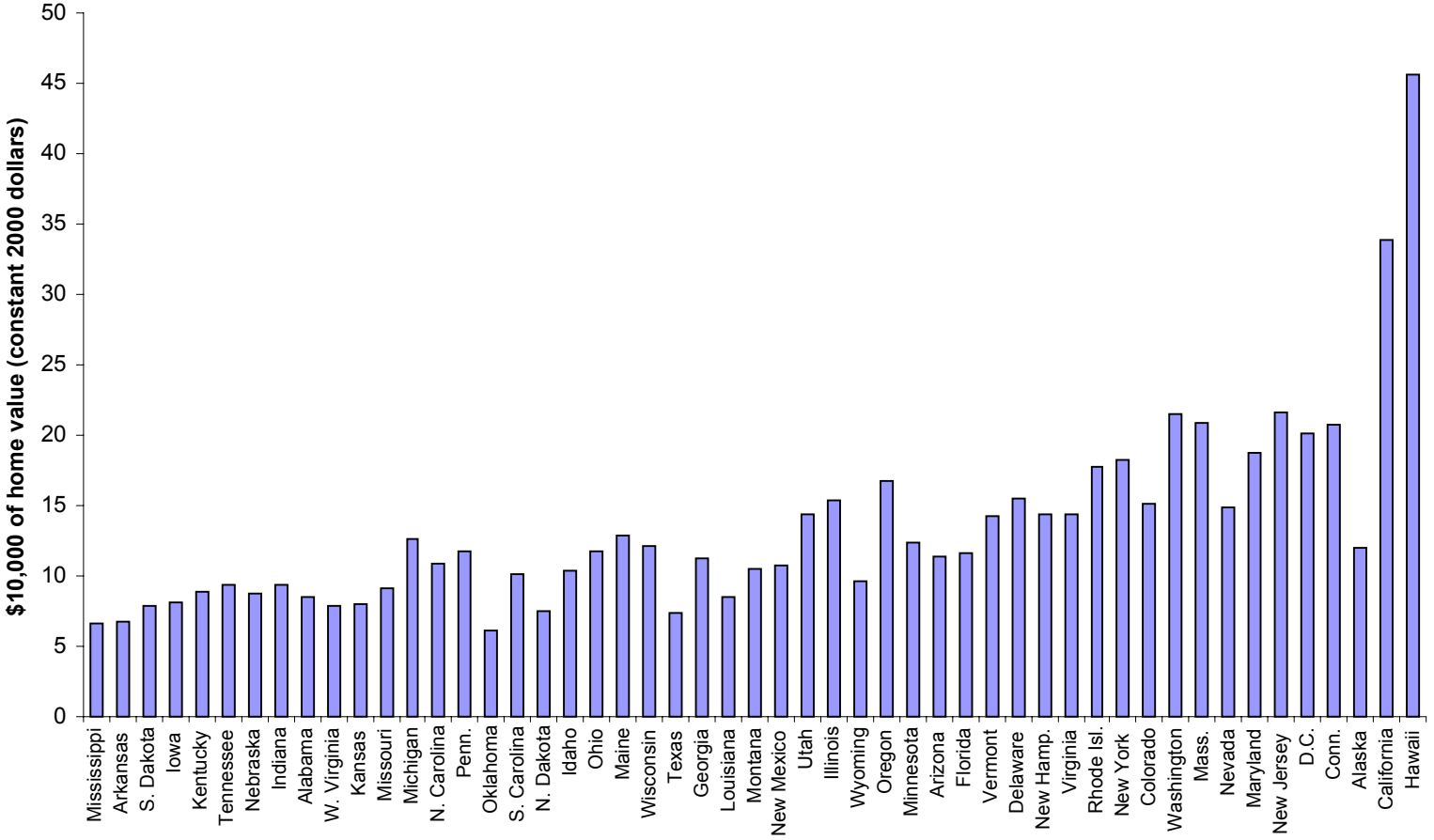


Figure 3: Effect of Home Value on Attendance, by Year

