Pell Grants and Student Success: Evidence from the Universe of Federal Aid Recipients*

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1 Introduction

Over the past decade, rising college prices and student debt levels combined with new data on stagnant economic mobility have intensified debates over the way higher education is financed in the United States. National candidates for political office have offered proposals to make college available "free" for some students, and several states and municipalities have adopted state or local initiatives that pursue that goal to varying degrees. These initiatives have grown in part out of a dissatisfaction with current grant programs aimed at lowering the price of college for low income students, which have been criticized as overly complex, insufficiently generous, and thus ineffective.

The federal government's Pell Grant is the largest such program aimed at reducing disparities in college going and success between students from rich and poor families. At a total budgetary cost of about $30 billion spread across six to seven million students in recent years, Pell grants currently subsidize the costs of attendance of low income students up to a maximum award of about $6,300 per student. Since its creation nearly fifty years ago, however, evidence on whether this subsidy has helped decrease inequality in college access and success has been largely disappointing until recently.

Like other grants for college, Pell is intended to reduce the net price of college attendance, and thus increase the probability of attendance for students who qualify. Yet despite ample evidence that grant aid can increase enrollment in many contexts (Barr 2019; Castleman and Long 2016; Cornwell et al. 2006; Deming and Dynarski 2010; Dynarski 2003; Scott-Clayton 2011), most studies have found little or no impacts of Pell on enrollment (Carruthers and Welch 2020; Denning 2016; Denning et al. 2019; Hansen 1983; Kane 1995; Marx and Turner 2018; Turner 2017).[1] Prominent explanations for this puzzle include hassle costs created by the complexity of applying for aid serving as a deterrent (Deming and Dynarski 2010; Bettinger et al. 2012), lack of awareness of eligibility and benefits (Dynarski and Scott-Clayton 2006; Hoxby and Turner 2012), and implicit or explicit

[1] Some exceptions are Seftor and Turner (2002) who both find tentative evidence that the introduction of Pell led to enrollment gains for older, independent students and Denning et al. (2019) who find increases in enrollment among 2-year Texas public schools.

interactions with other sources of aid, including institutional and state grants as well as loans, that undermine the intended impact of Pell on students' net prices or cash-on-hand (Turner 2017; Park and Scott-Clayton 2018; Marx and Turner 2018).

Even if Pell grants do not increase college enrollment, they might still improve outcomes among low-income students. A handful of recent studies have found impacts of grant aid on completion and other outcomes among students already enrolled (Denning forthcoming; Denning et al. 2019; Goldrick-Rab et al. 2012). It could be that students respond to grant aid by shifting enrollment to higher (or lower) quality institutions (Angrist et al. 2016; Cohodes and Goodman 2014; Cornwell et al. 2006; Dynarski 2000). Alternatively, the increased 'cash-on-hand' could allow students to allocate more time to studying rather than work (Darolia 2014; Denning forthcoming; Goldrick-Rab et al. 2016), and as a result increase enrollment intensity and academic momentum, improve learning and grades, or switch to more challenging majors (Anderson et al. 2020; Castleman et al. 2017; Denning et al. 2019; Sjoquist and Winters 2015). In general, the evidence for Pell has also been largely discouraging in this regard. Marx and Turner (2018) leverage a discontinuity in Pell awards near the eligibility threshold to show that increases in Pell grants at an urban community college system have no effect on college choice, credit attainment, or college completion. Using a similar research design studying community colleges in a single state, Park and Scott-Clayton (2018) also find little impact of Pell on course grades, credits earned, or completion rates despite evidence increased Pell grants allow students to work slightly less. Finally, Carruthers and Welch (2020) apply the same design to graduating high-school seniors in Tennessee, and find little evidence that increased Pell eligibility affects measures of college quality conditional on enrollment.

Against this backdrop of lackluster evidence on Pell's impact on students, a recent study by Denning et al. (2019) stands out. Focusing on a different discontinuity in the Pell eligibility formula affecting a lower-income group of students, they find much more positive effects on measures strongly associated with economic mobility. They find that for students starting at public four-year colleges in Texas, increases in Pell eligibility led to large increases in completion and earnings: for a \$1,000 increase in grant aid they estimate an increase in 5-year BA completion of 5 percentage points (15 percent) and an increase in earnings measured six years post-enrollment of 4.8 percent. For other students, including continuing four-year students and students at two-year institutions, no effects on those outcomes were detected. The paper concludes that increases in Pell grants can be a “free lunch,” in the sense that the tax revenues generated by students’ increased earnings can more than offset the initial public expenditures on grants. This finding has been influential on researchers’ views of the Pell program, and is cited in an influential study by Hendren and Sprung-Keyser (2020) as an example of a government program with an infinite marginal value of public funds. Nevertheless, it stands in contrast to much of the previous literature that estimates more modest effects of Pell on completion, and it is important to understand what underlies these differences and the extent to which the results generalize.

There are strong reasons to suspect that “the” effect of Pell may depend on a variety of state, institution, and student characteristics, and on other contextual factors like when in a students’ career aid is given. One reason, noted above, is that increases in Pell aid might ‘crowd-out’ or ‘crowd-in’ other sources of aid in different ways depending on the context, leading to different impacts on net-price, borrowing and cash-on-hand. Turner (2017) shows that one institution ‘captured’ about 67 cents of every dollar increase in Pell grant aid through reductions in institutional aid, suggesting one layer of heterogeneity related to the size of colleges’ aid budgets. Similarly, Park and Scott-Clayton (2018) find that increases in Pell affect students’ aid packages differently based on whether institutions participate in federal loan programs: Pell grant increases were offset with reductions in loans at programs that do, but with reductions in state aid at institutions that do not. Other studies, including Denning et al. (2019), find evidence that increases in Pell lead to increases in state aid.

This paper attempts to estimate “the” average effect of Pell on student outcomes across a much broader swath of higher education than has been examined in the literature to date. We apply a similar research design as in Denning et al. (2019) to administrative data on every undergraduate student receiving federal financial aid assistance that started their college careers between 2002 and 2014— in total, our data cover the experiences of over 29 million students. While our data contain less detail to shed light on mechanisms than some previous state- and institution-level studies, they cover every Pell recipient across the nation and thus provide a more generalizable estimate of the average impact of Pell grant increases generated by this source of variation on a key outcome— college completion. Moreover, our rich data allow us to estimate Pell’s impact on independent students without children using a regression kink design created by the same Pell eligibility formula.

Averaging our results over all years available, we find additional Pell eligibility has a small but positive impact on the likelihood of completing a degree, measured up to six years after starting. We find tentative evidence that Pell may increase the probability of completion by about 2 to 3 percent for dependent students (e.g., completion rates at the original institution within four years rises from 37.2 to 38.1 percent), and twice that for independent students, but the statistical significance of these results are not robust across our specification tests. We also do not find evidence that earnings after college are impacted by Pell eligibility, although these estimates are somewhat noisy. Our results for completion are substantially smaller than those reported by Denning et al. (2019), a discrepancy we suggest may be partly driven by interactions between Pell and a particularly generous state aid program in Texas. Our findings underscore the point that given the diversity of student aid, and higher education more generally, researchers should be careful to consider the specifics of their research setting and how it influences the generalizability of their results.

The next section presents the research design and the unique data we use in the project. We discuss how aspects of the federal Pell grant eligibility formula for different types of students create quasi-experimental variation in Pell eligibility amounts and describe how the strengths and weaknesses of our data inform important aspects of our research design. Section 3 explains our empirical methods and potential threats to the validity of the design. After ruling out these potential threats, we present estimates of Pell eligibility on completion and earnings outcomes in Section 4. Section 5 discusses the discrepancy between our results and the findings of Denning et al. (2019). Section 6 concludes.

2 Research Design and Data

In order to apply for federal financial aid in the United States, students must file a Free Application for Federal Student Aid (FAFSA) in the Spring prior to the academic year in which they plan to enroll in college. The FAFSA is a long online form with detailed questions about student and family financial resources and obligations that are used to create an index of each student's family's ability to pay for college: their expected family contribution (EFC). In this section, we briefly describe how this index is used to determine Pell eligibility and other student aid and the institutional features of this process that we exploit to estimate the impact of increases in Pell eligibility amounts on student outcomes.

2.1 The EFC Formula

Students' Pell grant eligibility is calculated as the (positive) difference between the maximum Pell amount for the year— set by Congress— and their Expected Family Contribution (EFC). Despite the apparent simplicity of this structure, there are several reasons why changes in Pell eligibility may not transfer dollar-for-dollar into changes in student resources available to pay for college. Colleges generally 'package' grant and loan aid to students and receive information on students' Pell award eligibility before packaging institutional and state grant aid or loan offers. Institutions have discretion, therefore, to adjust their aid packages in ways that may offset (or amplify) increases in Pell eligibility. Similarly, some state aid programs explicitly or implicitly use the federal EFC in determining their awards, creating a mechanical link between changes in Pell eligibility and changes in state aid. Finally, the Pell grants that students are awarded are pro-rated when students enroll part-time, so grant awards can differ from eligibility amounts.

This paper uses non-linearities in expected Pell eligibility as a function of family-income created by specific features of the EFC formula. The EFC index is calculated using measures of student and family income and assets, net of certain expenses, through a formula that generally leads to higher EFC and thus lower Pell eligibility for families with higher net income and assets. The parameters of the formula are generally updated from year to year and differ across three different "types" of students: dependent, independent with dependents, and independent without dependents. In general, a student is considered independent if she will be 24 or older on January 1st of the year she receives financial aid, is married or separated, is enrolling in a graduate program, is serving or has served in the military, or has dependents of her own (U.S. Department of Education 2017).

The EFC formula, much like rules for determining other government benefits such as cash assistance from the Temporary Assistance for Needy Families program, disregards low levels of income and thus results in a kinked relationship between Pell grant eligibility and family income. This is shown in the bottom panel of Figure 1, which illustrates the Pell eligibility at various family income levels of a hypothetical independent student without dependents using the 2010 EFC Formula.[2] Students with zero income are eligible for the maximum Pell award (set yearly by

[2]For this example, we assume that the student is single, lives in Michigan (the state tax allowance in the EFC

Congress), and as income rises up to a low level called the Income Protection Allowance (IPA), Pell eligibility is held constant (i.e., 100 percent of income is disregarded) since all income below this level is deemed necessary to pay for life's necessities. Once income rises above the IPA, some fraction of every dollar earned is deemed available to pay for the student's education, so EFC starts to rise, and Pell eligibility falls accordingly. This results in a change in the slope of the relationship between Pell eligibility and income at the IPA threshold— it changes from zero (i.e., flat) to negative. In our example, each additional dollar beyond the IPA reduces Pell eligibility by 50 cents. The EFC formula also has progressivity built in. At higher incomes, a higher fraction of each additional dollar is assumed available for education, so the downward sloping part of the graph is piecewise linear, with successive segments becoming steeper. Once EFC increases beyond the maximum Pell amount, Pell eligibility is set to zero.[3]

For some low-income dependent students or independent students with dependents other than a spouse, the eligibility determination process is simplified and students are given an "automatic-zero EFC" (AZ) and thus eligible for the maximum Pell award. Dependent students are eligible for an AZ EFC if their parents' adjusted gross income falls below a threshold amount set by Congress for each year and they fall into one of the following eligibility categories: someone in their household received benefits in the previous two years from a means-tested government benefit program,[4] their parents were eligible to file taxes using a 1040A or 1040EZ form or were not required to file any income tax return two years ago,[5] or their parent is a dislocated worker. Independent students with dependents face similar rules except that the requirements apply to the independent students themselves and not their parents. To the extent that the AZ income threshold falls in a range where EFC would have been positive under the baseline EFC formula, the AZ policy results in a discontinuity in the Pell grants for which students are eligible. Students whose family incomes

formula varies with state of residence), has no assets, only has income from wages, and does not itemize her deductions.

[3]This discussion simplifies somewhat. There is a minimum Pell award set at 10 percent of the maximum award by Congress, so that when Pell eligibility falls below the minimum, Pell awards are set to zero. The minimum was $596 in 2018-2019 award year.

[4]These programs include the Supplemental Security Income (SSI) Program, the Supplemental Nutrition Assistance Program (SNAP), the Free and Reduced Price Student Lunch Program, the Temporary Assistance for Needy Families (TANF) program, and the Supplemental Nutrition Program for Women, Infants, and Children (WIC). Before 2010, the window of program participation was one year instead of two.

[5]The Tax Cuts and Jobs Acts eliminated the 1040A and 1040EZ forms, so beginning in award year 2020-2021, this requirement is now met as long as the parents did not file a Schedule 1 or only filed a Schedule 1 for certain additions or adjustments to income. In general, only taxpayers with simple tax returns could file using the 1040A or 1040EZ. They could only have certain types of income, could not itemize their deductions, and could claim a limited set of adjustments or credits.

fall just below the AZ income threshold receive the maximum Pell while students just above the threshold receive the maximum Pell less their (positive) EFC. This is shown in the top two panels of Figure 1, which show the relationship between Pell eligibility and income for hypothetical dependent and independent students with dependents.[6] For the dependent student, Figure 1 shows that if the student's parent has $30,000 of income or less, the student receives the maximum Pell for 2010, $5,350. If the student's parent instead makes $30,100 dollars, the student's Pell grant falls to about $4,610, a drop of $740. Note that independents without dependents are never eligible for the AZ EFC—hence the lack of discontinuity in the schedule shown in the bottom panel. Moreover, the AZ threshold varies across years, which results in different sized discontinuities in Pell eligibility across the threshold in different years.

2.2 Data

The data used in this study come from federal administrative data files from the Department of Education and the Internal Revenue Service that were assembled in 2014 and 2015 to build the College Scorecard website.[7] The data include every undergraduate student receiving aid under Title IV of the Higher Education Act (primarily Pell Grant and Stafford Loans recipients), grouped into cohorts based on the award year in which they first begin their studies at a particular institution. The primary source of student data is the National Student Loan Data System (NSLDS), an administrative database used to monitor aid receipt and payments on loans for all Title IV beneficiaries by the Federal Student Aid (FSA) Office in the Department of Education. Our extract of data contains a subset of information from the FAFSA form used to establish students' financial aid types, their family income, and an estimate of the student's expected family contribution that is used to estimate Pell eligibility. The data set contains information on enrollment and aid receipt for all students receiving aid, and indicators for whether a student completes his or her program of study if the student has federal loans. This limitation on completion data is because

[6] The hypothetical dependent student lives in a single parent household in Michigan with one younger sibling. We assume that the student's parent has no assets, only has income from wages, and does not itemize her deductions for federal income taxes. We assume that the independent student (the middle panel) is a single parent in Michigan with two dependent children under the age of 17. Again, we assume the independent student has no assets, only has income from wages, and does not itemize her deductions.

[7] The College Scorecard is an online database with a web-based interface designed to provide college-level information about the price of attendance, financial aid availability, graduation rates, average salaries of past students, and characteristics of the student body.

the Department initially collected the data so that it knew when students should start repaying their federal loans.[8] The most recent data from the NSLDS is the 2014 award year (i.e., the year ending in June of 2014). These data were merged to individual labor market earnings gleaned from federal tax data by staff in the U.S. Treasury Department, with the most recent data from the 2012 calendar year. Individual earnings for each calendar year reflect the sum of all wage and salary income plus deferred compensation reported on W-2 forms by employers, plus self-employment earnings reported by tax-filers on Schedule SE. Importantly, the W-2 earnings data are reported by employers, so they do not depend on a worker's filing status.[9]

Our study focuses on cohorts of federally aided students beginning their studies between the 2002 and 2010 award years ("award years" run from July 1 to June 30 in the referenced year). These are the cohorts for which we can observe four-year completion outcomes. The full cohorts across years range from a low of about 2.8 million in 2002 to about 5.8 million in 2010. We make several sample restrictions to this initial data. The most significant is that we drop students who are not in their first year of studies when they are first observed (e.g., in their first year receiving federal aid they report they are a second year student), eliminating about 15 percent of the sample overall. We then drop students who are missing the minimum amount of information required to implement our research design: including demographic information needed to determine their financial aid type (i.e., dependent, independent with dependents, or independent without dependents), income, and state of residence. This drops an additional 1 to 3 percent of observations across cohorts. After these restrictions are made, we have 29.9 million students across all cohorts between 2002 and 2010, ranging from 2.6 million in 2003 to 4.3 million in 2010. Of these students, about 22.1 million have federal loans and can be included in our analyses of completion. Descriptive statistics for the 2003 and 2010 cohorts, combining students both with and without loans, are shown in Table 1. These two cohorts are focal years of our analysis shown in the paper. Descriptive statistics for additional years are shown in Appendix Table 1. These tables report the means of various student characteristics and sample sizes for each cohort and student type. We additionally report means separately for the groups of students that are within $1,000 of the discontinuity and kink points that form the basis for the research designs used in the paper.

[8] In more recent years the Department has asked institutions to report completion outcomes for all aided students.

[9] Further details around the contents and construction of various data in the Scorecard data files are in Council of Economic Advisers (2015).

Due to inherent lags in the measurement of some outcomes, the set of student cohorts we use in our analyses varies across outcomes. For our main results on completion outcomes, the latest year for which data are available is 2014, so four-year completion data are available only through the 2010 cohort. In contrast, in supplemental analyses of school choice outcomes, we can estimate impacts for every cohort from 2002 to 2014 since we observe the schools that students attend in the same year students first receive their grants. For the earnings outcomes, the last year of earnings data is 2012, so we examine earnings only through the 2006 cohort when earnings can be observed about 6 years after students first enroll (e.g., a couple years after a student at a four-year school would enter the labor market if she graduated on time).

We implemented most analyses described below on individual level micro-data, but some analyses are conducted using 'collapsed' cell-level data.[10] These data cover the entire working sample described above, but contain only the sample sizes, averages, and variances of key covariates and outcomes for students within $100 'bins' of family income, separated by students' financial aid type. As we demonstrate below, for analyses where estimates from the micro data are available, the results from the cell-mean analyses yield very similar point estimates and standard errors. For our main estimates, Appendix tables summarize results estimated on both the micro data and the cell-level data side by side.

3 Empirical Methods

3.1 Identification

As described above, for dependent students and independents with their own dependents who meet the categorical eligibility requirements, the AZ policy creates a discontinuity in Pell eligibility among students with family incomes just above and below the income threshold. For these students, we estimate the impact of greater Pell awards on future outcomes using a regression discontinuity design. Formally, suppose an outcome of interest for student i, who is financial aid type a, starting

[10]This was done because we lost access to the micro-data midway through the project and before some analyses were complete. Where not already reported in the Appendix, complete results using both sets of data are available on request.

her studies in year t can be written as

$$Y_i = f(M_i) + \tau g_i(M_i) + \varepsilon_i,$$

where we suppress a and t subscripts on all variables and functions for notational ease. Here Y is the outcome of interest, $g(M)$ is the student's Pell eligibility as a function of family income M, $f(M)$ is some function of family income, and ε captures other variables that affect the outcome of interest. The parameter we want to estimate is τ, the causal effect of an additional dollar of Pell eligibility on the outcome Y. We begin by allowing this effect to vary across student aid types (i.e., for $a \in \{$*dependent, independents with dependents, and independents with no dependents*$\}$) and years, but we present average estimates across years below. We also explore heterogeneity across student subgroups and sectors of higher education. With a discontinuity in $g(m)$ at the AZ threshold m_0, we use a regression discontinuity design (RD) (Lee and Lemieux 2010; Imbens and Lemieux 2008) to estimate the effect of Pell eligibility on students' outcomes. If the conditional expectation of other determinants of the outcome variable are continuous at the threshold, this estimator consistently estimates τ (Hahn et al. 2001).

For independent students without dependents, we take advantage of the kink in $g(m)$ at the income protection allowance amount m_1. The IPA creates a discontinuity in the derivative of $g(m)$ as a function of family income M (written $g'(m)$ below). This discontinuity results in the regression kink design (RK) (Card et al. 2015) estimand

$$\tau = \frac{\lim_{m \uparrow m_1} \frac{dE[Y|M=m]}{dm} - \lim_{m \downarrow m_1} \frac{dE[Y|M=m]}{dm}}{\lim_{m \uparrow m_1} g'(M=m) - \lim_{m \downarrow m_1} g'(M=m)}.$$

Here, we measure the impact of increased Pell grants on outcomes by estimating the change in the slope of the relationship between average outcomes and family income at the IPA threshold, scaled by the the kink in $g(m)$ at the income threshold. This estimand identifies τ under the assumptions that the partial effect of family income on Y is continuous and that the distribution of family income conditional on the unobservables is continuous at the threshold (Card et al. 2015).

For each student type, we implement the RD or RK estimators described above separately across cohorts t using local linear regression methods. We use the optimal bandwidth selector proposed by Calonico et al. (2014) to choose a bandwidth for each outcome variable we study, and assess the sensitivity of our results to other bandwidth choices. We observe a significant amount of heaping of family income reports at multiples of \$1,000 that is even more pronounced at multiples of \$5,000. To avoid potential biases due to this heaping (Barreca et al. 2011), we drop observations with these values of income when estimating τ. Under the assumption that this propensity to 'heap' is continuous at the AZ and IPA thresholds, this approach yields consistent estimates of τ.

A potential complication arising with the RK estimator described above is that the IPA threshold is sometimes close to the income threshold at which individuals begin to have positive federal income tax liability, and thus are legally required to file their taxes with the IRS. Saez (2010) shows that income data are bunched to the left of that cutoff point, which may induce non-linearities in average individual characteristics that lead to bias in the RK estimator. These thresholds are sufficiently far from the AZ income thresholds that they are outside the bandwidths chosen for estimation, and thus unlikely to create bias for our RD estimates. While we show below that observed covariates generally do not display kinks near these points, our RK estimates do tend to be sensitive to bandwidth choices, so caution is warranted in interpreting results from the RK design for this reason.

3.2 Validity of the RD and RK Designs

Identification in the RD and RK research designs rests on smoothness conditions on the conditional expectation (and its first derivative) of $f(M)$ and ε with respect to family income near the AZ and IPA thresholds (Hahn et al. 2001; Card et al. 2015). In other words, students with incomes near the threshold must be sufficiently similar that no other determinants of outcomes exhibit discontinuities or kinks in their conditional expectations near the AZ and IPA thresholds. Explicit manipulation of family income in FAFSA reporting seems unlikely, as students are very unlikely to be aware of AZ and IPA rules and even if they were aware, it seems unlikely they would falsely report their incomes to affect their financial aid eligibility— FAFSA income is often verified against IRS earnings data, and estimated rates of misreporting are thought to be very low (Warick 2018). A more likely possibility, however, is that increased Pell eligibility or the discrete change in the complexity of filling out a FAFSA form driven by the AZ policy causes more students to enroll. Since our study examines the impact of increased Pell eligibility on student outcomes conditional on enrollment, an enrollment effect of AZ policies could lead to sample selection bias in our estimates of τ.

In a companion study, Matsudaira (2017) explores whether the AZ policy affects enrollments by testing for discontinuities in the density of family income around the threshold among enrolled students. Figure 2 displays the densities of family income in 2010 by student aid type. The dashed lines in the figures indicate the AZ or IPA threshold. Though small in magnitude, Matsudaira (2017) shows there is a slight enrollment effect at the AZ threshold of about 4 percent on average for dependent students and about 1.9 percent for independent students with children. Across individual cohorts, estimates of the discontinuity range from 1.7 to 6.5 percent for dependent students and are occasionally negative and lacking statistical significance for independent students with dependents, as displayed in Table A.2.[11] Matsudaira (2017) finds the discontinuity in density is unlikely to be caused by either an impact of Pell on enrollment or an effect of the simplified application procedure created by AZ policies. Regardless of the mechanisms, however, if the change in enrollment at the threshold results in a change in the composition of students on either side of the threshold, then our estimates of the effects of Pell could be slightly biased.

Reassuringly, our analyses show that students' observable demographic characteristics do not differ significantly across the AZ and IPA thresholds. We examine predetermined covariates including age, gender, whether a student's mother graduated from college, and characteristics of the student's neighborhood (based on the zip code used on the FAFSA), including the percent of residents who are white, the percent born in the United States, the percent of households below the poverty line, the unemployment rate, and the median household income in 1999.[12] Graphs of the individual characteristics for students in the 2010 cohort are in Figure A.1. We find no visible discontinuities or kinks in the variables at the relevant thresholds and none of the estimated discontinuities or kinks are statistically significant. This is true in other years as well: Figures A.2 through A.4, for example, present graphs of individual and neighborhood characteristics for the 2003 cohort. These analyses suggest that the scope for bias due to differences in enrollment at the AZ threshold is low.

A further cause for concern arises in our analysis of completion rates. The administrative data

[11] For dependent students and independent students with dependents there is also evidence of bunching at the income level that corresponds to receiving the maximum Earned Income Tax Credit with two or more children, indicated by the dotted lines in the figures.

[12] The neighborhood data are based on 5-digit zip codes in a student's address on the FAFSA merged to zip code level information from the 2000 Census.

we use from the NSLDS was designed to track completions only for the purpose of triggering repayment status on federal loans. As such, completions are recorded accurately only for borrowers (though this has changed since our study period). Greater amounts of grant aid would be expected to decrease students' need to take on student loans in their first year of college, and perhaps alter whether students ever borrow.[13] This could affect our degree completion estimates since we can only observe degree completion for students with loans and thus, could have differential selection into our analysis sample near the cutoff.

Similar to other studies, we find that students who barely qualify for the automatic zero threshold and thus the maximum Pell grant are slightly less likely to take federal loans. For both dependent students and independents with dependents, an additional \$1,000 in Pell eligibility decreases the likelihood of borrowing by about 2 percentage points, relative to base probabilities of borrowing of 60 and 72 percent respectively (i.e., relative declines of 3.3 and 2.8 percent). Table A.3 displays the weighted average of IV estimates across the 2002 to 2014 cohorts while Figure A.5 plots the the reduced form relationship between family income and the use of loans for the 2010 cohort. While some of the discontinuities are statistically significant, they are small and given the evidence that students have similar observable characteristics on either side of the threshold, we expect the differences in loan rates will not result in large biases in our completion estimates.

In summary, there is some evidence that the smoothness conditions for RD are violated, and the estimates we show below may be affected by selection bias. The scope of the magnitude of this selection, however, is very small given the very small estimated discontinuities in the density of the running variable and the probability of borrowing (and thus having a valid completion measure). Moreover, observable characteristics correlated with the outcome exhibit no discontinuities at the RD thresholds. To the extent discontinuities in the average unobserved determinants of outcomes are induced by the slight enrollment and borrowing effects we document, standard reasoning suggests the bias is likely to understate any positive impact Pell has on student outcomes. Since students with incomes just below the AZ threshold are entitled to more Pell and faced a simpler application process, we might expect them to be negatively selected in terms of their likely outcomes relative to students with family incomes just above the threshold who also enrolled in

[13]While several studies have shown effects of Pell eligibility on borrowing amounts in the same year (Marx and Turner 2018; Park and Scott-Clayton 2018), what is relevant for whether completions are observed is whether Pell eligibility in their first year affects whether students *ever* borrow.

college despite not receiving greater assistance.

Finally, we consider an additional measure of the internal validity of our results by assessing the sensitivity of our estimates to the choice of bandwidth. In all analyses, we use the optimal bandwidth selector developed by Calonico et al. (2014) (henceforth CCT). We evaluate the robustness of our results by estimating the discontinuities and kinks over a range of bandwidths and evaluating whether these estimates are reasonably stable across bandwidths between 50 and 150% of the CCT bandwidth. Below, in summarizing results across years we estimate both an average of estimates across all years, and an estimate across only years where the result is robust to choices of bandwidth according to this definition. In practice these average estimates are qualitatively similar so we focus on results averaging over all years and only note when the estimates are meaningfully different. In analyses not presented here, we also conduct permutation tests that the discontinuities and kinks we estimate at the AZ and IPA thresholds are larger than we would expect under continuity or smoothness of the outcome variables. These tests are similar to those proposed in Ganong and Jäger (2018). We use these permutation tests to obtain an alternate p-value for the significance of our estimates. The tests rarely differ from inference with traditional standard errors, so we generally omit discussion of them for brevity.[14]

4 Results

We use the research designs explained above to estimate the impact of Pell eligibility on completion rates and post-enrollment earnings by student type and for each cohort-year in which the outcome variable is measured. To simplify the exposition of these many results while still giving the reader a sense for how the raw data inform our conclusions, we present results as follows. First, we show graphical evidence standard in the RD and RK literature on the first-stage and reduced-form relationships between Pell eligibility, the outcome of interest, and family income for one of two focal cohort-years, 2003 or 2010. For completion outcomes we focus on 2010 because the discontinuity

[14]In the permutation tests, we focus on the set of placebo thresholds that are unlikely to be affected by potential bunching induced by the income tax schedule or any potential effects of the Simplified Need Threshold (SNT), the income level where students qualify to use a simplified EFC formula. To do this, we only calculate placebo estimates at placebo thresholds that are more than one half CCT bandwidth away from the first kink in the EITC schedule for tax units with two children and similarly distant from the SNT. The SNT is at $50,000 of adjusted gross income in all years while the first kink of the EITC is indexed for inflation each year and was at $14,040 of earned income in 2017 for families with two children. Further details of this procedure are available on request.

caused by the automatic zero EFC rule was largest in 2010 and thus we expect our tests to have the most power in this year. It is also the most recent year in which we can observe four-year completion outcomes since 2014 is the last year that completion data are available. However, we cannot observe work and earnings outcomes for the 2010 cohort in our data, since they are measured at earliest six years after students begin their studies, and 2012 is the most recent calendar year that earnings and employment information is available in our data. Thus, for earnings analyses we use data from the 2003 cohort to illustrate our findings. Second, we graphically summarize the heterogeneity in results across cohort-years for selected outcomes by student type. Since in most cases a simple test for heterogeneity fails to reject the null that the estimated effects are constant across cohorts, we present estimates of the average (minimum-distance) of instrumental variables estimates of the effect of Pell eligibility across all cohorts. Because estimates in a particular cohort-year sometimes fail standard tests of robustness (e.g., if the estimate exhibits substantial sensitivity to bandwidth choices), we also calculate minimum-distance average estimates that omit non-robust estimates and report both sets of estimates.[15]

4.1 Effect of the Automatic Zero EFC and Income Protection Allowance Policies on Pell Eligibility

To assess how the automatic zero and income protection allowance policies affect Pell grants, we focus on Pell grant eligibility. This is calculated as the maximum Pell award in the relevant year, minus the student's actual EFC as calculated by the Department of Education, and represents the maximum Pell eligibility for a full-time student enrolled throughout the academic year. This can and commonly does differ from the actual amount in Pell Grants a student receives, since many students attend less than the full-year, or are enrolled part-time and thus receive pro-rated grants. We focus on estimating the effect of Pell eligibility rather than actual Pell awards since enrollment intensity may itself be influenced by the eligibility amount, and moreover we do not observe administrative data on Pell awards. Using data from the 2012 National Postsecondary Student Aid Study (NPSAS), we estimate that actual Pell grants received are about 77 percent of the simulated Pell for dependent students near the AZ threshold and about 64 percent for

[15]For those analyses we adopt an admittedly ad hoc definition of robust, based on whether coefficient estimates remain within one standard error of the preferred estimate for alternate bandwidth choices ranging from 0.5 to 1.5 times the CCT bandwidth.

independent students with dependents.[16]

Our measure of Pell eligibility is based on the FAFSA from the first award year that students received federal aid. Because we restrict our sample to students who are first observed in their first year of study, our IV estimates using this measure of Pell thus capture the effect of a student being eligible for additional Pell in their first year, the same subgroup where Denning et al. (2019) found that Pell grants had the most impact for students in four-year public colleges in Texas.

Figure 3 presents the average simulated Pell Grant eligibility for students in $100 income bins for 2010 and 2003. In 2010, the difference in average Pell grant eligibility between dependent students just below and just above the $30,000 family income AZ threshold was about $855 (except where noted, figures are in real 2012 dollars). Students with family incomes just below the AZ threshold were thus eligible for nearly 20 percent more in Pell grants than those with family income just above the threshold. For independent students with dependents, the discontinuity in 2010 was $409, or 8 percent of the average Pell grant for students with family income just above the threshold. The estimated discontinuities differ slightly from the discontinuities we presented with the example students in Figure 1 due to differences in family composition, assets, and taxes paid. In 2003, the other focal year of our study, the estimated discontinuities were $205 for dependent students and $36 for independent students with dependents. The discontinuities are much smaller because the AZ threshold was much lower— only $13,000— in that year.[17]

For independent students without dependents, the bottom panel of Figure 3 exhibits a visible kink in average Pell eligibility just at the IPA threshold. In 2010, additional income less taxes paid had no effect on students' Pell grants up to the IPA of $7,000. Past $7,000, an additional dollar of income reduced students' Pell grant eligibility by about 34 cents. A kink of similar magnitude occurs in 2003 at $5,300 of family income, the IPA in that year.

Table 2 displays the estimated discontinuities in the Pell schedule for every year from 2002 to

[16]Actual Pell receipt may also be affected by the FAFSA verification process, which requires students to submit additional information to verify the accuracy of their FAFSA. During our study period, between 36 and 66 percent of Pell-eligible students were selected for the verification process (U.S. Department of Education 2019). The National College Attainment Network estimates that about a quarter of these students do not complete the verification process and therefore do not receive a Pell grant (DeBaun 2018). Very little public information exists about the FAFSA verification process, but it is possible that students that qualify for the automatic zero could face lower rates of verification since they submit less information that could be potentially flagged as error-prone. If so, the discontinuity in actual Pell grants received could be larger than our estimate.

[17]We express the running variable, AZ threshold, and IPA kink-points relevant for the research design in current dollars throughout.

2014, and the estimated kinks for the years 2002 to 2004, and 2009 and 2010. The Table shows that the size of the discontinuity in Pell at the AZ threshold has varied over time based on where Congress set the AZ threshold, and to a lesser extent on the maximum Pell award. For dependent students, it rose from \$205, or about 4.8 percent more than the average Pell award of students with incomes just above the AZ threshold in 2002, to its peak of \$855, or 19.8%, in 2010. The AZ threshold was reduced beginning in 2013, shrinking the discontinuity to a low of \$142, or 2.7 percent, in 2014. The pattern over time for independent students with dependents is similar, with estimated discontinuities of only about \$37 to \$87 (less than 2%) from 2002 to 2006, rising to between \$211 and \$409 (about 4 to 8 percent) between 2007 and 2011, and then falling to nearly zero (about \$4-5, and only marginally statistically different from zero) in 2013 and 2014. The kink due to the income protection allowance has remained more steady over time at about 0.27 to 0.34, though the family income level where it occurs has increased slightly from \$5,110 in 2002 to \$7,000 in 2010. In nearly every year, the estimated discontinuities and kinks are statistically significant despite variation in their magnitude.

In analyses not shown, we investigate the robustness of these results. The estimates are robust to changes in bandwidth, with the point estimates changing very little for bandwidth choices between 50 and 150 percent of the CCT optimal bandwidth (and remaining stable well outside of that interval). For each estimate shown in Table 2 we also conduct the permutation test described in section 3.2. The permutation test fails to reject the null of no discontinuity in Pell eligibility at the one percent level of significance only for independent students with dependents in 2013 and 2014, and for independent students without dependents in 2004 ($p = .052$).

4.2 Pell Eligibility and Degree Completion

Completion is an important student outcome that is strongly correlated with post-college measures of economic well-being including employment and earnings, and successful repayment of student loans (Looney and Yannelis 2015; Scott-Clayton 2018). In Denning et al. (2019), the strong effects of Pell aid on completion contribute to the finding that earnings rise enough to have the program pay for itself. Figure 4 shows our measure of completion rates by \$100 family income bins for the 2010 cohort.[18] For the 2010 cohort, we have data on whether students graduate from the first institution they attend within four years of enrolling and whether they graduate from *any* institution within four years of first enrolling in college (for cohorts up to 2008, we can observe the same two outcomes measured 6 years after entry). All of the completion outcomes trend smoothly through the automatic zero and income protection allowance thresholds, with discontinuity and kink estimates that are small and precisely estimated.

Figure 5 shows results using all years available for completion within four (first column) or six years (second column) for dependent students (top row) and independent students with dependents (bottom row). Each point corresponds to estimates for a particular cohort-year, with the discontinuity in Pell at the AZ threshold in the relevant year plotted on the x-axis, and the discontinuity in the probability of degree completion plotted on the y-axis. The vertical bars indicate the confidence interval around the estimated reduced form impact. The slope of the ray from the origin to each point is thus the IV estimate of a \$1 increase in Pell eligibility on the probability of degree completion, and the figure allows for quick visual inspection of the heterogeneity in estimated effects across years. The figure shows that for dependent students the IV estimates of the effect of Pell eligibility on both four- and six-year completion rates are generally close to zero, but tend to be positive with a couple exceptions (e.g., 2007 for four-year completion). For independent students the increases in Pell at the AZ threshold are much smaller, so IV estimates are noisier. Overall, however, the estimated reduced form impacts tend to be more positive despite less of an increase in Pell, suggesting a larger positive effect on the likelihood of completion.

While Figure 5 displays some variation in the IV estimates of Pell eligibility's effect on degree completion, a chi-squared test fails to reject the null of a constant effect equal to the inverse-variance-weighted average of estimated effects across years.[19] Since we do not reject the null of a constant effect of Pell eligibility, we summarize our analyses by presenting estimates of the average IV estimate of the effect of an additional \$1,000 in Pell eligibility across all years on the degree

[18] Note that in our extract of NSLDS data we know only whether students completed their credential program, but not which credential they received, so our completion measure differs somewhat from typical degree-specific completion rates. Below, however, we confirm that our qualitative results are similar when we split our sample by sectors based on institutions' predominant degree awarded.

[19] For each outcome and student type, we estimate a weighted average of IV effects, call this $\overline{\tau}_a$, across all available years using the inverse of the estimated variance as a weight. The chi-square test statistic is then estimated as the sum of squared terms of the following form for each year: $\frac{\hat{\tau_{at}}-\overline{\tau}_a}{\sqrt{Var(\hat{\tau_{at}})}}$. The p-values for the test for constant effects is shown in the last two rows of Appendix Tables A.10 to A.12.

completion outcomes and for each student type in Table 3.

For dependent students, using data for all years we find an additional $1,000 in Pell eligibility raises the probability of completion within four-years from the original institution by 0.9 percentage points (standard error: 0.4), relative to a base probability of degree completion of 37.2 percent (a relative effect of 2.4 percent). We find roughly the same effect for the six-year completion rate, though noisier and not statistically significant, but when we measure whether students complete at *any* institution the effects are roughly halved and not statistically significant.

As explained above, we conduct sensitivity tests for each of our estimated IV effects to assess whether they are robust to modest changes to the choice of bandwidth. We re-estimate the IV effects using alternative bandwidth choices ranging from 50% to 150% of the CCT bandwidth in increments of 10%. We define an ad hoc binary measure of robustness based on whether the point estimate resulting from an alternate bandwidth choice between 50% and 150% of the CCT bandwidth falls within one standard error of the main coefficient estimate. To illustrate, the results of these sensitivity analyses for the 2010 cohort are shown in Figure A.8. The figure plots the estimated IV effect of $1,000 of Pell at each bandwidth, with vertical dashed lines showing the bandwidths corresponding to 50% and 150% of the CCT bandwidth (indicated with the solid vertical line) and horizontal dashed lines showing a one standard error band around the preferred estimate. The figure shows that for some student types and some outcomes, the estimated effects are sensitive to bandwidth choice.

When we exclude estimates that are not robust to bandwidth choice, our findings are more tentative. In particular, the average estimated effect on completion within six years for dependent students is 0.6 percentage points (2.0), and the effect on completion from any institution within six years is an increase of 1.0 percentage points (1.0)— neither being significantly different from zero. In each case, the point estimates are similar to those using data for all years, but the standard errors become larger and we cannot reject a null of no effect. While tentative, the weight of the evidence suggests that a $1,000 increase in Pell eligibility likely has a small positive effect on the probability of dependent students completing their degree, of up to about 1 percentage point (2.7 percent) measured six years after enrollment, with much of the increase due to an increased likelihood of graduating from the students' initial institution. The full set of results for all cohorts and student types are shown in Appendix Tables A.10 to A.12.

The average estimates for independent students with dependents are noisier but the point estimates suggest larger effects on completion. We estimate impacts on the probability of completion within 4 years at the original institution of 2.1 percentage points (0.9), or 5.7 percent. For completion outcomes at a six year horizon and at any institution, the point estimates are smaller (between 1.0 and 1.6 percentage points) and not statistically significant. Again, our conclusions become more tentative if we ignore estimates that are not robust to bandwidth changes, but the point estimates continue to suggest slightly larger effects of Pell on completion of about 4 to 6 percent. Unfortunately, the RK estimates for independents without dependents shown in the last two columns of Table 3 are too imprecisely estimated to be informative, and most estimates are not robust to begin with.

The degree completion results presented thus far combine completion outcomes across different school sectors (public, private non-profit, private for-profit) and degree types (four-year, two-year, less than two-year). We present estimates of the IV effect of Pell on completion by sector and degree type in Table 4. The effects in Table 4 are estimated on the micro data, so the reported effects in the "All" column are slightly different from the effects reported earlier in Table 3 but are similar both quantitatively and qualitatively.

For dependent students, the effect of Pell eligibility on the probability of completing a degree at their original institution within four years, shows little evidence of heterogeneity by school sector or type. The point estimates by sector and type are similar to the average effect, with the exception of public four-year schools, where we estimate a negative but statistically insignificant effect on completion. Only the estimated effect at public two- to three-year schools rises to the level of statistical significance: an increase of 1.5 percentage points (0.5). As with the results using the collapsed data, when we consider completion at any institution within four years for this group, the estimated effect on completion falls by almost half and loses statistical significance. More broadly, the estimated effect of Pell eligibility on degree completion at *any* institution within four years appears similar across all school sectors and degree types. We also see no differences in the effect of Pell on completion for schools that are in the top quartile of median institutional earnings or schools listed in U.S. News and World Report's Top 200 schools.

Among independent students with dependents, the estimated effect of Pell on completion outcomes shows more variation across sectors and degree types. At for-profit less than two year schools, we estimate that Pell eligibility increases the probability of completing a degree from the original institution or any institution within four years by 12.3 (5.3) and 10.8 (5.4) percentage points, respectively. These estimates are six to seven times larger than the effect estimated across all schools. At the same time, we estimate negative but not statistically significant effects of Pell on completion at public four-year schools. Thus, while we find some evidence that Pell may improve completion outcomes at for-profit less than two year schools, our results are still at odds with those of Denning et al. (2019), who find that Pell significantly increases completion rates at public four-year schools in Texas.

Unfortunately our data are not sufficiently detailed to comprehensively explore effects on many intermediate outcome variables like credits attempted, grades, or work-hours that might provide insight into the mechanisms through which Pell grant aid helps students succeed. One channel we can investigate is whether increases in Pell affects the quality of institutions that students choose to attend, conditional on enrolling in a postsecondary institution. As noted by Carruthers and Welch (2020), greater Pell eligibility could lead students to shift to higher quality colleges by reducing their net price, or could instead lead students to lower quality, less expensive, colleges— especially community colleges— due to greater reductions in their relative prices.

We investigate impacts on school choices using three proxies for school quality: whether the school attended is a four year school (versus a school where the highest degree offered is a 2 year or less degree), the institution's average tuition and fees, and the median earnings of students who enrolled in the 2001 or 2002 award years measured 10 years later in 2011 or 2012 taken from the College Scorecard.[20] Table A.4 presents the instrumental variable estimates of the effect of an additional $1,000 of Pell eligibility on school choice outcomes, along with the average baseline of each outcome for students with family income just above the thresholds. Figure A.6 presents the reduced form relationship between school choice and family income for the 2010 cohort.[21]

For dependent students, we find that increased Pell eligibility results in very little change in the type of institutions that students attend. While the estimated IV impacts are negative and statistically significant for each of the three outcomes, they are small in magnitude. We estimate a $1,000 increase in Pell results in a 0.55 percentage point decline (standard error of .24) in the

[20]The measure of earnings is based on federal student aid recipients only.

[21]IV and reduced form estimates for all cohorts with available data are shown in A.7 to A.9.

probability of enrolling in a four-year school, relative to a base probability of 53 percent— a relative effect of about 1 percent. We estimate increases in Pell eligibility reduce the average tuition and fees of the institutions students attend by about $132 (41.3), or about 1.4 percent, and reduce average institution earnings by $139 (49.2), or 0.4 percent.[22] The pattern of point estimates for independent students with dependents is very similar. Larger standard errors mean that only the average effect on four-year school attendance is statistically significant, although this estimate loses significance when we drop nonrobust estimates. For independent students without dependents, the point estimates from the regression kink design are all not significantly different from zero, and the standard errors rule out estimates larger than about a 3-4 percent change relative to the mean. Overall, we conclude that Pell grants have little effect on the schools students decide to attend, and thus shifting composition of enrollment across institutions is unlikely to be an important channel through which Pell impacts the completion results above.[23]

4.3 Pell and Employment Outcomes

Although we find very limited effects of Pell eligibility on degree completion, we estimate the impacts on earnings outcomes for completeness, given their primary importance in evaluating the returns to this public investment. It may be that Pell aid leads to increases in human capital not reflected in degree attainment— e.g., by affecting the amount of learning students achieve in classes— and thus improves labor market outcomes even without impacts on completion. The total budgetary effects of the Pell Grant program ultimately depend on the future tax revenues that the program induces through its effect on post-college earnings.