1992 effect normed to zero

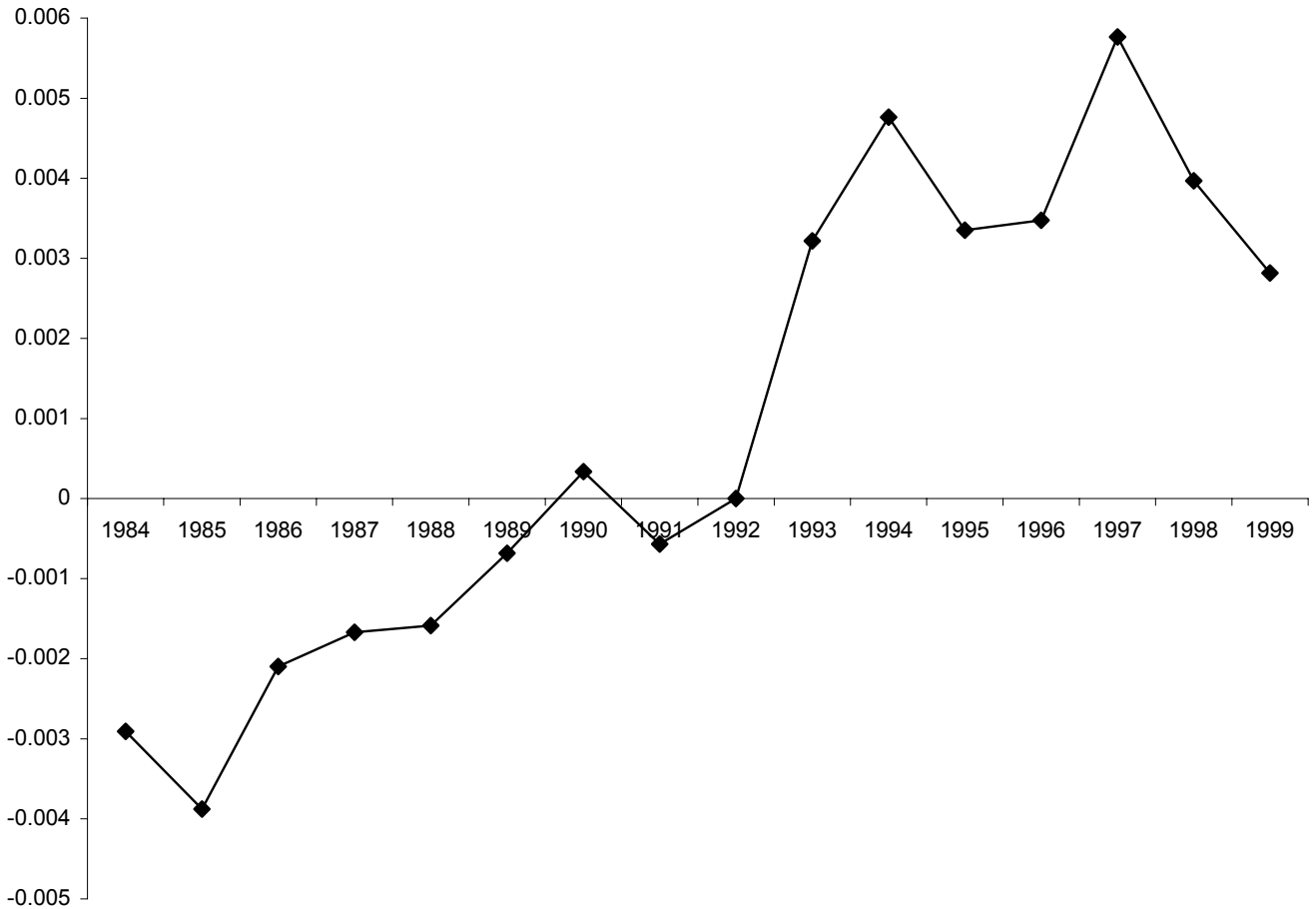


Figure 4A: College Attendance Rate of 18-19-year-olds