We have data on employment and earnings outcomes through 2012. To observe cohorts in years when they are likely to have completed any post-secondary education and training, we limit our examination of labor market outcomes to students in the 2002 through 2006 cohorts where we can

[22] While these discontinuities in school choice outcomes are consistent with the intuition that the small discontinuity in enrollment at the AZ threshold could result in some negative selection into the sample with higher Pell eligibility, we note that evidence from Matsudaira (2017) suggests the pattern of enrollment discontinuities by sector cannot fully explain the decrease in four-year school enrollment at the AZ threshold.

[23] Our findings are further bolstered by the fact that many students—roughly 60 percent in our data—only consider one school when they apply for financial aid, implying there is little scope for financial aid to affect students' choice of school, as other work has confirmed (Smith et al. 2013). Even limiting our sample to students in the 2009 and 2010 cohorts who request their FAFSA be sent to at least two schools in Table A.5, we find very few differences relative to our results for the full sample of students, and exploring heterogeneity by borrower status, sex, and first-generation status (i.e., whether a students' mom attended college) we find no evidence of significant differences in effects for any subgroups (see Appendix Table A.6).

observe their earnings at least six years after they first enroll. In Figure 6 we display log earnings for the 2003 cohort in 2010 and 2012.[24] Most of the discontinuities and kinks are not statistically significant although some imply large decreases in earnings.

Figure 7 shows estimated effects on earnings in 2010 (first column) and earnings in 2012 (second column) for dependent students (first row) and independent students with dependents (second row) from all applicable cohorts. For dependent students, more negative effects on earnings in both 2010 and 2012 tend to be observed in the years with the largest discontinuities in Pell. However, for both dependent students and independent students with dependents the effects are evenly spread across positive and negative estimated effects, suggesting that Pell eligibility may not have a strong effect on earnings. In Appendix tables A.13 to A.15, we present the estimates for log earnings from all years.[25]

We again summarize our results by presenting weighted averages of the IV estimates across years in Table 5. As the figures suggest, for dependent students, we find that a $1,000 increase in Pell eligibility decreases earnings by 12.3 percent (7.0) in 2010 and 4 percent (6.4) in 2012. While these point estimates are large, when we exclude estimates sensitive to bandwidth, the estimates are no longer significant and become either less negative or flip to positive. For independent students with dependents, we find very large effects of Pell on earnings— around a 27 percent increase in both years— but the estimates are never statistically significant, in part because of the small first-stage effect of the AZ threshold on Pell eligibility. The estimated effects for independent students without dependents are both not statistically significant and not robust to bandwidth.

Overall, our estimates for employment and earnings are too noisy to make firm conclusions about whether Pell eligibility affects employment outcomes either positively or negatively. With data covering more recent cohorts exposed to the larger discontinuities in Pell eligibility between 2008 and 2012, similar to the years spanned by the data in Denning et al. (2019), more precise conclusions might be possible.

[24] To create our measure of log earnings, we first top code earnings at $1 million, then we add one before taking the natural log of earnings.

[25] In these tables, we also include estimates for the effect of Pell on whether students had positive earnings in 2010 and 2012— a measure of employment at some point during the year. In general, the estimated effects on whether students had positive earnings are not significant, vary widely across years, and are often not robust to the choice of bandwidth, so we focus only on the results on log earnings in the main discussion of our results.

5 Discussion

In our analyses above, we find evidence of a small but positive effect on the probability of completing a degree conditional on enrolling in college, of about 2 to 3 percent for dependent students and about twice that for independent students with dependents. These estimated impacts are substantially smaller than those found in the recent work of Denning et al. (2019) using the same research design with data on students attending public colleges in Texas between 2008 and 2011. They find students receiving an additional $1,000 in grant aid (in 2013 dollars) are about 5 percentage points (12.8 percent) more likely to graduate within 6 years. To facilitate direct comparison with our estimates, based on their published tables and our analyses of reported aid amounts in the NPSAS in Appendix Table A.19, we estimate this is equivalent to about a 4.95 percentage point effect per $1,000 in Pell eligibility (in real 2012 dollars).[26] This section explores potential reasons for the discrepancy, and situates our findings in the context of the academic literature and public policy considerations.

Ideally we could address the discrepancy directly and simply attempt replication of the results in Denning et al. (2019) and other recent studies, since these state-specific studies are essentially subsets of the data used here.[27] Unfortunately we lost access to the micro-data that would allow this, and accordingly cannot assess whether the discrepancies stem from simple (and slight) differences in methodology such as different groupings of students into cohorts, differences in the characteristics of the underlying cohorts, or averaging effects across different cohort years.[28]

These technical differences aside, there are good reasons to suspect that 'the effect' of Pell is

[26]We convert their published estimates as follows. From their Table 2, we get that a $1,000 change in total grant aid at the AZ threshold is associated with a $749 increase in Pell grant aid (all in 2013 dollars). From our analyses of the NPSAS, we estimates the change in Pell grant aid for 2008 and 2012 at the AZ threshold (2008 results not shown), and from Table 2, we have estimates of the discontinuities in Pell eligibility in the same years. Taking the average of these ratios for the two years, we calculate that a $749 change in Pell grant aid would result from a $1,004 change in Pell eligibility. Adjusting for inflation from 2013 to 2012, we estimate that $1,000 (in 2013 dollars) in total grant aid in Denning et al. (2019) corresponds to a $989 (2012 dollars) change in Pell eligibility.

[27]This may not be fully accurate, as our data are comprised only of students who receive Title IV student aid. While other studies might include data for students who don't receive federal aid, in practice since most recent quasi-experimental studies rely on students' financial information (either EFC or family income) that is most likely derived from the FAFSA, the overlap is likely substantial.

[28]Attempts to adjust our estimated effects to account for the small discontinuities in enrollment that we find also cannot explain the differences between the completion estimates. Our average estimated discontinuity in enrollment across years is about 4%. If all of the students we do not observe to the right of the AZ threshold, relative to the left side, would have had a 0% completion rate, this would lower the completion rate to the right side of the threshold from .39 to .375. Using our largest six-year completion effect estimate (a 0.010 increase in completion at any institution), our resulting discontinuity in completion rates would be .025— still about half of the Denning et al. (2019) estimate.

in fact heterogeneous across both states and institutions. In other words, there is no 'the effect'. If so, it may be that the effect of Pell measured by Denning et al. (2019) is not representative of the average effect across all institutions of an increase in Pell eligibility due to the discontinuity in the EFC formula. Why might the impact of Pell be heterogenous across states and different institutions? One possibility raised by previous research is that differences in Pell grant aid across students are correlated with differences in grants that students receive from other sources, especially states and institutions (Turner 1998; Turner 2017). That is, consider a reduced form relationship between total grant aid and Pell eligibility as

$$T_i = \lambda_0 + \lambda P_i + u_i,$$

where T_i represents total grant aid (and thus the reduction in net price), and P_i represents Pell eligibility. The term λ captures how other forms of grant aid are related to increases in Pell grant eligibility, and could be greater or less than one. Since Pell is administered through institutions that observe a student's Pell eligibility before making decisions about how much grant aid to package for a student, institutions might reduce their own aid to a student as her Pell eligibility increases. Indeed, federal law may encourage this crowd-out of federal aid to some extent by requiring that students not receive more aid than their 'need' (i.e., the institution's cost of attendance minus a student's EFC), and more generally institutions may want to avoid large differences in aid being awarded to students with similar financial circumstances. On the other hand, Turner (2017) finds that in some instances Pell might crowd-in institutional aid, for example if institutions place extra value on enrolling Pell eligible students.

In practice, the extent of crowd-out or crowd-in is likely to vary considerably across institutions due to differences in both institutional priorities and resources and so the impact of Pell on total grant aid and thus student outcomes is also likely to differ. In particular, private four-year universities have much more institutional aid to offer, and so the scope for crowd out is much greater in that sector. According to data from the NPSAS, average institutional grant aid per dependent student at private four-year institutions was \$11,179 in 2012 (see Table A.18). By contrast, the same figure was \$1,395 at public four-year schools and \$296 at public community colleges suggesting less scope for institutions to counteract the increase in Pell eligibility in the public sector. Denning et al. (2019) report that Pell aid has no impact on institutional grants in their sample of public college students in Texas.

While we cannot observe institutional aid in our data, we address the possibility that Pell grants appear relatively ineffective in our study due to crowding-out institutional aid in two ways. First, we use data from the National Postsecondary Student Aid Study (NPSAS) to examine differences in institutional grant aid and other financial aid components around the AZ threshold. We also use a simplified version of our IV strategy to estimate λ. As shown in results in Appendix Figure A.10 and Table A.19, institutional aid appears to decrease for students who qualify for the maximum Pell award, but our estimates of λ across sectors are too imprecise to be informative.

Another source of heterogeneity in λ that may be more important in this context is state financial aid programs. Denning et al. (2019) find that students just eligible for the maximum Pell award receive an extra \$151 in state grant aid. This large increase accounts for nearly a quarter of the overall increase in total grant aid in their setting, and causes the total increase in grant aid to be about one-third larger than the increase in average Pell awards. Moreover, being awarded this state grant at college entry effectively guarantees full college tuition for four years at a public university, suggesting a sustained difference in aid for students around the AZ family income threshold. This is in contrast to Pell grant aid, which is likely to be similar for students on either side of the AZ family income threshold in subsequent years since Pell eligibility is recalculated each year and both the AZ threshold and incomes change from year to year.

To assess how state aid programs might interact with the Pell eligibility rules studied here, we reviewed the state financial aid eligibility formulae for the twenty-four states with the most generous need-based grant programs or largest undergraduate enrollment. These states account for about 80% of undergraduate enrollment (National Center for Education Statistics 2018), 94% of all need-based grant aid for undergraduates (National Association of State Student Grant & Aid Programs 2018), and 70% of Pell Grant funds (U.S. Department of Education 2018). We attempted to simulate award amounts as a function of family income for the largest need-based grant programs in each state based on publicly available information. For five of the states— Arizona, Colorado, Tennessee, Texas, and Virginia—the specific students who receive aid and how much aid they receive is generally determined at the institution level so we could not determine from review of the rules alone how these states' grant programs might interact with Pell eligibility. Georgia did not have a need-based grant program, and New Jersey and North Carolina did not publish enough information on their formulae for us to determine how grant amounts relate to Pell eligibility. For the remaining sixteen states, we simulated the combined Pell and state grant schedule that an example student would face in the 2018-2019 academic year.[29]

For the sake of illustration, in Appendix Figures A.11 and A.12, we graph modified versions of these schedules for dependent students assuming an AZ threshold at $35,000–high enough to create a substantial discontinuity in Pell eligibility.[30] For each state, we show the discontinuity in Pell eligibility ("Pell Disc." in the figure) and the discontinuity in combined state and federal grant aid ("Grant Disc.").[31] The discontinuity in combined grant aid is the same as the discontinuity in Pell for twelve of the 16 states. It is smaller in one state, indicating that Pell eligibility crowds *out* state aid there, and for the remaining three, the grant schedule implies that, as with Texas, a discontinuity in Pell eligibility results in a larger discontinuity in combined state and federal grant aid. More details of these simulations can be found in Appendix B.

While this exercise suggests that the effect of the automatic zero threshold on total grant aid is likely smaller at the national level than it is for students in Texas, this difference cannot fully reconcile our results with the results of Denning et al. (2019). In the Denning et al. (2019) setting, automatic zero eligibility increases total aid by about $150 more than the increase in Pell eligibility, or 24 percent more than the increase in Pell.[32] Their estimate of the effect of grant aid on six-year completion, though, is more than six times as large as our estimate. The effects of grant aid on degree completion would need to be extremely non-linear for the discrepancies in our results to be simply due to differences in the effect of AZ eligibility on total grant aid.

Another important way that the Texas state grant program differs from many other state programs is in how it affects students' aid over the course of their postsecondary education. Denning et al. (2019) note that a first year TEXAS grant guarantees coverage of tuition and fees for all four years of college as long as a student makes satisfactory academic progress and continues to demon-

[29]We chose 2018-2019 since for many state grant programs information was only readily available for the most recent academic years.

[30]In the true EFC formula for 2018-2019, the AZ threshold is at $25,000, where EFC would generally be zero even under the full EFC formula, so it does not create a discontinuity in Pell eligibility. We impose a higher AZ threshold to illustrate whether and how state aid is affected by the discontinuity in EFC.

[31]The discontinuity amounts vary by state since the EFC formula includes a state tax allowance based on the level of state taxes, resulting in larger discontinuities for low tax states like Florida and Washington and smaller discontinuities for high tax states like New York.

[32]This uses the average discontinuity for dependent students measured in our data for 2008 to 2011.

strate financial need. This guarantee of full tuition coverage arises because the state mandates that when institutions award a student a TEXAS grant, they assure that the student receives sufficient aid to meet tuition and fees, whether through the TEXAS grant itself, Pell grants, or additional institutional aid, but not in the form of loans. Of the other state grant programs we reviewed, very few provide a similar guarantee that grant aid will be at least the amount of tuition and fees. Two exceptions are California's Cal Grant program, which covers Cal State or University of California systemwide fees for students who attend these institutions, and Virginia's Guaranteed Assistance Program, which must cover at least tuition for students with the highest demonstrated need. Most other states set maximum award amounts at the state level, often based on an average of tuition and fees in previous years and Pell awards. Thus, by design these states do not guarantee that awardees will receive grant aid to pay full tuition and fees. While this difference between the Texas grant program and other state programs is hard to quantify, it seems reasonable, as Denning et al. (2019) speculate, that a guarantee of financial aid over a longer horizon could have a larger effect on student success than more variable aid.

6 Conclusion

Our results above suggest that the increase in Pell grants triggered when students qualify for the automatic zero EFC results in a small increase in the likelihood of degree completion, but the results vary and are often statistically insignificant in particular years, and on average are several times smaller than the effect found in the influential study by Denning et al. (2019). We provide some evidence that the more positive impacts of Pell in their analysis may be partly driven by how Pell interacts with features of Texas's financial aid program that are not representative of the nation.

While the estimates presented here are more representative of the increase in Pell grants caused by the quasi-experimental variation in Pell grants produced by the AZ and IPA thresholds we study, they should not be taken as an evaluation of the impacts of increasing Pell aid generally. In the same way that the increases in Pell driven by the automatic zero EFC policy have different impacts on total grant aid and other key causal mechanisms across different states and types of institutions, different policies aimed at increasing Pell may also be expected to have a different effect than we document here. In this sense studies seeking to estimate 'the' effect of increases in Pell (or other types of grant aid) may be missing the point. As John DiNardo put it, "although we can learn about the effect of an intervention in a well-designed study, we aren't guaranteed to learn about *the* putative cause in question, because the cause under consideration may be inextricably implementation specific" (DiNardo 2007). The most readily available and commonly used policy lever for increasing Pell aid is Congress' ability to set maximum benefit levels. Increasing Pell grants via the maximum benefit is likely to interact with state and institution aid in different ways, and affect much broader groups of students than is true in any of the individual studies that have estimated impacts of Pell on students. While this study and other studies based on non-linearities in the Pell eligibility formula move the literature forward by providing internally valid estimates of Pell increases in particular contexts, the literature still has far to go in synthesizing such results to provide information that would be useful for *ex ante* evaluations of policy attempts to increase Pell.

That said, due to a lack of detailed data, a major limitation of our study is our inability to shed light on the mechanisms behind financial aid's impact on student success. As researchers have gained access to better data allowing more credible estimates of the causal effect of financial aid programs, it is becoming clearer that providing grants to students *can* increase college access and success. Some programs still appear more effective than others, however, and too little is known about why. Future research should endeavor to better understand how various grant programs affect intermediate outcomes like other aid sources and net prices; but also students' perceptions of the cost of college; their cash-on-hand, which will depend on borrowing and work behavior; their sense of their own ability or odds of success in college, which might be enhanced if the grant is presented as a reward for merit; and institutional resources and education quality. With a better grasp on how these mechanisms are impacted, differences in the estimated impacts of different grant programs may be more readily understood, and future programs can be better designed with these lessons in mind.

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7 Figures and Tables

Figure 1:
Pell Grant Amount for Hypothetical Students by Family Income and Aid Type, 2010

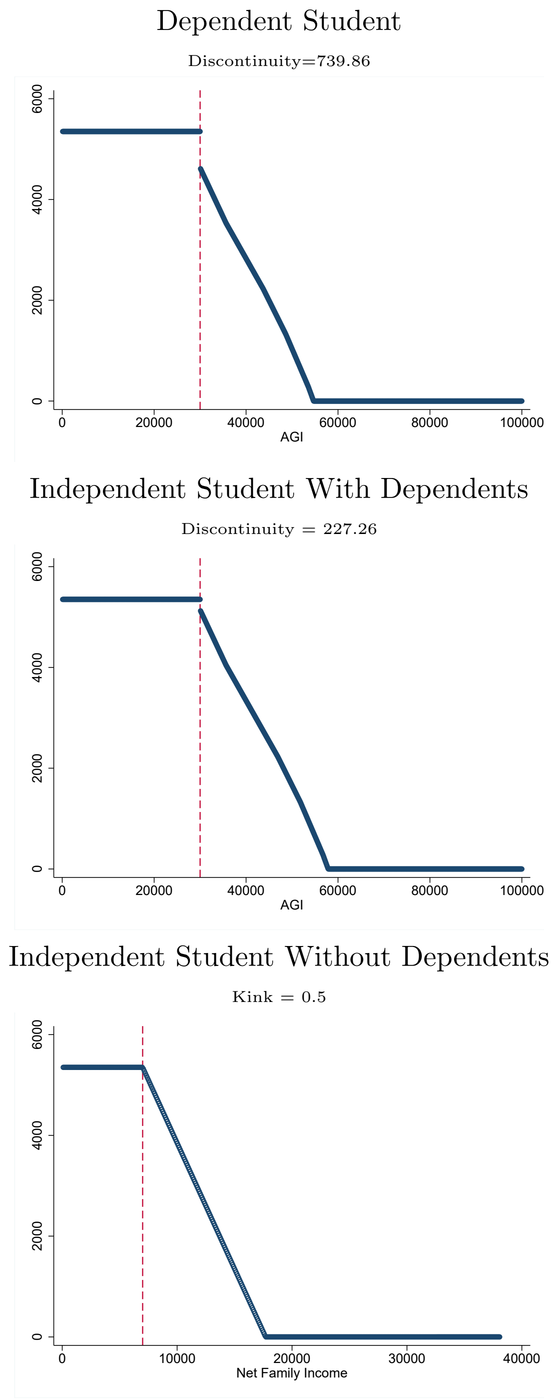
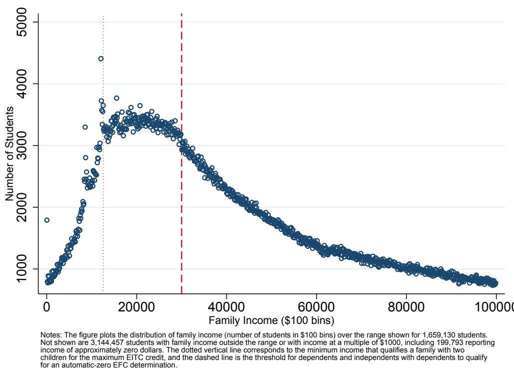


Figure displays the Pell grant schedules of three example students. The example dependent student is a seventeen year old student from a single parent household in Michigan with one younger sibling not in college. The example dependent student's parent has no assets, only has income from wages, and does not itemize deductions. The example independent student with dependents is a single parent in Michigan with two children under the age of 17, with no assets, income from wages only, and no itemized deductions. The independent student without dependents is a single person from Michigan, with no assets, income from wages only, and no itemized deductions. For the dependent student and independent student with dependents, dashed lines indicate the auto-zero EFC threshold of $30,000 in 2010. For the independent student without dependents, the x-axis is adjusted gross income less taxes paid, and the dashed line indicates the income protection allowance of $7,000 in 2010.

Figure 2:
Density of Family Income by Aid Type, 2010

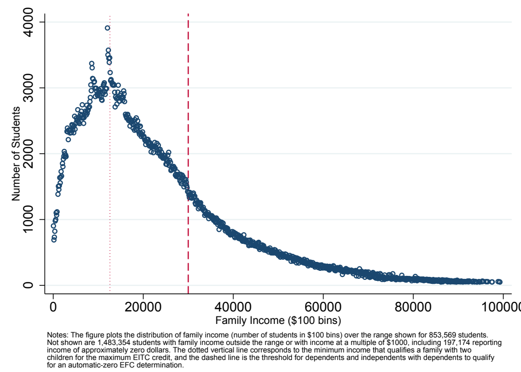
Dependent Students

Discontinuity=0.0499 (0.0069)



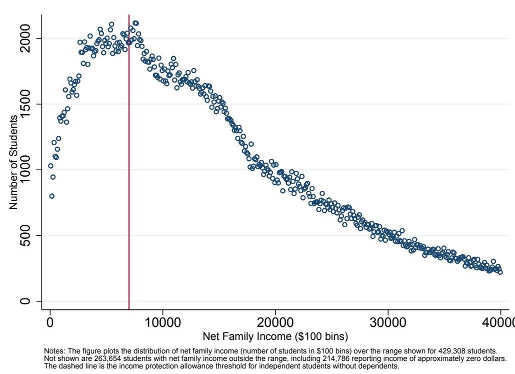
Independent Students With Dependents

Discontinuity = 0.0505 (0.0116)



Independent Students Without Dependents

Discontinuity = 0.0076 (0.0187)

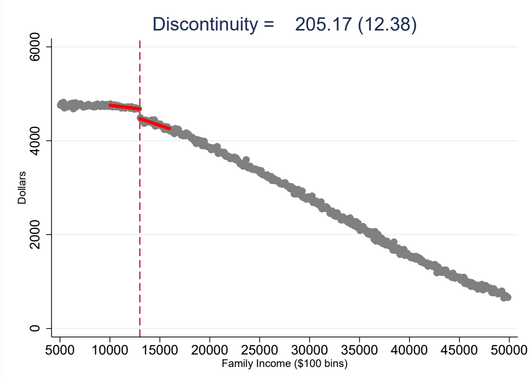


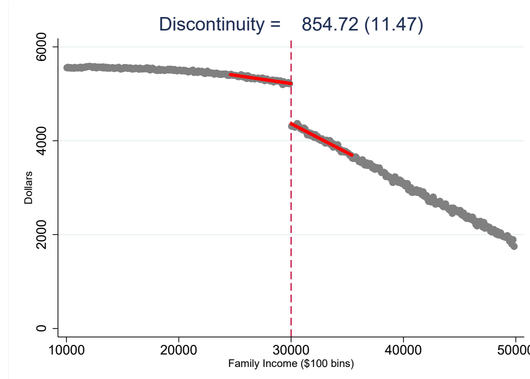
Figures display the number of students in each $100 income group. For dependent students and independent students with dependents, the relevant income type is family income, which is usually equivalent to adjusted gross income. Figures for dependent students and independent students with dependents exclude income bins that include a multiple of $1,000. For independent students without dependents, the relevant income type is family income less an allowance for federal income taxes, state taxes, and Social Security taxes. For dependent students and independent students with dependents, the dashed lines mark the automatic zero threshold while the dashed line for independent students without dependents is the income protection allowance threshold. The dotted lines in the figures for dependent students and independent students with dependents mark the income level corresponding to the first kink point in the Earned Income Tax Credit schedule for families with two children.

Figure 3:
Average Simulated Pell Grant by Family Income and Aid Type, 2003 and 2010

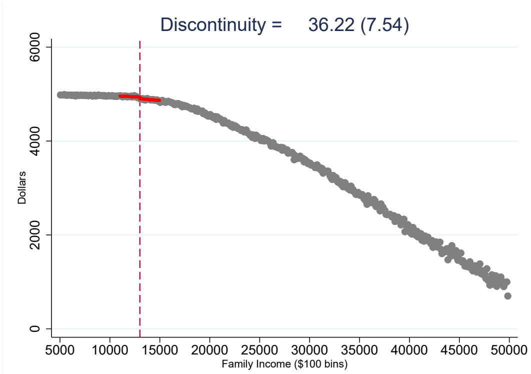
2003 | 2010

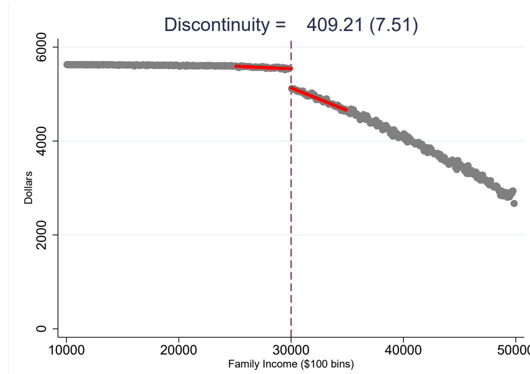
Dependent Students



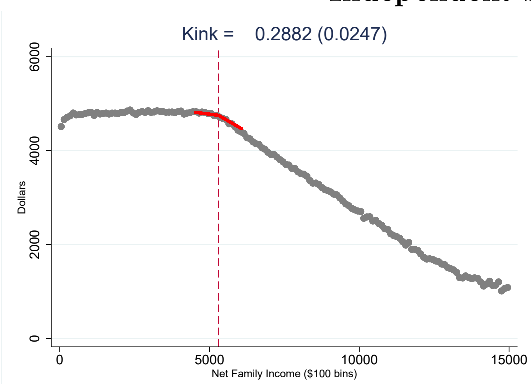


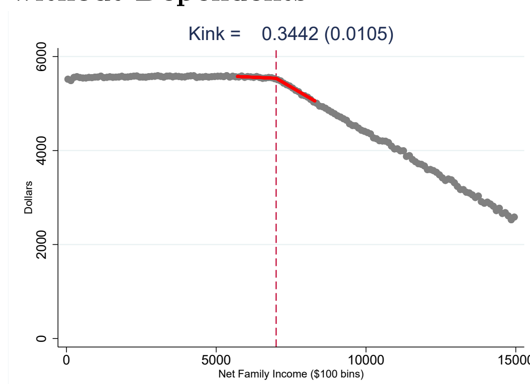
Independent Students with Dependents





Independent Students without Dependents

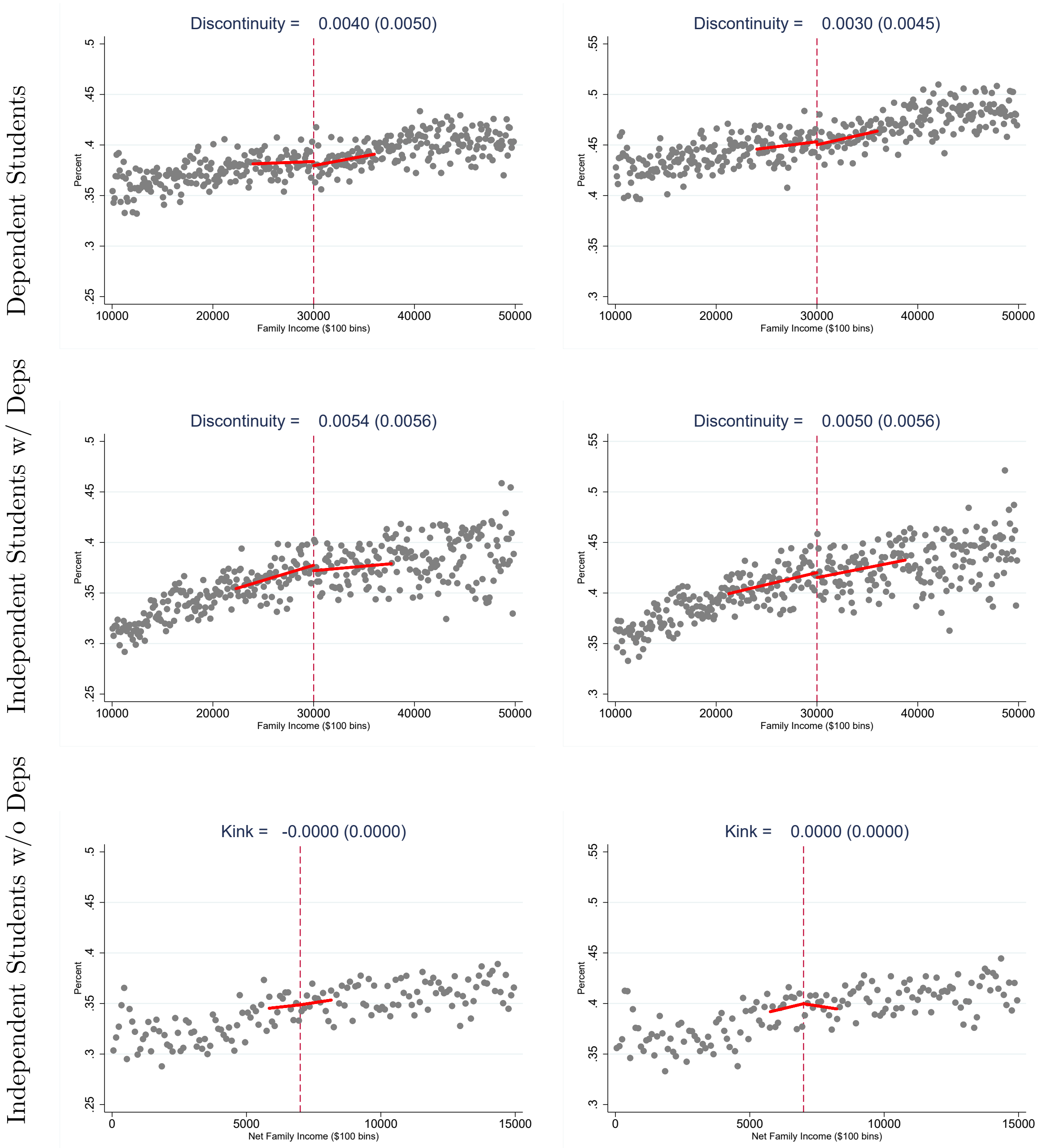




Figures display the average simulated Pell grant of students in each $100 bin of family income (dependent students or independent students with dependents) or net income (independent students without dependents). Pell grant amounts are in 2012 dollars. Family income is usually equivalent to adjusted gross income. Net family income is usually adjusted gross income less an allowance for federal income taxes, state taxes, and Social Security taxes. For dependent students and independent students with dependents, dashed lines indicate the auto-zero EFC threshold for the given year. The auto-zero threshold was $13,000 in 2003 and $30,000 in 2010. For independent students without dependents, dashed lines indicate the income protection allowance for the given year. The income protection allowance was $5,300 in 2003 and $7,000 in 2010.

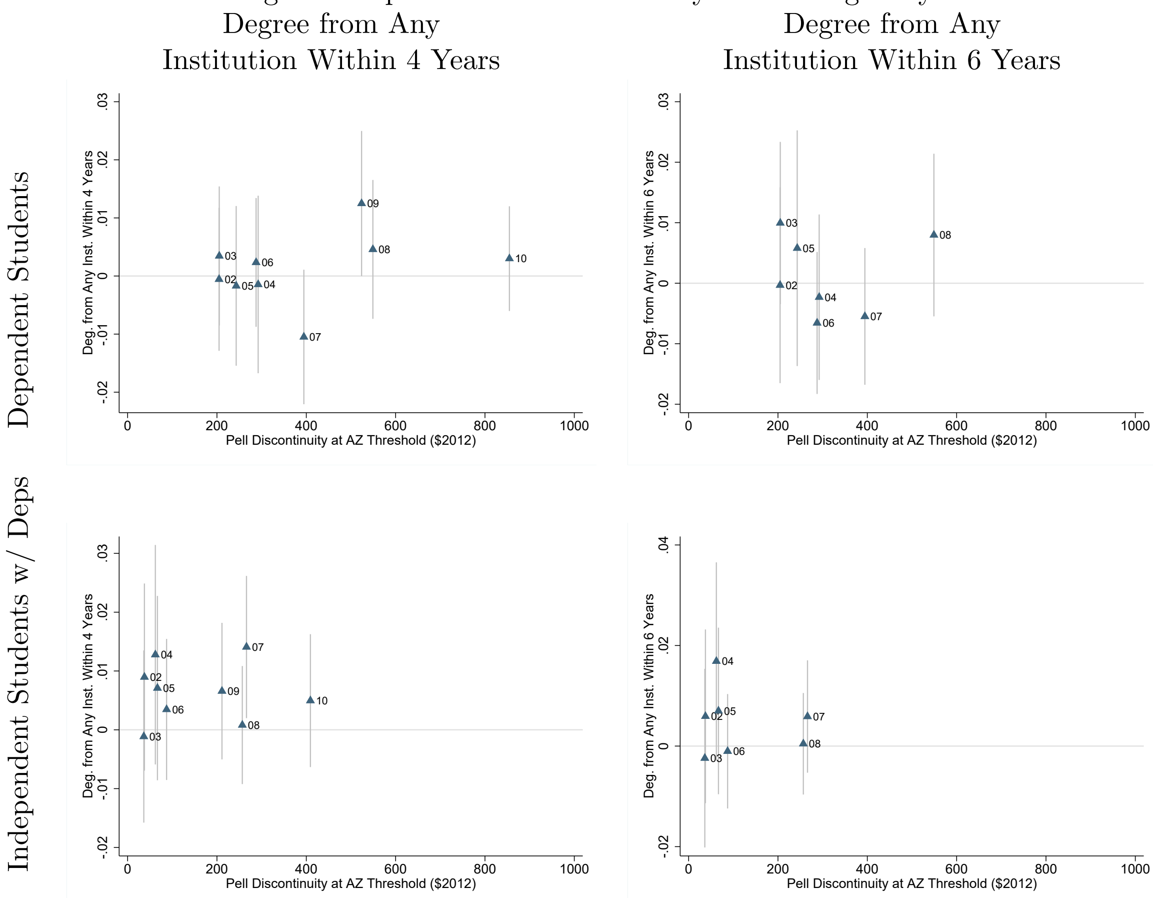
Figure 4:
Degree Completion Outcomes, 2010

Degree from Original Institution Within 4 Years | Degree from Any Institution Within 4 Years



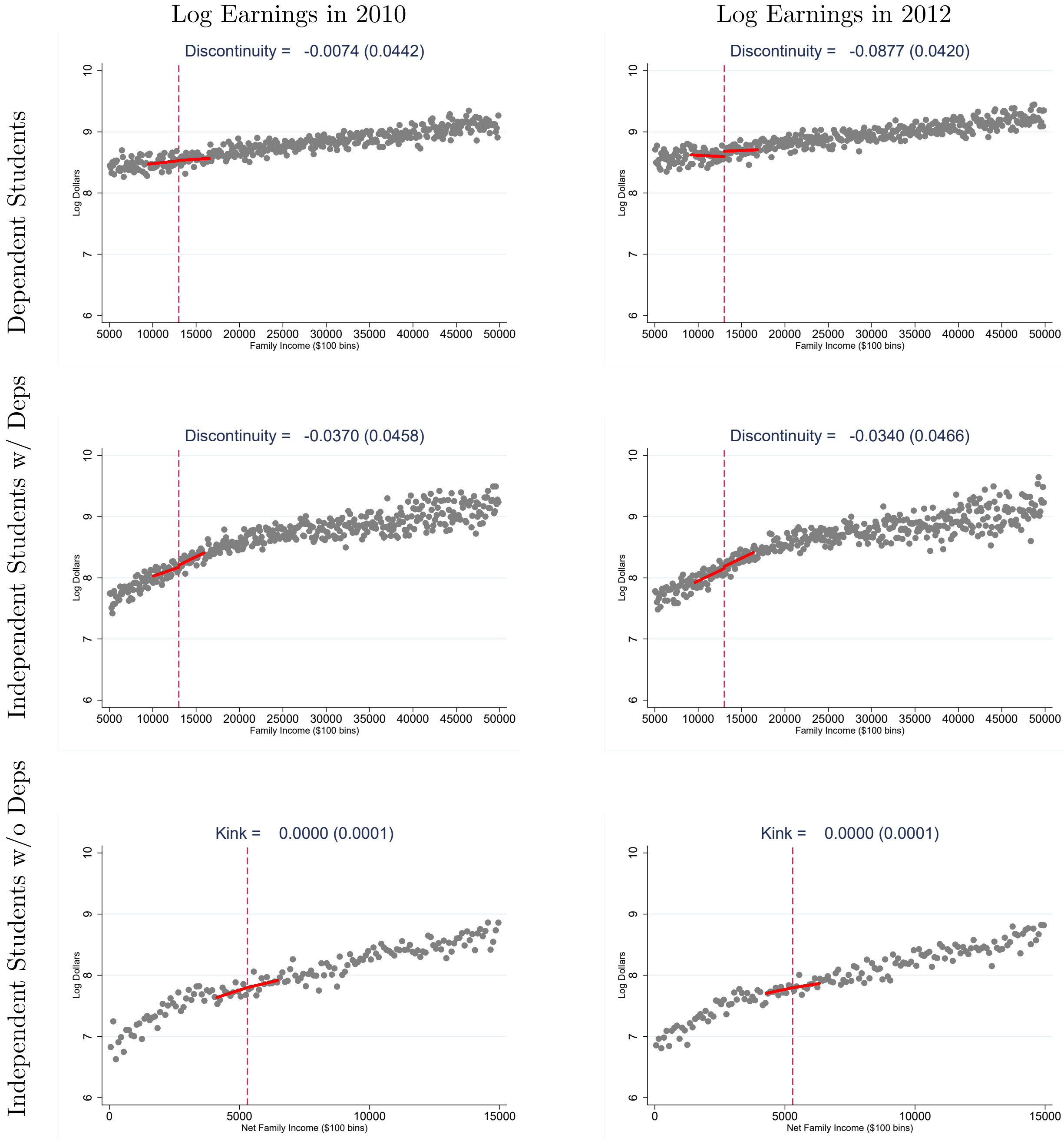
Figures display the average of the outcome variable for students with student loans in each $100 family income or net income bin. Figures for dependent students and independent students with dependents exclude income bins that include a multiple of $1,000. Dashed lines for dependent students and independent students with dependents indicate the $30,000 auto-zero EFC threshold for 2010. Dashed lines for independent students without dependents indicate the $7,000 IPA threshold for 2010.

Figure 5:
Degree Completion Outcomes, 2002-2010 Cohorts: Relationship Between Reduced Form Effect on Degree Completion and Discontinuity in Pell Eligibility



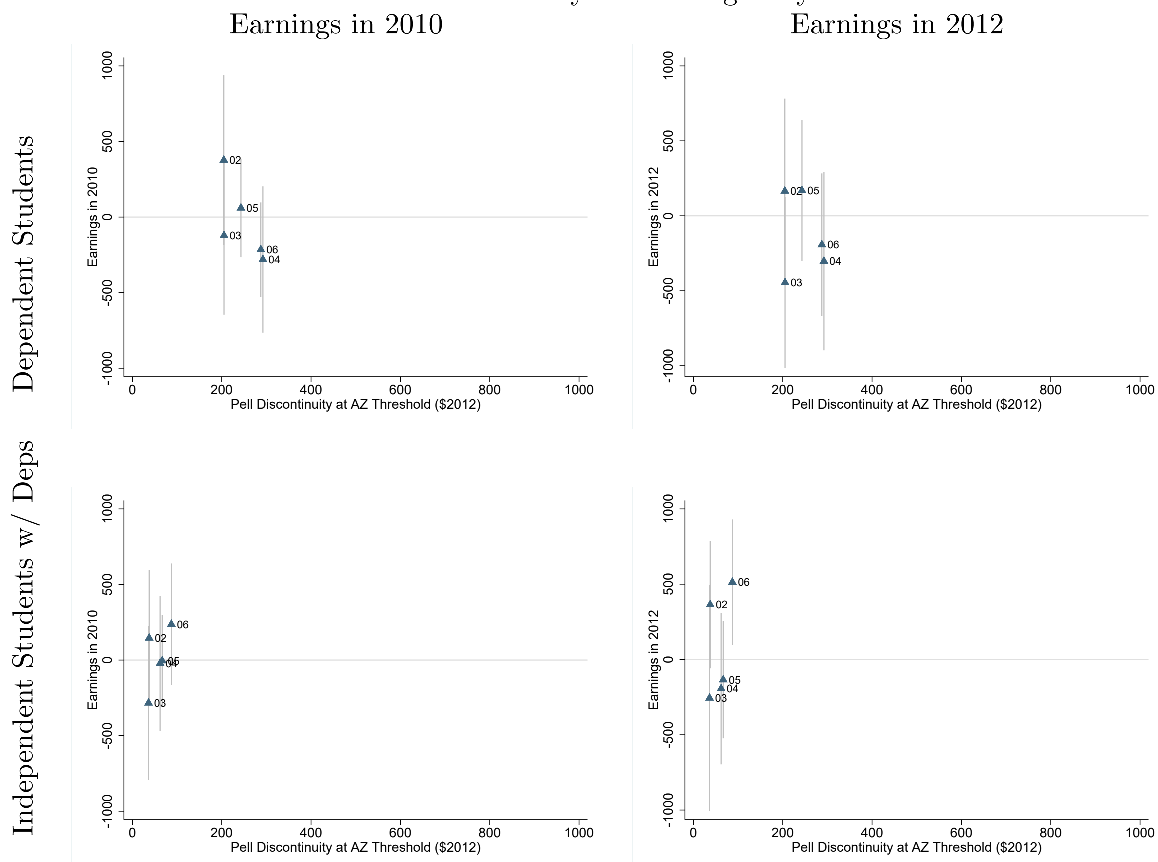
Figures display the reduced form estimate of the discontinuity in the probability of completing a degree at the automatic zero threshold for each cohort from 2002-2010 versus the estimated discontinuity in Pell at the same threshold. Each point is labeled with its corresponding cohort. Pell discontinuities are in 2012 dollars.

Figure 6:
Employment Outcomes, 2003 Cohort



Figures display the average of the outcome variable for students in each $100 family income or net income bin. Figures for dependent students and independent students with dependents exclude income bins that include a multiple of $1,000. Dashed lines for dependent students and independent students with dependents indicate the $13,000 auto-zero EFC threshold for 2003. Dashed lines for independent students without dependents indicate the $5,300 income protection allowance threshold for 2003.

Figure 7:
Earnings Outcomes, 2002-2006 Cohorts: Relationship Between Reduced Form Effect on Earnings and Discontinuity in Pell Eligibility



Figures display the reduced form estimate of the discontinuity in earnings at the automatic zero threshold for each cohort from 2002-2006 versus the estimated discontinuity in Pell at the same threshold. Each point is labeled with its corresponding cohort. Pell discontinuities are in 2012 dollars.

Table 1: Descriptive Statistics for Select Variables and Years

	Dependent Students			Indep. Students w/ Deps			Indep. Students w/o Deps		
	All	$1,000 Below Thresh.	$1,000 Above Thresh.	All	$1,000 Below Thresh.	$1,000 Above Thresh.	All	$1,000 Below Thresh.	$1,000 Above Thresh.
2003									
Student Characteristics									
Age	19.59	19.70	19.72	30.46	30.00	29.99	29.03	27.86	27.66
Female	56.34	58.78	59.57	81.32	84.41	85.57	54.46	53.58	53.35
Mom College Graduate	38.05	23.49	23.18	21.29	21.35	20.83	27.57	28.48	29.24
Characteristics of Students' ZIP Codes									
Percent White	75.29	67.38	66.71	68.34	67.67	67.69	71.20	71.93	71.86
Percent U.S. Born	8.18	7.83	7.78	8.16	8.13	8.18	8.01	8.05	8.04
Median Household Income in 1999	44,237	38,371	38,233	39,124	37,981	37,861	42,205	41,429	41,741
Percent of Families Below Poverty	9.67	13.22	13.59	12.01	12.67	12.69	10.62	10.89	10.80
Unemployment Rate	3.71	4.37	4.43	4.21	4.31	4.28	3.99	4.04	4.01
Observations	1,440,083	17,634	17,449	685,371	16,643	16,960	449,499	17,154	16,945
2010									
Student Characteristics									
Age	19.63	19.62	19.63	30.72	33.53	33.43	30.74	29.69	29.39
Female	55.23	56.30	55.92	77.12	70.57	71.03	50.67	51.70	51.27
Mom College Graduate	40.28	34.54	33.35	23.60	22.49	23.30	25.37	26.37	27.23
Observations	2,428,537	26,790	28,476	1,181,638	12,279	13,774	692,962	20,292	19,725

Notes: Entries show means of each variable for the subgroup indicated in the column. The threshold for dependent students and independent students with dependents is the automatic zero threshold, which is $13,000 of family income in 2003 and $30,000 of family income in 2010. The threshold for independent students without dependents is the income protection allowance, which is $5,300 and $7,000 of family income less taxes paid for the 2003 and 2010 cohorts, respectively. For dependent students and independent students with dependents, observations with family income at a multiple of $1,000 are excluded for the samples near the threshold.