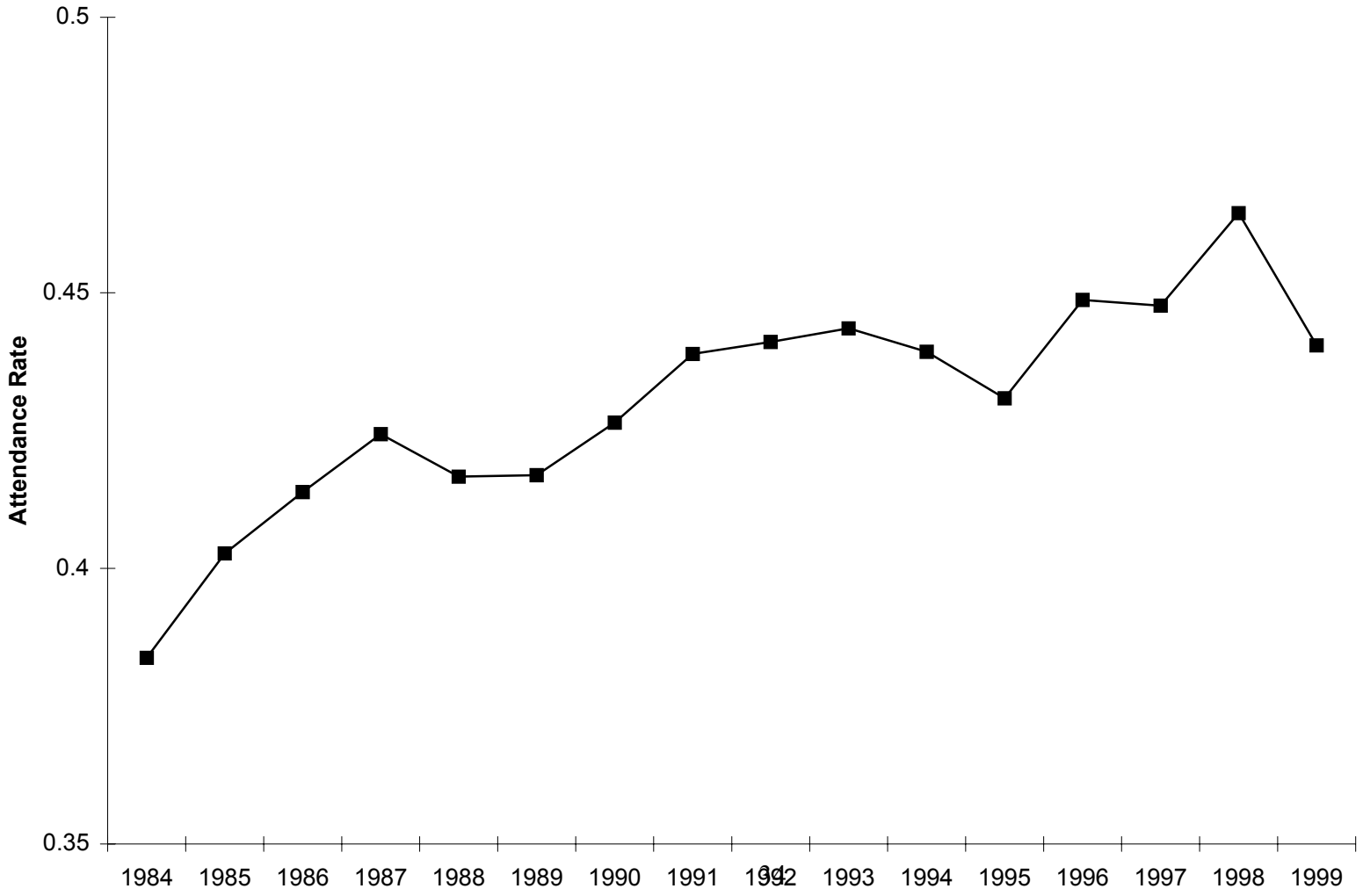
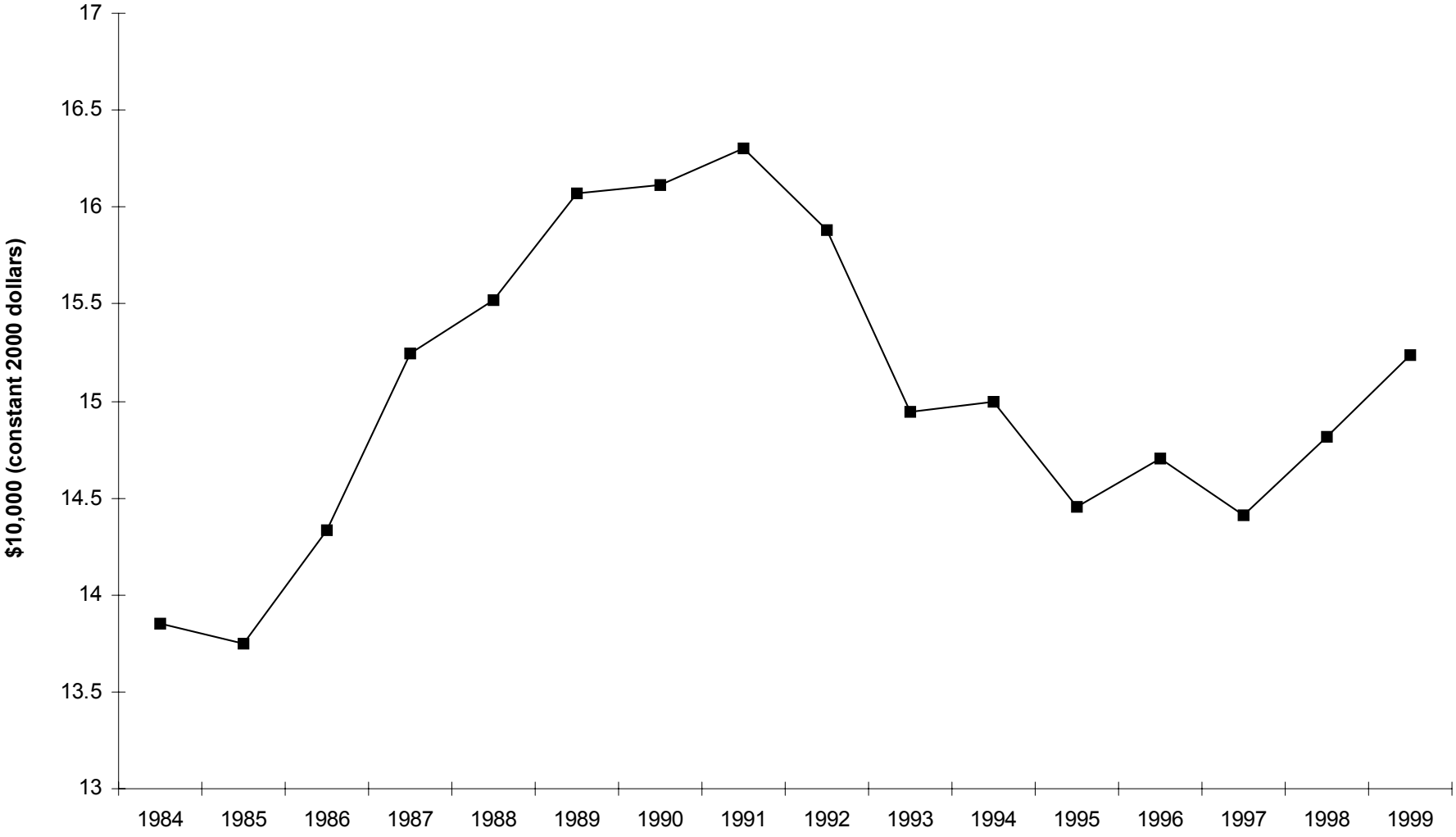


Figure 4B: Average of State Home Values
Weighted by Population of 18-19-year-olds



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