Table 2: Estimated Discontinuity in Pell Grant Eligibility at AZ Threshold and Kink at IPA Threshold, 2002-2014

					Dependent Students		Indep. Students w/ Deps		Indep. Students w/o Deps	
Year	AZ Thresh. (Nom. $)	IPA Thresh. (Nom. $)	Max. Pell (Nom. $)	Max. Pell (2012$)	Mean Pell Near Thresh. (2012$)	Discontinuity (2012$)	Mean Pell Near Thresh (2012$)	Discontinuity (2012$)	Mean Pell Near Thresh (2012$)	Kink
2002	13,000	5,110	3,750	4,786	4,263.02	204.71***	4,698.00	37.64***	4,340.69	0.3123***
						(11.33)		(5.53)		(0.0287)
2003	13,000	5,300	4,000	4,991	4,430.41	205.17***	4,893.90	36.22***	4,517.21	0.2882***
						(12.38)		(7.54)		(0.0247)
2004	15,000	5,400	4,050	4,922	4,253.21	292.29***	4,780.81	62.09***	4,443.75	0.2659***
						(15.32)		(6.56)		(0.0309)
2005	15,000	5,490	4,050	4,761	4,146.95	243.10***	4,621.44	66.79***	—	—
						(12.31)		(7.15)		
2006	15,000	5,560	4,050	4,612	3,961.26	287.61***	4,454.20	87.27***	—	—
						(10.23)		(6.27)		
2007	20,000	5,790	4,050	4,485	3,499.32	394.57***	3,992.56	266.14***	—	—
						(12.60)		(9.68)		
2008	20,000	6,050	4,310	4,596	3,733.68	549.10***	4,232.65	256.80***	—	—
						(10.41)		(5.01)		
2009	20,000	6,220	4,731	5,063	4,249.16	523.76***	4,753.53	211.26***	4,541.76	0.2956***
						(9.23)		(5.19)		(0.0200)
2010	30,000	7,000	5,350	5,633	4,308.51	854.72***	5,083.62	409.21***	5,340.93	0.3442***
						(11.47)		(7.51)		(0.0105)
2011	30,000	7,780	5,550	5,665	4,584.26	607.56***	5,351.22	199.56***	—	—
						(10.25)		(5.30)		
2012	31,000	8,550	5,550	5,550	4,395.96	680.90***	5,314.48	144.29***	—	—
						(10.14)		(7.76)		
2013	23,000	9,330	5,550	5,470	5,156.76	146.99***	5,441.65	4.92	—	—
						(7.91)		(3.15)		
2014	24,000	9,540	5,645	5,475	5,179.36	141.85***	5,450.08	4.43	—	—
						(7.80)		(3.25)		

Notes: Discontinuities and kinks estimated with local linear regressions and uniform kernel on data collapsed into $100 income bins. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Bandwidths range from 2,078.46 in 2014 to 5,492.44 in 2009 for dependent students; 1,808.80 in 2006 to 4,917.61 in 2010 for independent students with dependents; and 772.46 in 2003 to 1,294.95 in 2010 for independent students without dependents. Estimates for dependent students and independent students with dependents exclude observations with family income at a multiple of $1,000. Conventional robust standard errors are in parentheses. The mean Pell grant near the threshold is the mean for students with nominal family income between $1 and $1,000 above the threshold. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table 3: Average IV Estimate of Effect of $1000 of Pell Eligibility on Degree Completion Outcomes

Dependent Var.	Dependent Students: Dep. Var. Mean Near Thresh.	Dependent Students: IV Estimate	Indep. Students w/ Deps: Dep. Var. Mean Near Thresh	Indep. Students w/ Deps: IV Estimate	Indep. Students w/o Deps: Dep. Var. Mean Near Thresh	Indep. Students w/o Deps: IV Estimate
Weighted Averages of All Estimates						
Deg. from Original Inst. Within 4 Years	0.372	0.0090**	0.365	0.0209**	0.380	-0.0361
	(0.255)	(0.0038)	(0.226)	(0.0087)	(0.285)	(0.0235)
Deg. from Original Inst. Within 6 Years	0.390	0.0084	0.387	0.0160	0.445	0.0103
	(0.064)	(0.0065)	(0.014)	(0.0139)	(0.293)	(0.0582)
Deg. from Any Inst. Within 4 Years	0.441	0.0041	0.413	0.0159	0.429	0.0113
	(0.282)	(0.0038)	(0.224)	(0.0100)	(0.348)	(0.0264)
Deg. from Any Inst. Within 6 Years	0.488	0.0036	0.460	0.0101	0.502	0.0297
	(0.080)	(0.0067)	(0.015)	(0.0150)	(0.296)	(0.0588)
Weighted Averages Excluding Estimates Sensitive to Bandwidth						
Deg. from Original Inst. Within 4 Years	0.377	0.0070	0.370	0.0206	—	—
	(0.385)	(0.0047)	(0.463)	(0.0130)		
Deg. from Original Inst. Within 6 Years	0.383	0.0056	0.387	0.0160	—	—
	(0.344)	(0.0199)	(0.014)	(0.0139)		
Deg. from Any Inst. Within 4 Years	0.375	0.0034	0.413	0.0216*	—	—
	(0.047)	(0.0108)	(0.272)	(0.0110)		
Deg. from Any Inst. Within 6 Years	0.474	0.0100	0.462	0.0211	0.479	-0.0090
	(0.055)	(0.0101)	(0.025)	(0.0196)	(0.500)	(0.0994)

Notes: Table presents the average of individual year estimates, weighted by the inverse variance of each estimate. Estimates in the top panel average over all years with estimates. Estimates in the bottom panel average only over estimates that are not sensitive to the choice of bandwidth. We consider an estimate to be sensitive to bandwidth if estimates using bandwidths between 50% and 150% of the bandwidth chosen using the bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014) fall outside on standard error of the main estimate. For dependent students and independent students with dependents, estimates in the top panel for four year completion outcomes are averaged over 2002-2010 while six year completion estimates are averaged over 2002-2008. For independent students without dependents, estimates in the top panel for four year completion outcomes are averaged over 2002, 2003, 2004, 2009, and 2010 while six year completion estimates are averaged over 2002-2004. See the appendix tables A.10-A.12 for the years excluded in the bottom panel. Discontinuities in Pell eligibility are in 2012 dollars. In each year, discontinuities and kinks estimated with local linear regressions and uniform kernel on data collapsed into $100 income bins. Regressions are weighted by the number of observations in each bin. For dependent students and independent students with dependents, observations with family income at a multiple of $1,000 are excluded. Bandwidths range from 2,442.46 to 8,319.28 for dependent students; 2,588.68 to 7,095.57 for independent students with dependents; and 708.69 to 1,508.65 for independent students without dependents. Conventional robust standard errors are in parentheses. The dependent variable mean near the threshold is the mean for students with family income between $1 and $1,000 above the threshold, weighted by the inverse variance of the RD or RK estimate.
*** Statistically significant at the 1% level. ** Statistically significant at the 5% level. * Statistically significant at the 10% level.

Table 4: Average IV Effect of $1000 of Pell Eligibility on Degree Completion Outcomes, Individual Micro Data, By School Sector or Type

Dependent Var. & Cohort	All	Proprietary < 2 Year	Public 2-3 Year	Proprietary 2-3 Year	Public 4+ Year	Private 4+ Year	Proprietary 4+ Year	Top 25% Inst. Med. Earnings	US News Top 200
Dependent Students									
Deg. from Original Inst. Within 4 Years	0.0096**	0.0038	0.0148***	0.0125	-0.0009	0.0135	0.0101	0.0029	0.0111
	(0.0038)	(0.0190)	(0.0048)	(0.0138)	(0.0055)	(0.0109)	(0.0085)	(0.0062)	(0.0104)
Deg. from Any Inst. Within 4 Years	0.0076*	0.0063	0.0080	0.0042	0.0048	0.0083	0.0010	0.0044	0.0168
	(0.0039)	(0.0173)	(0.0060)	(0.0134)	(0.0053)	(0.0119)	(0.0080)	(0.0068)	(0.0106)
Independent Students w/ Dependents									
Deg. from Original Inst. Within 4 Years	0.0195*	0.1229**	0.0146	0.0103	-0.0017	0.0131	0.0001	0.0184	0.1005
	(0.0104)	(0.0531)	(0.0174)	(0.0322)	(0.0304)	(0.0283)	(0.0168)	(0.0179)	(0.0701)
Deg. from Any Inst. Within 4 Years	0.0141	0.1083**	0.0126	0.0178	-0.0099	0.0181	-0.0102	0.0091	0.0534
	(0.0101)	(0.0539)	(0.0195)	(0.0317)	(0.0287)	(0.0278)	(0.0180)	(0.0179)	(0.0686)

Notes: Table presents the average of individual year estimates, weighted by the inverse variance of each estimate. Averages include estimates for the 2009 and 2010 cohorts. IV estimates are calculated as the reduced form effect of the AZ threshold divided by the discontinuity in Pell at the AZ threshold. Standard errors are calculated using the delta method, assuming no covariance between the reduced form and first stage estimates. For each year, discontinuities estimated with local linear regressions and uniform kernel on individual micro data. Sample is students with student loans only. Observations with family income at a multiple of $1,000 are excluded. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Bandwidths range from 4,003.39 to 9,904.48 for dependent students; and 3,638.24 to 9,765.65 for independent students with dependents. Conventional standard errors are in parentheses.

*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table 5: Average IV Estimate of Effect of $1000 of Pell Eligibility on Earnings

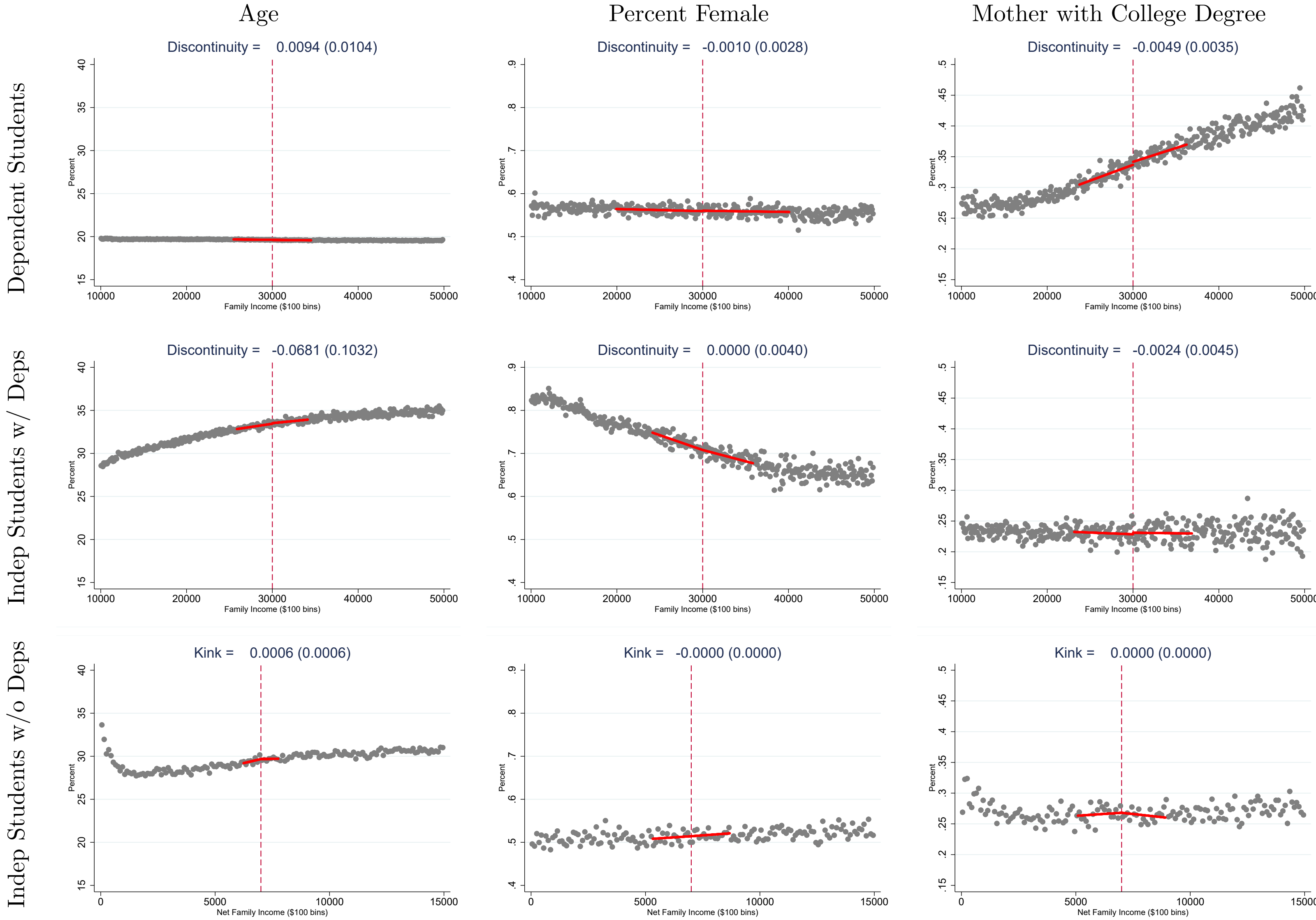
	Dependent Students		Indep. Students w/ Deps		Indep. Students w/o Deps	
Dependent Var.	Dep. Var. Mean Near Thresh.	IV Estimate	Dep. Var. Mean Near Thresh	IV Estimate	Dep. Var. Mean Near Thresh	IV Estimate
Weighted Averages of All Estimates						
Log Earnings in 2010	8.361	-0.1229*	8.300	0.2839	7.864	0.2165
	(1.606)	(0.0702)	(1.962)	(0.3136)	(2.828)	(0.2327)
Log Earnings in 2012	8.579	-0.0414	8.311	0.2660	7.831	-0.1115
	(1.689)	(0.0637)	(1.953)	(0.3459)	(2.615)	(0.2296)
Weighted Averages Excluding Estimates Sensitive to Bandwidth						
Log Earnings in 2010	8.430	-0.0625	8.268	0.6662	—	—
	(2.057)	(0.0917)	(2.419)	(0.5937)		
Log Earnings in 2012	8.644	0.1863	8.311	0.2660	—	—
	(3.628)	(0.1845)	(1.953)	(0.3459)		

Notes: Table presents the average of individual year estimates, weighted by the inverse variance of each estimate. Estimates in the top panel average over all years with estimates. Estimates in the bottom panel average only over estimates that are not sensitive to the choice of bandwidth. We consider an estimate to be sensitive to bandwidth if estimates using bandwidths between 50% and 150% of the bandwidth chosen using the bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014) fall outside on standard error of the main estimate. For dependent students and independent students with dependents, estimates in the top panel are averaged over 2002-2006. Estimates for independent students without dependents in the top panel are averaged over 2002-2004. See the appendix tables A.13-A.15 for the years excluded in the bottom panel. Discontinuities in Pell eligibility are in 2012 dollars. In each year, discontinuities and kinks estimated with local linear regressions and uniform kernel on data collapsed into $100 income bins. Regressions are weighted by the number of observations in each bin. For dependent students and independent students with dependents, observations with family income at a multiple of $1,000 are excluded. Bandwidths range from 2,513.19 to 4,618.88 for dependent students; 2,300.24 to 3,549.93 for independent students with dependents; and 741.45 to 1,188.49 for independent students without dependents. Conventional robust standard errors are in parentheses. The dependent variable mean near the threshold is the mean for students with family income between $1 and $1,000 above the threshold, weighted by the inverse variance of the RD or RK estimate.

*** Statistically significant at the 1% level. ** Statistically significant at the 5% level. * Statistically significant at the 10% level.

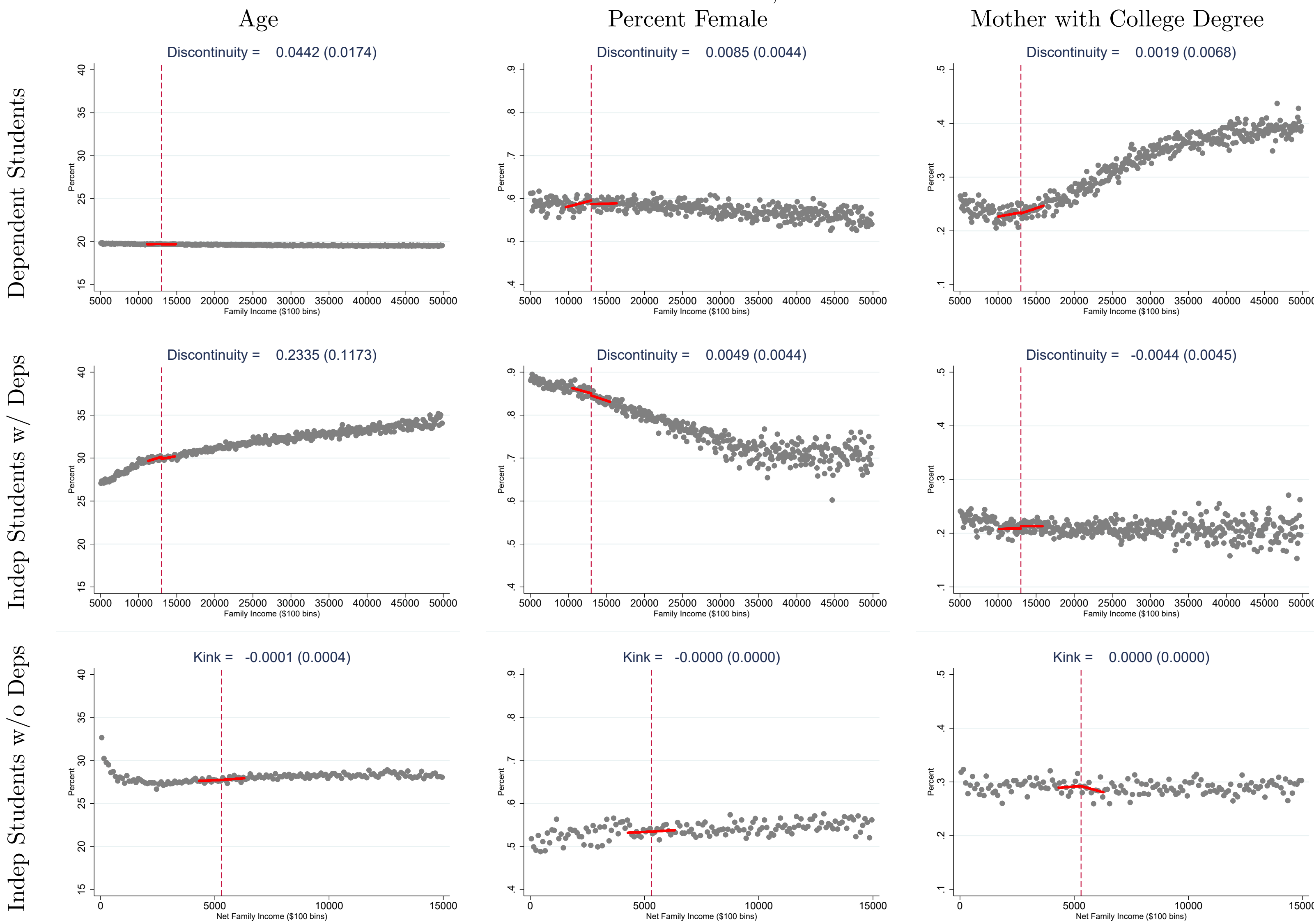
A Appendix Figures and Tables

Figure A.1:
Covariates Near Threshold, 2010



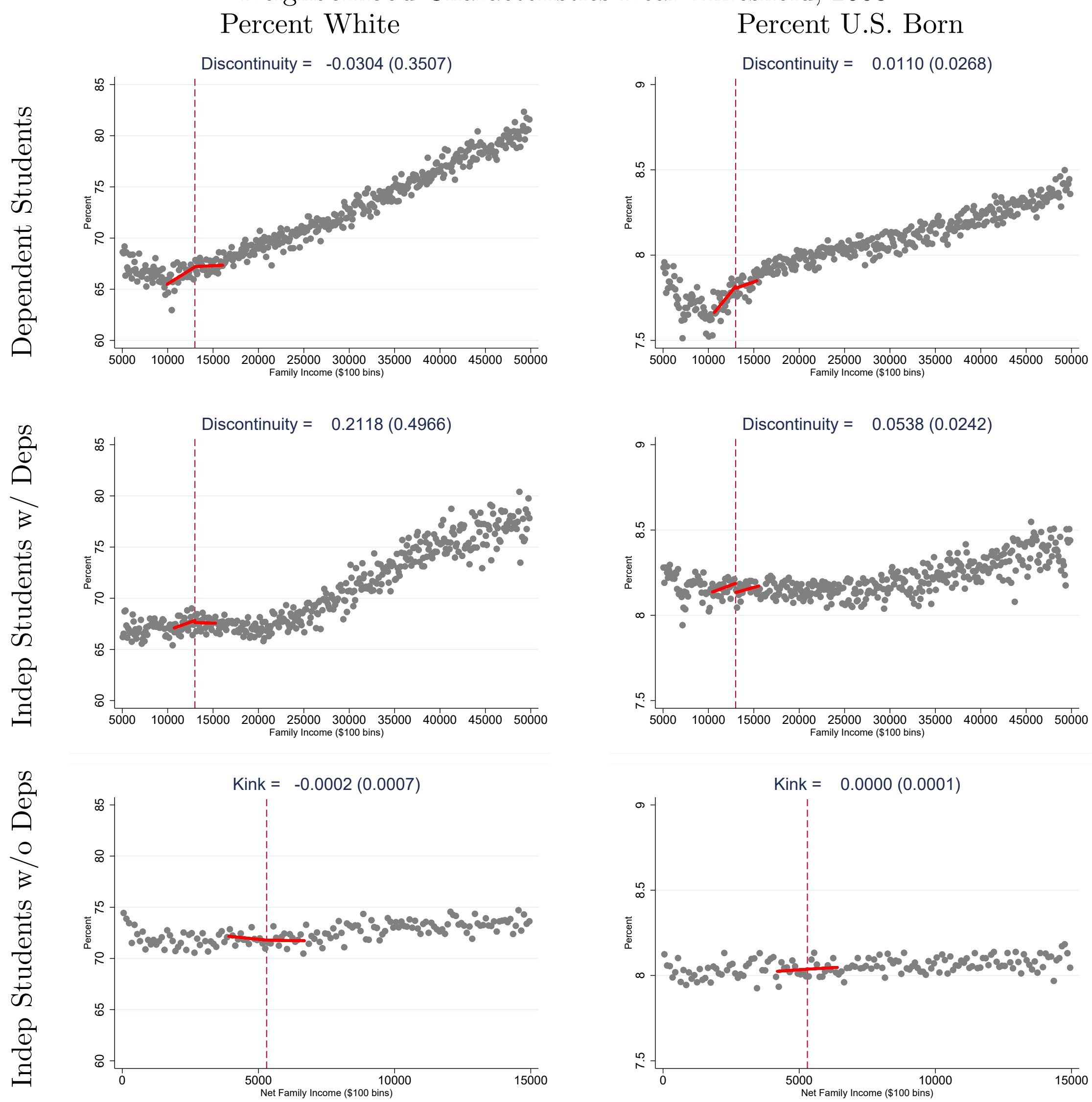
Figures display the average of the outcome variable for students in each $100 family income or net income bin. Figures for dependent students and independent students with dependents exclude income bins that include a multiple of $1,000. Dashed lines for dependent students and independent students with dependents indicate the $30,000 auto-zero EFC threshold for 2010. Dashed lines for independent students without dependents indicate the $7,000 IPA threshold for 2010.

Figure A.2:
Covariates Near Threshold, 2003



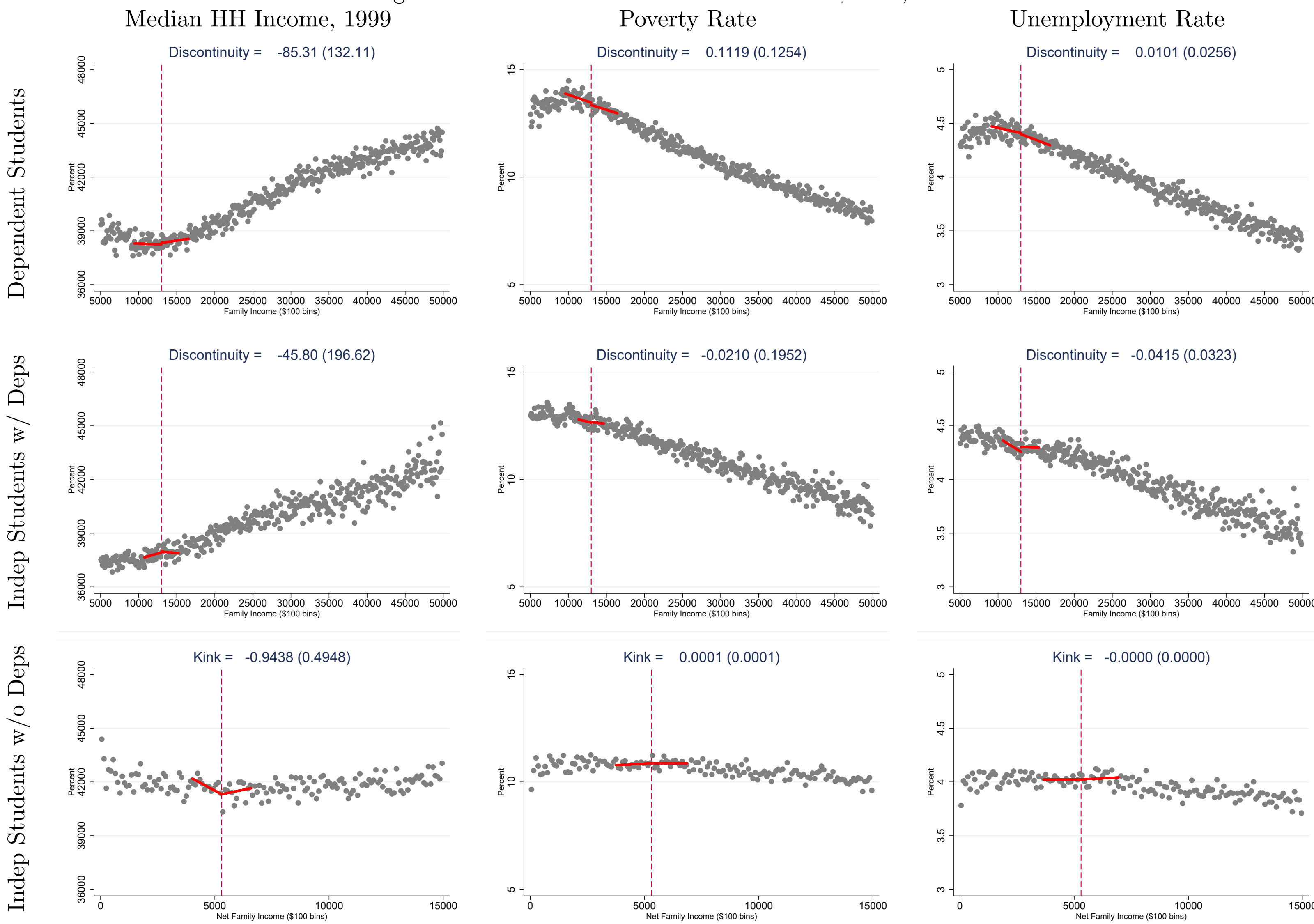
Figures display the average of the outcome variable for students in each $100 family income or net income bin. Figures for dependent students and independent students with dependents exclude income bins that include a multiple of $1,000. Dashed lines for dependent students and independent students with dependents indicate the $15,000 auto-zero EFC threshold for 2003. Dashed lines for independent students without dependents indicate the $5,300 IPA threshold for 2003.

Figure A.3:
Neighborhood Characteristics Near Threshold, 2003



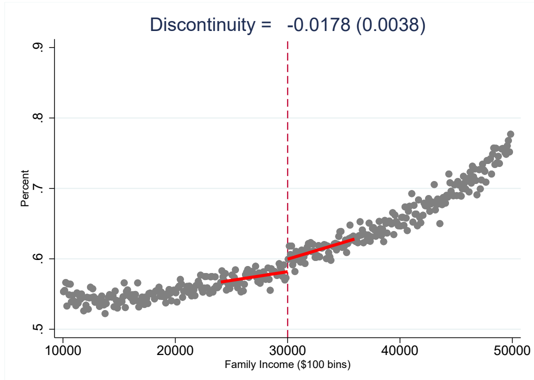
Figures display the average of the covariate for students in each $100 family income or net income bin. Figures for dependent students and independent students with dependents exclude income bins that include a multiple of $1,000. Dashed lines for dependent students and independent students with dependents indicate the $15,000 auto-zero EFC threshold for 2003. Dashed lines for independent students without dependents indicate the $5,300 IPA threshold for 2003.

Figure A.4:
Neighborhood Characteristics Near Threshold, 2003, cont.

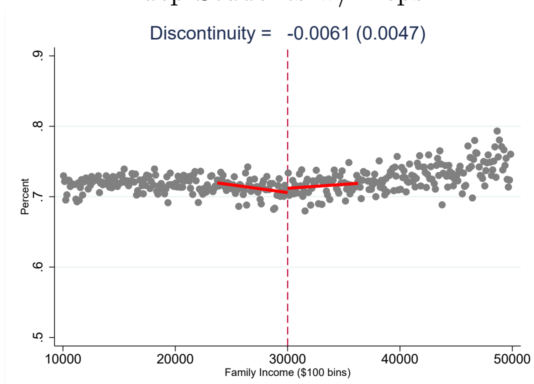


Figures display the average of the outcome variable for students in each $100 family income or net income bin. Figures for dependent students and independent students with dependents exclude income bins that include a multiple of $1,000. Dashed lines for dependent students and independent students with dependents indicate the $15,000 auto-zero EFC threshold for 2003. Dashed lines for independent students without dependents indicate the $5,300 IPA threshold for 2003.

Figure A.5:
Probability of Having Any Federal Student Loans, 2010
Dependent Students



Indep Students w/ Deps



Indep Students w/o Deps

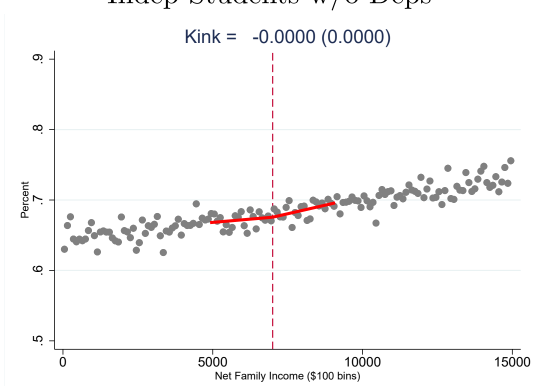
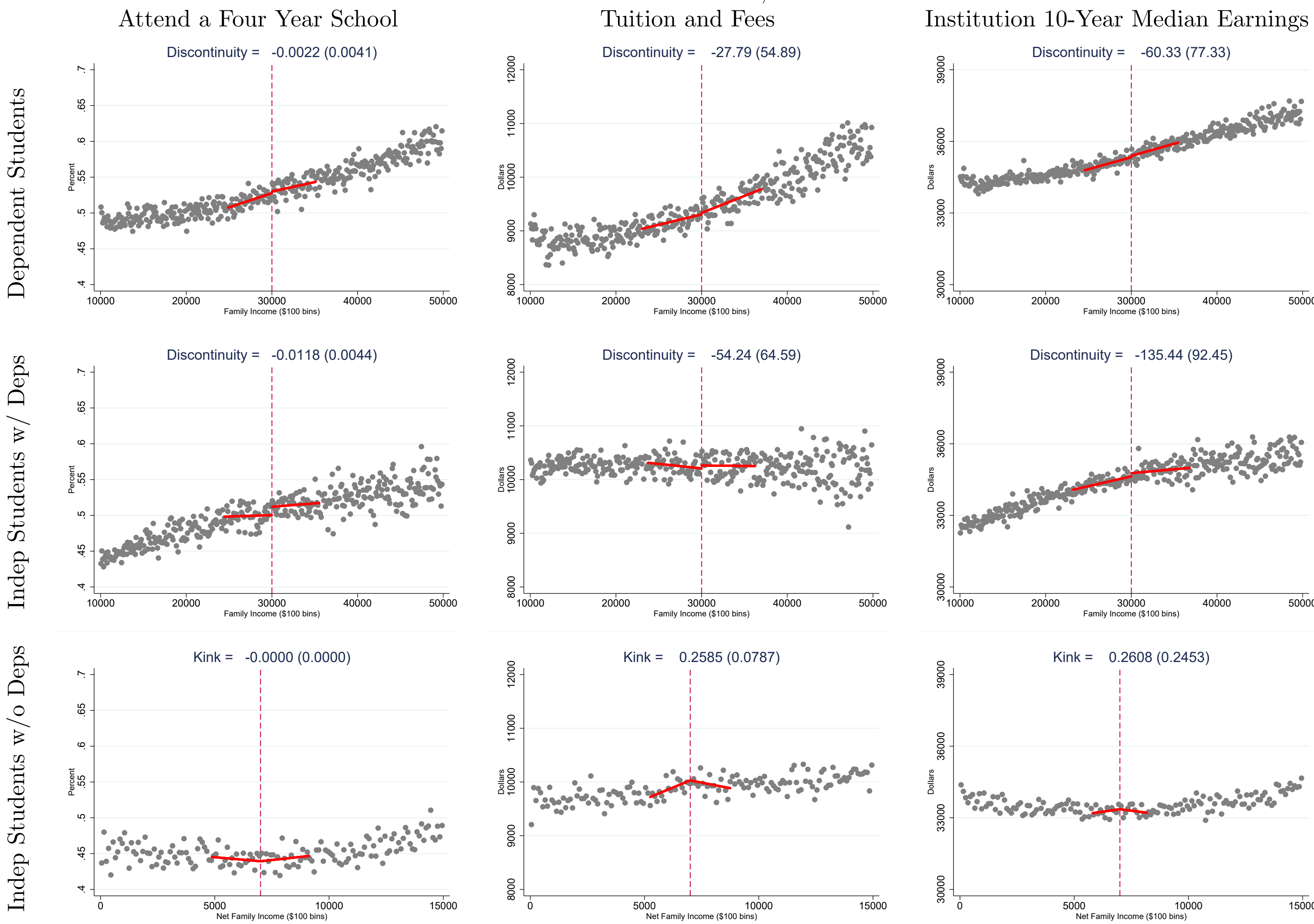
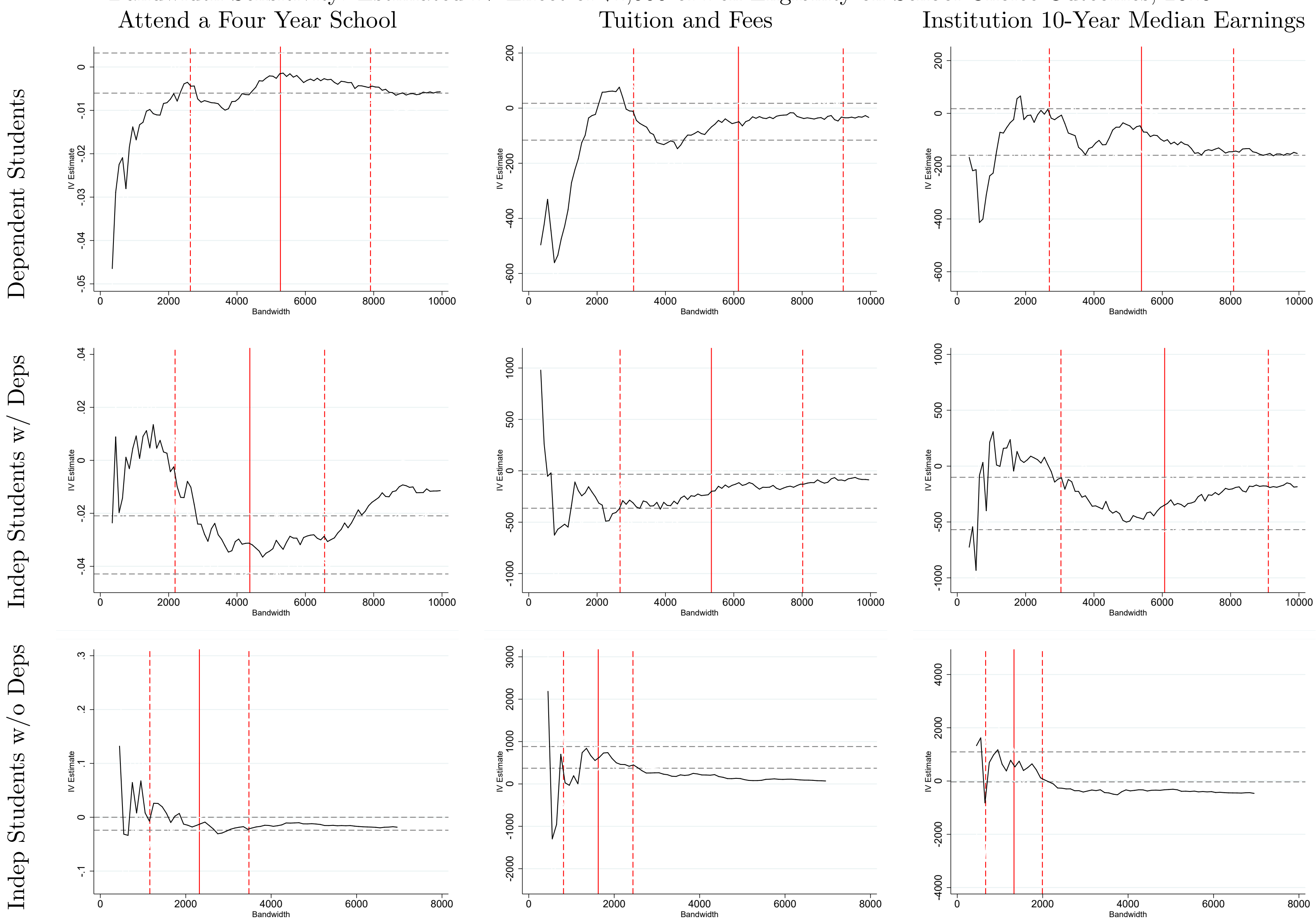


Figure A.6:
School Choice Outcomes, 2010



Figures display the average of the outcome variable for students in each $100 family income or net income bin. Tuition and fees are in 2012 dollars. Figures for dependent students and independent students with dependents exclude income bins that include a multiple of $1,000. Dashed lines for dependent students and independent students with dependents indicate the $30,000 auto-zero EFC threshold for 2010. Dashed lines for independent students without dependents indicate the $7,000 IPA threshold for 2010.

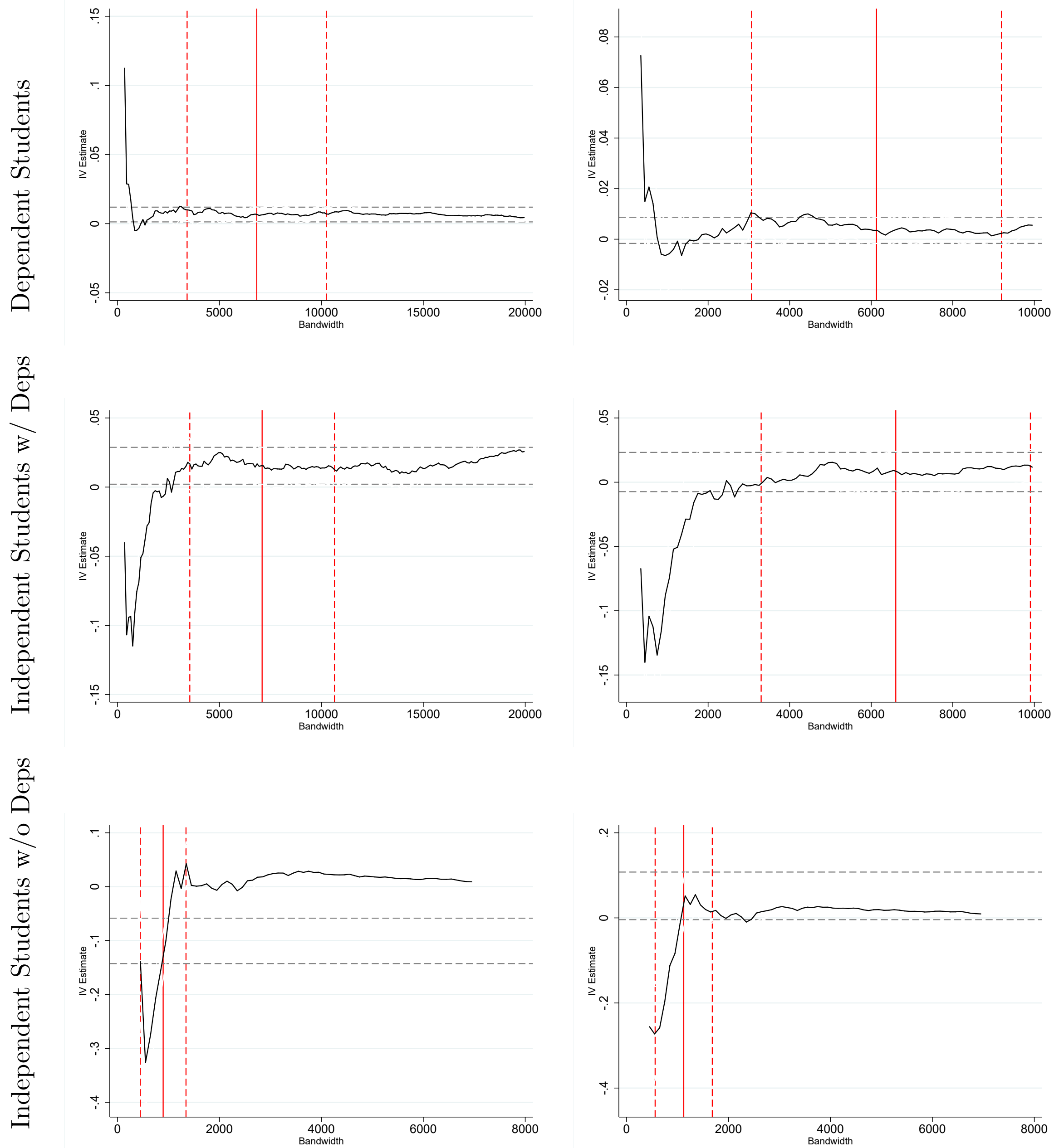
Figure A.7:
Bandwidth Sensitivity: Estimated IV Effect of $1,000 of Pell Eligibility on School Choice Outcomes, 2010



Figures display the estimated effect of Pell eligibility estimated at various bandwidths. Solid red lines denote the bandwidth used for the main estimates. Dashed red lines show 50% and 150% of the chosen bandwidth. Dashed gray lines mark a one standard error band around the main estimate. Pell eligibility and tuition and fees are in 2012 dollars.

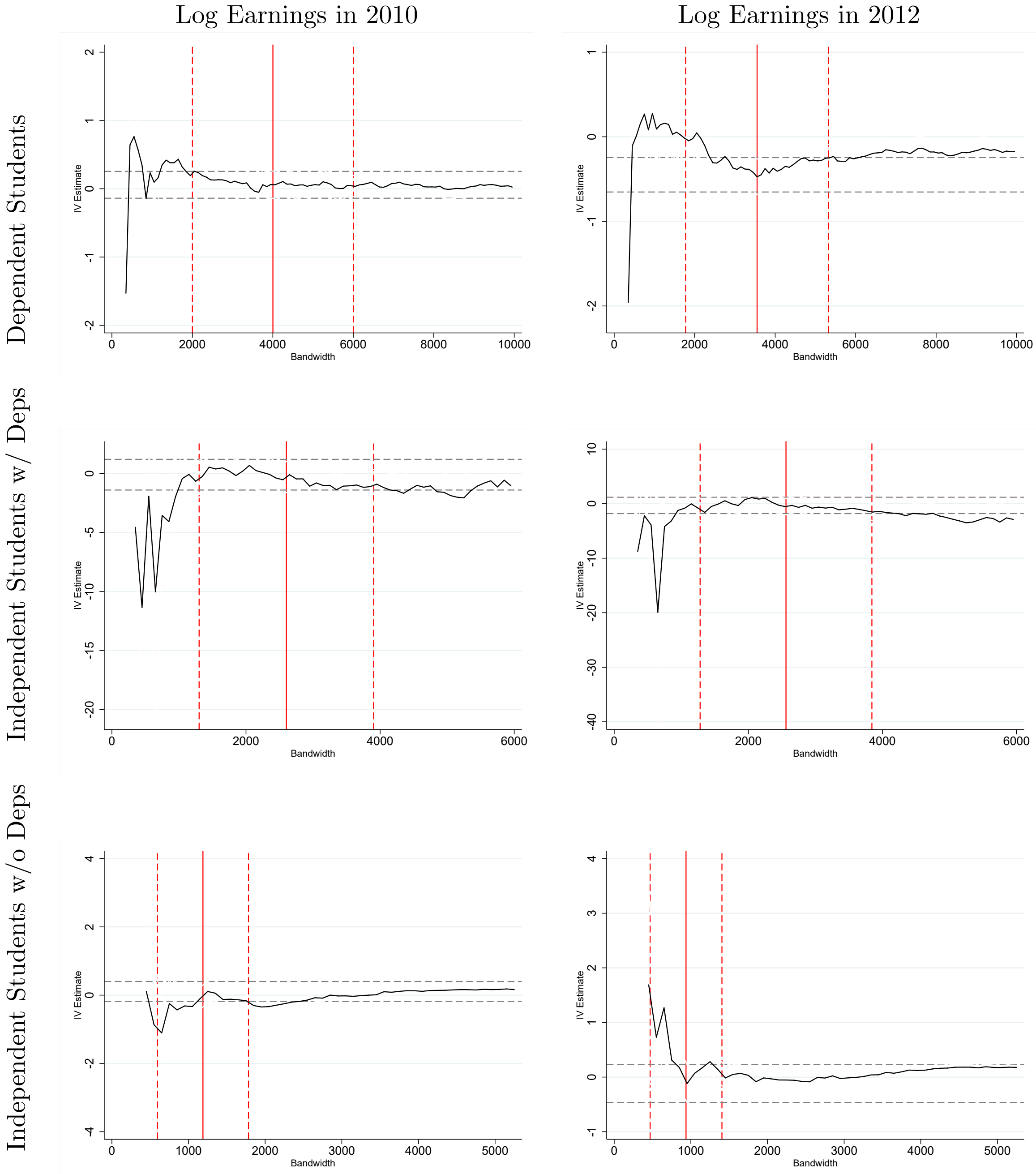
Figure A.8:
Bandwidth Sensitivity: Estimated IV Effect of $1,000 of Pell Eligibility on Degree Completion Outcomes, 2010

Degree from Original Institution Within 4 Years | Degree from Any Institution Within 4 Years



Figures display the estimated effect of Pell eligibility estimated at various bandwidths. Solid red lines denote the bandwidth used for the main estimates. Dashed red lines show 50% and 150% of the chosen bandwidth. Dashed gray lines mark a one standard error band around the main estimate. Pell eligibility is in 2012 dollars.

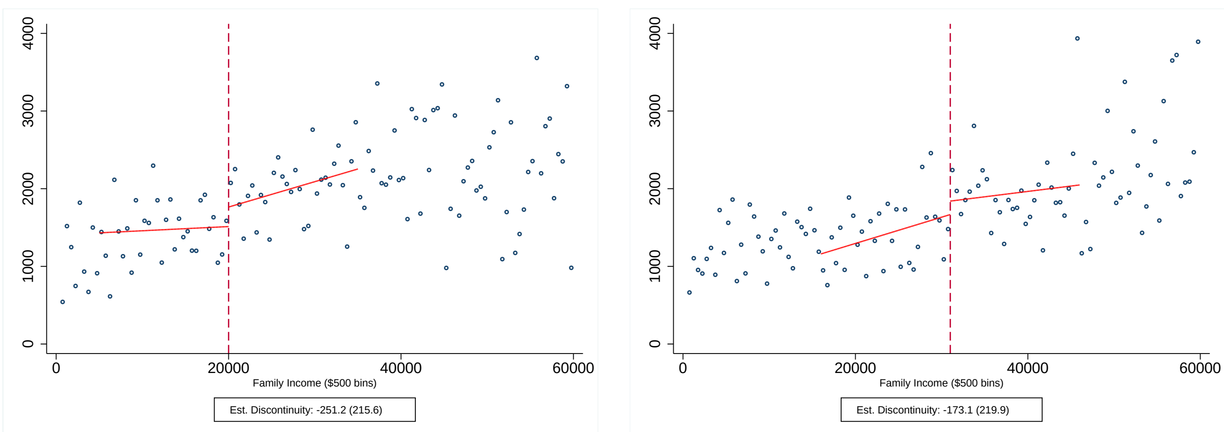
Figure A.9:
Bandwidth Sensitivity: Estimated IV Effect of $1,000 of Pell Eligibility on Earnings, 2003 Cohort



Figures display the estimated effect of Pell eligibility estimated at various bandwidths. Solid red lines denote the bandwidth used for the main estimates. Dashed red lines show 50% and 150% of the chosen bandwidth. Dashed gray lines mark a one standard error band around the main estimate. Pell eligibility is in 2012 dollars.

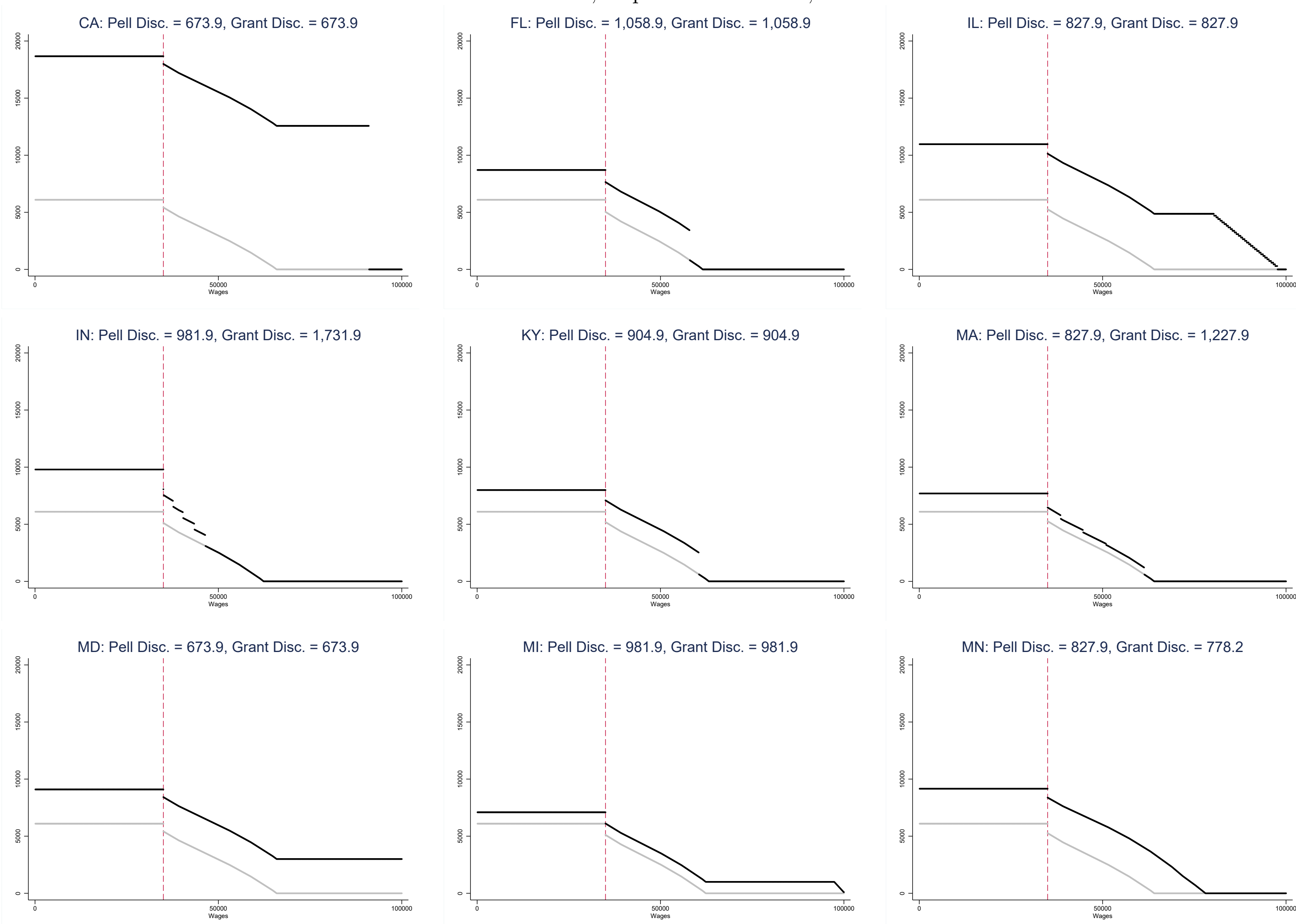
Figure A.10:
Family Income and Institutional Grants, Dependent Students

2008 2012



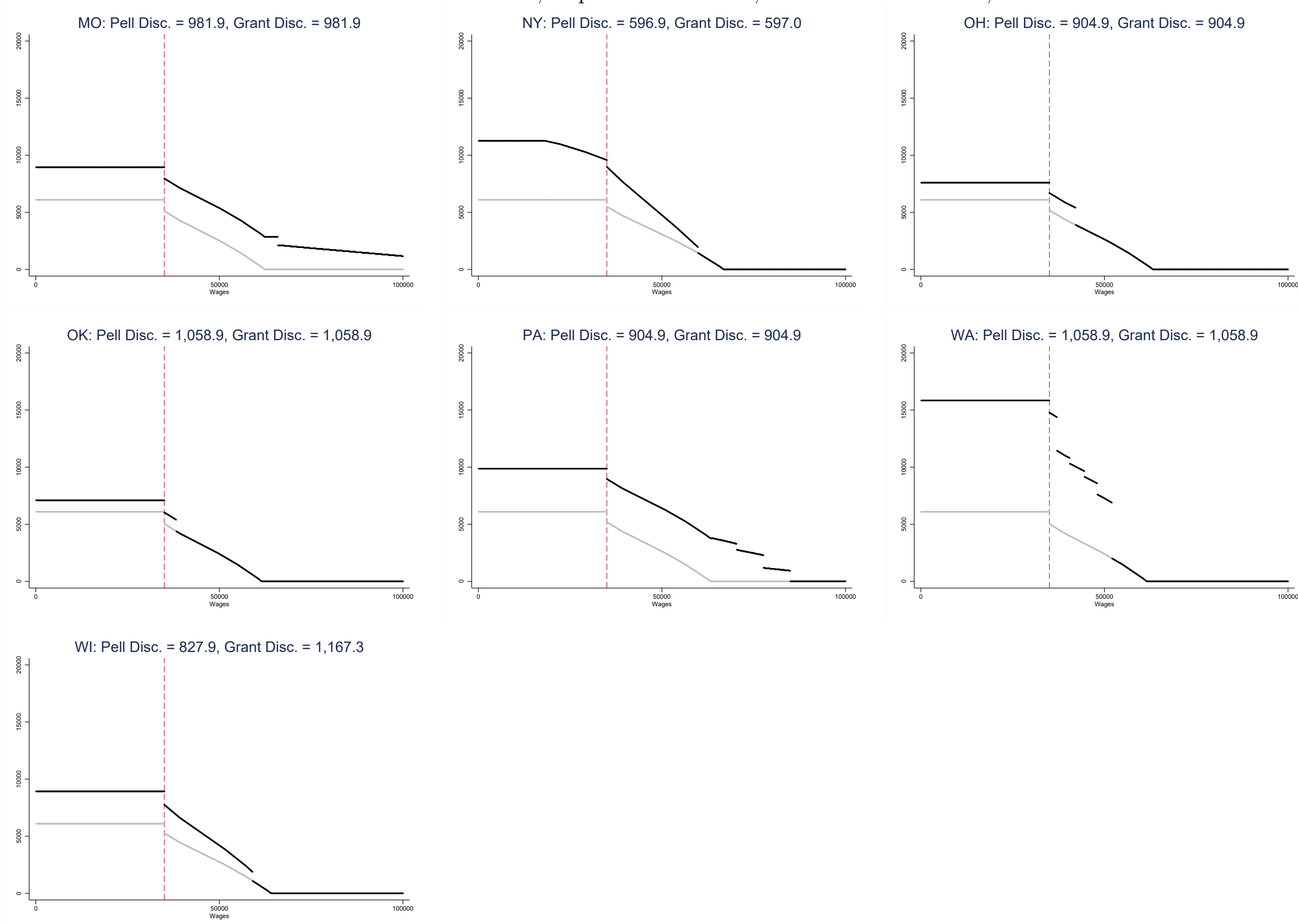
Notes: Data from the National Postsecondary Student Aid Study 2007-08 and 2011-12 waves. Each dot in the figure represents the average outcome of all students within $500 family income bins.

Figure A.11:
Simulated State and Pell Grants, Dependent Students, Public 4-Year Schools



Figures display simulated Pell grants (in grey) and the sum of Pell grants and state grants (in black) using the EFC formula and state grant formulae for 2018-2019, but changing the automatic zero EFC threshold from \$25,000 to \$35,000. The discontinuity in Pell and total grants displayed above each graph is the difference in Pell or total grants, respectively, for a student with family income (wages) of \$35,000 versus \$35,001.

Figure A.12:
Simulated State and Pell Grants, Dependent Students, Public 4-Year Schools, continued



Figures display simulated Pell grants (in grey) and the sum of Pell grants and state grants (in black) using the EFC formula and state grant formulae for 2018-2019, but changing the automatic zero EFC threshold from $25,000 to $35,000. The discontinuity in Pell and total grants displayed above each graph is the difference in Pell or total grants, respectively, for a student with family income (wages) of $35,000 versus $35,001.

Table A.1: Descriptive Statistics for Select Variables and Years

	Dependent Students			Indep. Students w/ Deps			Indep. Students w/o Deps		
	All	$1,000 Below Thresh.	$1,000 Above Thresh.	All	$1,000 Below Thresh.	$1,000 Above Thresh.	All	$1,000 Below Thresh.	$1,000 Above Thresh.
2002									
Student Characteristics									
Age	19.57	19.68	19.68	30.08	29.62	29.61	29.09	27.80	27.54
Female	55.79	58.96	58.38	79.97	82.82	83.38	53.30	53.19	52.81
Mom College Graduate	36.59	23.68	22.48	20.21	20.38	20.21	25.28	26.60	27.72
Characteristics of Students' ZIP Codes									
Percent White	74.46	66.82	66.40	67.94	67.31	67.20	70.93	71.49	71.58
Percent U.S. Born	8.12	7.78	7.73	8.09	8.10	8.10	7.95	7.99	8.00
Median Household Income in 1999	43,757	37,911	37,891	38,880	37,729	37,624	41,851	41,287	41,449
Percent of Families Below Poverty	9.96	13.51	13.72	12.19	12.72	12.84	10.79	10.96	10.94
Unemployment Rate	3.77	4.41	4.43	4.26	4.32	4.35	4.02	4.05	4.06
Observations	1,474,954	19,185	19,446	722,858	18,053	18,650	466,498	18,427	18,409
2004									
Student Characteristics									
Age	19.56	19.65	19.66	30.57	30.47	30.43	29.29	28.04	28.08
Female	56.57	58.72	59.24	81.64	83.63	83.79	55.05	54.05	54.27
Mom College Graduate	38.79	25.09	24.59	21.81	21.64	21.08	28.06	30.14	29.62
Characteristics of Students' ZIP Codes									
Percent White	75.37	67.58	67.69	68.52	67.80	67.86	71.25	72.13	72.02
Percent U.S. Born	8.18	7.86	7.84	8.18	8.17	8.17	8.03	8.05	8.05
Median Household Income in 1999	44,475	38,718	38,705	39,543	38,507	38,391	42,472	42,025	41,965
Percent of Families Below Poverty	9.61	13.09	13.02	11.80	12.31	12.33	10.52	10.65	10.67
Unemployment Rate	3.69	4.34	4.28	4.17	4.23	4.25	3.96	4.00	3.97
Observations	1,597,415	19,989	20,246	769,006	17,402	18,274	505,423	18,634	18,356
2009									
Student Characteristics									
Age	19.58	19.59	19.64	30.93	31.46	31.30	30.38	28.96	28.88
Female	55.79	57.58	57.72	79.17	79.28	79.56	52.57	51.87	51.35
Mom College Graduate	40.31	28.29	28.42	24.67	24.23	24.38	29.00	30.57	30.60
Observations	2,123,464	25,988	27,362	1,261,267	22,343	23,092	789,303	24,158	23,967

Notes: Entries show means of each variable for the subgroup indicated in the column. The threshold for dependent students and independent students with dependents is the automatic zero threshold, which is $13,000, $15,000, and $20,000 of family income ifor the 2002, 2004, and 2009 cohorts, respectively. The threshold for independent students without dependents is the income protection allowance, which is $5,110, $5,400, and $6,220 of family income less taxes paid for the 2002, 2004, and 2009 cohorts, respectively. For dependent students and independent students with dependents, observations with family income at a multiple of $1,000 are excluded for the samples near the threshold.

Table A.2: Estimated Discontinuity in the Density of Family Income at AZ and IPA Thresholds

Year	Dependent Students	Indep. Students w/ Deps	Indep. Students w/o Deps
2002	0.0597	0.0033	-0.0097
	(0.0082)	(0.0101)	(0.0128)
2003	0.0245	0.0104	-0.0410
	(0.0092)	(0.0100)	(0.0130)
2004	0.0188	0.0091	0.1148
	(0.0083)	(0.0097)	(0.0136)
2005	0.0165	-0.0023	0.0791
	(0.0080)	(0.0092)	(0.0129)
2006	0.0537	0.0078	0.0726
	(0.0082)	(0.0093)	(0.0126)
2007	0.0397	0.0394	-0.0200
	(0.0078)	(0.0104)	(0.0145)
2008	0.0654	0.0104	-0.0219
	(0.0075)	(0.0097)	(0.0138)
2009	0.0445	-0.0080	-0.0258
	(0.0071)	(0.0088)	(0.0129)
2010	0.0499	0.0505	0.0076
	(0.0069)	(0.0116)	(0.0187)

Notes: Discontinuities and standard errors are estimated using the procedure proposed by McCrary (2008), using rule-of-thumb bandwidths on the microdata.

Table A.3: Average IV Estimate of Effect of $1000 of Pell Eligibility on Federal Loan Receipt

	Dependent Students		Indep. Students w/ Deps		Indep. Students w/o Deps	
Dependent Var.	Dep. Var. Mean Near Thresh.	IV Estimate	Dep. Var. Mean Near Thresh	IV Estimate	Dep. Var. Mean Near Thresh	IV Estimate
Weighted Averages of All Estimates						
Any Student Loans	0.600	-0.0223***	0.716	-0.0227***	—	0.0029
	(0.168)	(0.0023)	(0.145)	(0.0065)		(0.0141)
Weighted Averages Excluding Estimates Sensitive to Bandwidth						
Any Student Loans	—	-0.0248***	0.716	-0.0208***	—	—
		(0.0013)	(0.145)	(0.0055)		

Notes: Table presents the average of individual year estimates, weighted by the inverse variance of each estimate. Estimates in the top panel average over all years with estimates. Estimates in the bottom panel average only over estimates that are not sensitive to the choice of bandwidth. We consider an estimate to be sensitive to bandwidth if estimates using bandwidths between 50% and 150% of the bandwidth chosen using the bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014) fall outside on standard error of the main estimate. For dependent students and independent students with dependents, estimates in the top panel are averaged over 2002-2014. Estimates in the top panel for independent students without dependents are averaged over 2002, 2003, 2004, 2009, and 2010. Discontinuities in Pell eligibility are in 2012 dollars. In each year, discontinuities and kinks estimated with local linear regressions and uniform kernel on data collapsed into $100 income bins. Regressions are weighted by the number of observations in each bin. For dependent students and independent students with dependents, observations with family income at a multiple of $1,000 are excluded. Bandwidths range from 2,507.78 to 6,727.02 for dependent students; 2,666.34 to 6,414.69 for independent students with dependents; and 837.30 to 1,477.99 for independent students without dependents. Conventional robust standard errors are in parentheses. The dependent variable mean near the threshold is the mean for students with family income between $1 and $1,000 above the threshold, weighted by the inverse variance of the RD or RK estimate.
*** Statistically significant at the 1% level. ** Statistically significant at the 5% level. * Statistically significant at the 10% level.

Table A.4: Average IV Estimate of Effect of $1000 of Pell Eligibility on School Choice Outcomes

Dependent Var.	Dependent Students: Dep. Var. Mean Near Thresh.	Dependent Students: IV Estimate	Indep. Students w/ Deps: Dep. Var. Mean Near Thresh	Indep. Students w/ Deps: IV Estimate	Indep. Students w/o Deps: Dep. Var. Mean Near Thresh	Indep. Students w/o Deps: IV Estimate
Weighted Averages of All Estimates						
Attend 4 Year School	0.530	-0.0055**	0.502	-0.0159**	0.450	-0.0125
	(0.142)	(0.0024)	(0.229)	(0.0073)	(0.431)	(0.0112)
Tuition and Fees	9,158.5	-132.40***	10,050.3	-160.68	8,990.6	77.33
	(3,487.6)	(41.33)	(2,819.8)	(99.76)	(4,621.1)	(168.86)
Institution 10-Year Median Earnings	35,262.9	-139.02***	34,456.6	-180.18	33,784.7	52.69
	(2,950.0)	(49.20)	(3,847.0)	(141.45)	(7,565.7)	(499.28)
Weighted Averages Excluding Estimates Sensitive to Bandwidth						
Attend 4 Year School	0.527	-0.0058	0.392	-0.0269	—	—
	(0.114)	(0.0038)	(0.023)	(0.0170)		
Tuition and Fees	8,925.4	-123.31**	8,058.3	-133.86	—	—
	(1,300.9)	(59.62)	(109.3)	(187.38)		
Institution 10-Year Median Earnings	35,326.9	-145.03***	34,499.2	-280.32	34,784.6	-589.87
	(3,310.8)	(52.86)	(7,917.8)	(202.94)	(10,509.7)	(1,688.18)

Notes: Table presents the average of individual year estimates, weighted by the inverse variance of each estimate. Estimates in the top panel average over all years with estimates. Estimates in the bottom panel average only over estimates that are not sensitive to the choice of bandwidth. We consider an estimate to be sensitive to bandwidth if estimates using bandwidths between 50% and 150% of the bandwidth chosen using the bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014) fall outside on standard error of the main estimate. For dependent students and independent students with dependents, estimates in the top panel for four-year school attendance and institution ten-year median earnings are averaged over 2002-2014 while estimates for tuition and fees are averaged over 2002-2012. Estimates in the top panel for independent students without dependents are averaged over 2002, 2003, 2004, 2009, and 2010. See the appendix tables A.7-A.9 for the years excluded in the bottom panel. Discontinuities in Pell eligibility and tuition and fees are in 2012 dollars. In each year, discontinuities and kinks estimated with local linear regressions and uniform kernel on data collapsed into $100 income bins. Regressions are weighted by the number of observations in each bin. For dependent students and independent students with dependents, observations with family income at a multiple of $1,000 are excluded. Bandwidths range from 2,341.36 to 9,716.53 for dependent students; 2,140.35 to 7,383.85 for independent students with dependents; and 727.49 to 2,319.79 for independent students without dependents. Conventional robust standard errors are in parentheses. The dependent variable mean near the threshold is the mean for students with family income between $1 and $1,000 above the threshold, weighted by the inverse variance of the RD or RK estimate.

*** Statistically significant at the 1% level. ** Statistically significant at the 5% level. * Statistically significant at the 10% level.

Table A.5: Average IV Estimate of Effect of $1000 of Pell Eligibility on School Choice Outcomes, Individual Micro Data, By Student Characteristics

	Dependent Students		Indep. Students w/ Deps.	
Dependent Var. & Cohort	All Students	More Than 1 School on FAFSA	All Students	More Than 1 School on FAFSA
Attend 4 Year School	-0.0013	0.0032	-0.0192*	-0.0104
	(0.0041)	(0.0054)	(0.0107)	(0.0168)
Tuition and Fees	-91.24	-134.29	-66.91	514.71**
	(60.86)	(102.13)	(144.40)	(236.44)
Institution 10-Year Median Earnings	-8.41	39.27	-324.12	-105.34
	(73.84)	(112.00)	(205.97)	(345.10)

Notes: Table presents the average of individual year estimates, weighted by the inverse variance of each estimate. Averages include estimates for the 2009 and 2010 cohorts. IV estimates are calculated as the reduced form effect of the AZ threshold divided by the discontinuity in Pell at the AZ threshold. Standard errors are calculated using the delta method, assuming no covariance between the reduced form and first stage estimates. For each year, discontinuities estimated with local linear regressions and uniform kernel on individual micro data. Observations with family income at a multiple of $1,000 are excluded. Pell eligibility and tuition and fees are in 2012 dollars. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Bandwidths range from 3,339.94 to 6,098.84 for dependent students; and 4,224.28 to 6,590.90 for independent students with dependents. Conventional standard errors are in parentheses.
*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table A.6: Average IV Estimate of Effect of $1000 of Pell on School Choice Outcomes, Independent Students Without Dependents, Individual Micro Data, By Student Characteristics

Dependent Var.	All Students	With Loans	Female	First Generation
Attend 4 Year School	-0.0001	-0.0007	-0.0223	-0.0134
	(0.0197)	(0.0199)	(0.0189)	(0.0260)
Tuition and Fees	152.47	265.92	-78.47	-201.86
	(345.78)	(256.29)	(324.34)	(360.20)
Institution 10-Year Median Earnings	502.15	190.70	554.84	363.85
	(480.44)	(646.60)	(426.09)	(416.97)

Notes: Table presents the average of individual year estimates, weighted by the inverse variance of each estimate. Averages include estimates for the 2002, 2003, 2004, 2009, and 2010 cohorts. For each year, kinks estimated with local linear two stage least squares regressions and uniform kernel on individual micro data. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Bandwidths range from 607.01 to 2,006.96. Conventional standard errors are in parentheses.
*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table A.7: IV Estimate of Effect of $1000 of Pell Eligibility on School Choice Outcomes, Dependent Students, All Years

	Attend 4 Year School				Tuition and Fees				Inst. 10-Yr Median Earnings			
	Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates	
Year	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed
2002	-0.0086†	-0.0075†	-0.0416	-0.0316†	-67.03	9.70†	-322.06	50.21†	-41.70	-9.88	-200.32	-183.80
	(0.0060)	(0.0046)		(0.0209)	(81.91)	(67.20)		(341.59)	(120.88)	(94.15)		(452.54)
2003	-0.0016	0.0066	-0.0071	0.0282	-149.86†	-96.87	-674.70	-421.91	-158.90†	34.01†	-715.43	7.57†
	(0.0065)	(0.0077)		(0.0377)	(94.28)	(107.23)		(567.55)	(129.14)	(138.01)		(665.93)
2004	-0.0060	-0.0052	-0.0213	-0.0182†	-88.34†	-78.07†	-314.96	-264.01†	-137.38†	-172.87	-489.78	-517.62
	(0.0057)	(0.0070)		(0.0242)	(96.84)	(90.13)		(309.16)	(118.45)	(152.07)		(621.39)
2005	0.0042	0.0025	0.0167	0.0117	-171.00*†	-173.14*†	-687.62	-482.39†	42.34	20.32	170.26	86.85
	(0.0053)	(0.0043)		(0.0186)	(90.52)	(95.78)		(357.41)	(105.28)	(103.46)		(432.89)
2006	-0.0064	-0.0037	-0.0226	-0.0171	-81.70†	-39.45	-288.08	-235.34	-173.67*	-221.05*†	-612.36	-774.60*
	(0.0056)	(0.0054)		(0.0197)	(89.97)	(96.80)		(348.52)	(104.67)	(120.33)		(425.31)
2007	—	0.0003	—	0.0024†	—	-131.94*	—	-354.75**	—	-77.23†	—	-433.86*†
		(0.0047)		(0.0118)		(74.39)		(179.99)		(96.77)		(226.62)
2008	—	-0.0139***†	—	-0.0246***†	—	-211.50***	—	-479.62***†	—	-196.00*	—	-423.16**
		(0.0051)		(0.0094)		(71.13)		(136.65)		(106.88)		(200.05)
2009	0.0017	-0.0010†	0.0032	0.0043	-72.10†	-126.15*	-137.78	-257.26	57.35	50.18†	109.59	95.50†
	(0.0044)	(0.0049)		(0.0081)	(66.51)	(74.51)		(156.63)	(83.55)	(95.06)		(181.72)
2010	-0.0022	-0.0022†	-0.0026	-0.0014†	-66.09†	-27.79†	-77.39	-49.35†	-34.59†	-60.33	-40.51	-70.59
	(0.0039)	(0.0041)		(0.0046)	(59.19)	(54.89)		(66.84)	(71.12)	(77.33)		(88.32)
2011	—	-0.0053	—	-0.0056†	—	18.40	—	-24.08	—	-108.89*	—	-182.21*
		(0.0034)		(0.0054)		(68.09)		(93.29)		(58.58)		(95.60)
2012	—	-0.0063*	—	-0.0101**	—	-73.61	—	-86.78	—	-65.95	—	-55.88
		(0.0034)		(0.0047)		(57.33)		(109.48)		(67.70)		(115.73)
2013	—	0.0061*†	—	0.0452*†	—	—	—	—	—	25.33†	—	2.39†
		(0.0035)		(0.0253)						(62.64)		(554.95)
2014	—	-0.0002	—	-0.0031	—	—	—	—	—	-169.89**†	—	-939.28
		(0.0043)		(0.0331)						(83.85)		(666.62)
Weighted Averages												
All Estimates	-0.0022	-0.0023*	-0.0031[1]	-0.0055**	-91.00***	-75.33***	-128.29***[1]	-132.40***	-45.46	-65.11***	-45.85[1]	-139.02***
	(0.0019)	(0.0012)	(0.0024)	(0.0024)	(29.81)	(22.05)	(41.33)	(41.33)	(37.21)	(23.90)	(49.20)	(49.20)
Excl. Estimates Sensitive to BW	-0.0014	-0.0025	-0.0018[2]	-0.0058	-67.03	-93.88***	-322.06[2]	-123.31**	-16.97	-78.97**	-2.96[2]	-145.03***
	(0.0021)	(0.0016)	(0.0038)	(0.0038)	(81.91)	(28.04)	(341.59)	(59.62)	(50.43)	(31.41)	(147.39)	(52.86)
p-value From Test That Individual Year Coefficients are Different from the Mean Across Years												
All Estimates	0.6308	0.1190	0.3524[4]	0.2048	0.9578	0.4250	0.5624[4]	0.1992	0.5300	0.5359	0.6232[4]	0.5172
Excl. Estimates Sensitive to BW	0.6922	0.6006	0.6738[5]	0.4881	—	0.3857	—	0.5483	0.3351	0.7578	0.4257[5]	0.4681

Notes: Discontinuities estimated with local linear regressions and uniform kernel. Analyses exclude observations with family income at a multiple of $1,000. Estimates in the Micro columns were estimated on individual student micro data. Estimates in Collapsed columns were estimated on data collapsed in $100 bins of family income. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Conventional standard errors are in parentheses. Pell eligibility and tuition and fees are in 2012 dollars.

*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

† Estimate is sensitive to choice of bandwidth: estimates using between 50-150% of the chosen bandwidth differ by more than one standard error from the main estimate.

1. Averages weighted by inverse of the variance of the Collapsed estimates.
2. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
3. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.
4. χ^2-statistic estimated using standard error of the Collapsed estimates.
5. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
6. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.

Table A.8: IV Estimate of Effect of $1000 of Pell Eligibility on School Choice Outcomes, Independent Students With Depenents, All Years

	Attend 4 Year School				Tuition and Fees				Inst. 10-Yr Median Earnings			
	Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates	
Year	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed
2002	-0.0033†	-0.0039†	-0.0777	-0.1287†	28.23†	33.96†	671.82	765.54†	43.69†	35.48	1,039.69	981.51
	(0.0056)	(0.0057)		(0.1344)	(67.23)	(76.34)		(2,552.36)	(95.47)	(115.95)		(2,999.98)
2003	-0.0001†	0.0006	-0.0021	0.0481	-42.56†	-62.07†	-1,022.42	-655.84†	-276.46**†	-185.71†	-6,641.41	-6,084.18*
	(0.0059)	(0.0053)		(0.1559)	(76.35)	(69.72)		(2,756.71)	(111.59)	(133.64)		(3,488.98)
2004	-0.0048	-0.0077*	-0.0753	-0.1050†	-46.51†	-35.27	-736.17	-457.99†	-169.90	-86.11	-2,689.42	-2,237.10
	(0.0051)	(0.0046)		(0.0863)	(73.06)	(58.71)		(1,096.11)	(108.32)	(107.43)		(2,061.40)
2005	0.0037†	0.0034†	0.0559	0.0004†	2.97	0.0659	45.36	205.90	222.87**†	249.12**†	3,404.06	1,303.62†
	(0.0052)	(0.0047)		(0.0718)	(60.97)	(76.0085)		(1,490.26)	(112.12)	(112.59)		(1,656.59)
2006	-0.0064	-0.0076	-0.0714	-0.0821	116.68*	143.31**	1,309.26	1,706.67†	-68.10†	-101.58†	-764.18	-1,252.49
	(0.0053)	(0.0054)		(0.0796)	(69.19)	(72.56)		(1,097.09)	(107.01)	(112.64)		(1,624.83)
2007	—	0.0016†	—	0.0076†	—	-27.75	—	-158.56	—	-40.59†	—	-170.80†
		(0.0049)		(0.0182)		(63.74)		(260.67)		(82.57)		(296.86)
2008	—	-0.0127**†	—	-0.0384*	—	-104.88†	—	-401.90*†	—	-87.08†	—	-406.81†
		(0.0054)		(0.0225)		(66.60)		(239.26)		(94.72)		(374.42)
2009	0.0074*†	0.0046	0.0345	0.0274†	6.25	37.41	29.12	56.02†	34.25†	31.81	159.60	130.92
	(0.0043)	(0.0043)		(0.0219)	(61.63)	(67.51)		(249.96)	(83.95)	(93.37)		(437.25)
2010	-0.0168***†	-0.0118***†	-0.0402	-0.0319***†	-41.65	-54.24†	-99.39	-198.94†	-213.53**†	-135.44	-509.58	-334.35
	(0.0053)	(0.0044)		(0.0110)	(70.00)	(64.59)		(165.33)	(101.43)	(92.45)		(234.12)
2011	—	0.0024†	—	0.0006†	—	29.67	—	319.95	—	-20.63†	—	362.36†
		(0.0056)		(0.0260)		(63.63)		(349.21)		(85.36)		(470.60)
2012	—	-0.0012	—	-0.0043	—	-120.13**	—	-820.55*	—	37.23	—	227.15†
		(0.0043)		(0.0281)		(55.50)		(442.26)		(81.80)		(653.85)
2013	—	0.0064†	—	0.5899†	—	—	—	—	—	90.56†	—	20,698.43†
		(0.0054)		(0.8623)						(94.24)		(29,326.04)
2014	—	-0.0002	—	-0.3663†	—	—	—	—	—	-122.79†	—	-67,541.28†
		(0.0052)		(2.8908)						(81.66)		(51,029.81)
Weighted Averages												
All Estimates	-0.0023	-0.0020	-0.0252***[1]	-0.0159**	5.60	-22.46	-49.21[1]	-160.68	-52.85	-26.11	-347.88**[1]	-180.18
	(0.0020)	(0.0014)	(0.0073)	(0.0073)	(25.60)	(19.87)	(99.76)	(99.76)	(38.31)	(26.60)	(141.45)	(141.45)
Excl. Estimates Sensitive to BW	-0.0055	-0.0016	-0.0732[2]	-0.0269	19.38	-7.87	-38.28[2]	-133.86	-169.90	-21.13	-2,689.42[2]	-280.32
	(0.0037)	(0.0020)	(0.0585)	(0.0170)	(32.53)	(24.32)	(136.24)	(187.38)	(108.32)	(42.95)	(2,061.40)	(202.94)
p-value From Test That Individual Year Coefficients are Different from the Mean Across Years												
All Estimates	0.0214	0.0875	0.0759[4]	0.4076	0.6692	0.2494	0.8875[4]	0.5191	0.0145	0.2874	0.0671[4]	0.5704
Excl. Estimates Sensitive to BW	0.8276	0.3822	0.9736[5]	0.6549	0.4144	0.1446	0.6322[5]	0.2446	—	0.5640	—	0.3989

Notes: Discontinuities estimated with local linear regressions and uniform kernel. Analyses exclude observations with family income at a multiple of $1,000. Estimates in the Micro columns were estimated on individual student micro data. Estimates in Collapsed columns were estimated on data collapsed in $100 bins of family income. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Conventional standard errors are in parentheses. Pell eligibility and tuition and fees are in 2012 dollars.
*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.
† Estimate is sensitive to choice of bandwidth: estimates using between 50-150% of the chosen bandwidth differ by more than one standard error from the main estimate.
1. Averages weighted by inverse of the variance of the Collapsed estimates.
2. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
3. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.
4. χ^2-statistic estimated using standard error of the Collapsed estimates.
5. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
6. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.

Table A.9: IV Estimate of Effect of $1000 of Pell Eligibility on School Choice Outcomes, Independent Students Without Depenents, All Years

	Attend 4 Year School				Tuition and Fees				Inst. 10-Yr Median Earnings			
	Reduced Form Kinks		IV Estimates		Reduced Form Kinks		IV Estimates		Reduced Form Kinks		IV Estimates	
Year	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed
2002	-0.0000	0.0000†	-0.0169†	-0.0495†	0.0502	-0.1328†	230.04†	-700.91†	0.0055	-0.0164†	22.57	-1,292.09†
		(0.0000)	(0.0553)	(0.1026)		(0.2847)	(1,932.01)	(1,611.96)		(0.2428)	(2,846.51)	(2,760.68)
2003	0.0000	-0.0000†	0.0176	0.1397**†	-0.2671	-0.0889†	-821.01†	-601.96†	-0.3431	-0.1391†	-1,205.42	-153.21†
		(0.0000)	(0.0572)	(0.0662)		(0.1002)	(777.77)	(528.68)		(0.7051)	(2,041.45)	(2,913.84)
2004	-0.0000	0.0000†	-0.1471*†	-0.1936***†	0.1699	0.3224†	600.38†	958.26†	0.3772	0.1882†	1,136.93†	-4,106.14**†
		(0.0000)	(0.0868)	(0.0690)		(0.3303)	(1,049.69)	(1,428.20)		(0.6241)	(1,160.97)	(1,960.11)
2009	0.0000	0.0000†	0.0030	-0.0053†	0.0532	-0.0332	163.06†	-321.76†	0.1062	0.0555†	302.15	-589.87
		(0.0000)	(0.0290)	(0.0425)		(0.2237)	(626.36)	(255.57)		(0.3503)	(664.24)	(1,688.18)
2010	0.0000	-0.0000†	0.0252	-0.0120†	0.1805	0.2585***†	539.24	626.67**†	0.3257	0.2608†	971.93	531.42†
		(0.0000)	(0.0399)	(0.0120)		(0.0787)	(578.69)	(255.60)		(0.2453)	(1,020.31)	(563.32)
Weighted Averages												
All Estimates	0.0000[1]	-0.0000	-0.0001	-0.0125	0.0192[1]	0.1109*	152.47	77.33	0.1436[1]	0.1017	502.15	52.69
	(0.0000)	(0.0000)	(0.0197)	(0.0112)	(0.0575)	(0.0575)	(345.78)	(168.86)	(0.1469)	(0.1469)	(480.44)	(499.28)
Excl. Estimates Sensitive to BW	0.0000[3]	—	0.0116	—	0.1805**[3]	-0.0332	539.24	—	0.1298[3]	—	370.98	-589.87
	(0.0000)		(0.0217)		(0.0787)	(0.2237)	(578.69)		(0.1512)		(527.75)	(1,688.18)
p-value From Test That Individual Year Coefficients are Different from the Mean Across Years												
All Estimates	0.0158[4]	0.6321	0.4826	0.0150	0.0134[4]	0.0599	0.6995	0.0535	0.8262[4]	0.9370	0.8563	0.2301
Excl. Estimates Sensitive to BW	0.8243[6]	—	0.8981	—	—	—	—	—	0.7163[6]	—	0.8088	—

Notes: Kinks estimated with local linear regressions and uniform kernel. Estimates in the Micro columns were estimated on individual student micro data. Estimates in Collapsed columns were estimated on data collapsed in $100 bins of family income less taxes paid. All estimates with an absolute value less than one are rounded to the fourth decimal place. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Conventional standard errors are in parentheses. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

† Estimate is sensitive to choice of bandwidth: estimates using between 50-150% of the chosen bandwidth differ by more than one standard error from the main estimate.

1. Averages weighted by inverse of the variance of the Collapsed estimates.
2. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
3. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.
4. χ^2-statistic estimated using standard error of the Collapsed estimates.
5. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
6. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.

Table A.10: IV Estimate of Effect of $1000 of Pell Eligibility on Degree Completion Outcomes, Dependent Students, All Years

	Deg. from Original Inst. Within 4 Years				Deg. from Original Inst. Within 6 Years				Deg. from Any Inst. Within 4 Years				Deg. from Any Inst. Within 6 Years			
	Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates	
Year	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed
2002	-0.0006	0.0024†	-0.0027	-0.0028	-0.0005†	0.0028	-0.0025	0.0016	0.0014†	-0.0006	0.0065	-0.0013	-0.0016†	-0.0003	-0.0078	0.0140
	(0.0073)	(0.0062)		(0.0281)	(0.0079)	(0.0067)		(0.0277)	(0.0065)	(0.0062)		(0.0347)	(0.0079)	(0.0081)		(0.0370)
2003	-0.0007	0.0009†	-0.0032	-0.0034†	0.0014	0.0047†	0.0063	-0.0009†	0.0023	0.0035†	0.0104	-0.0244†	0.0067†	0.0099†	0.0300	0.0009†
	(0.0068)	(0.0057)		(0.0297)	(0.0071)	(0.0060)		(0.0289)	(0.0070)	(0.0060)		(0.0312)	(0.0073)	(0.0067)		(0.0362)
2004	—	-0.0041†	—	-0.0029†	—	-0.0041†	—	-0.0198†	—	-0.0015	—	-0.0118	—	-0.0023	—	-0.0016†
		(0.0063)		(0.0217)		(0.0061)		(0.0210)		(0.0076)		(0.0251)		(0.0068)		(0.0251)
2005	-0.0030†	-0.0049	-0.0119	-0.0095†	0.0054	0.0029	0.0218	0.0099	-0.0003†	-0.0017†	-0.0011	-0.0073†	0.0089	0.0058	0.0356	0.0248
	(0.0068)	(0.0071)		(0.0254)	(0.0073)	(0.0078)		(0.0285)	(0.0071)	(0.0069)		(0.0267)	(0.0076)	(0.0097)		(0.0354)
2006	0.0090†	0.0072†	0.0319	0.0223†	0.0045	0.0036†	0.0160	0.0137†	0.0064	0.0023†	0.0224	0.0074†	-0.0047	-0.0066†	-0.0164	-0.0056†
	(0.0070)	(0.0055)		(0.0188)	(0.0072)	(0.0057)		(0.0211)	(0.0068)	(0.0055)		(0.0167)	(0.0067)	(0.0059)		(0.0173)
2007	—	-0.0036†	—	0.0047†	—	-0.0004	—	0.0033†	—	-0.0105*†	—	-0.0054†	—	-0.0055†	—	0.0004†
		(0.0053)		(0.0113)		(0.0053)		(0.0115)		(0.0058)		(0.0109)		(0.0057)		(0.0120)
2008	—	0.0092*†	—	0.0102	—	0.0108**	—	0.0218**†	—	0.0046	—	0.0080	—	0.0080	—	0.0082
		(0.0050)		(0.0112)		(0.0054)		(0.0111)		(0.0060)		(0.0127)		(0.0067)		(0.0110)
2009	0.0121***†	0.0130**†	0.0232	0.0248**†	—	—	—	—	0.0114**†	0.0125**†	0.0218	0.0240**†	—	—	—	—
	(0.0046)	(0.0052)		(0.0100)					(0.0048)	(0.0062)		(0.0118)				
2010	0.0058	0.0040†	0.0068	0.0066	—	—	—	—	0.0037	0.0030†	0.0043	0.0035†	—	—	—	—
	(0.0042)	(0.0050)		(0.0053)					(0.0044)	(0.0045)		(0.0052)				
Weighted Averages																
All Estimates	0.0054**	0.0035*	0.0103***[1]	0.0090**	0.0028	0.0031	0.0112*[1]	0.0084	0.0050**	0.0015	0.0080**[1]	0.0041	0.0020	0.0003	-0.0016[1]	0.0036
	(0.0023)	(0.0019)	(0.0038)	(0.0038)	(0.0037)	(0.0023)	(0.0065)	(0.0065)	(0.0023)	(0.0020)	(0.0038)	(0.0038)	(0.0037)	(0.0026)	(0.0067)	(0.0067)
Excl. Estimates Sensitive to BW	0.0031	-0.0049	0.0062[2]	0.0070	0.0038	0.0043	0.0150[2]	0.0056	0.0040	0.0012	0.0060[2]	0.0034	0.0012	0.0026	-0.0064[2]	0.0100
	(0.0032)	(0.0071)	(0.0052)	(0.0047)	(0.0042)	(0.0030)	(0.0146)	(0.0199)	(0.0033)	(0.0037)	(0.0049)	(0.0108)	(0.0050)	(0.0038)	(0.0155)	(0.0101)
p-value From Test That Individual Year Coefficients are Different from the Mean Across Years																
All Estimates	0.3652	0.2608	0.4692[4]	0.8078	0.9400	0.6857	0.9268[4]	0.7183	0.7115	0.3986	0.7384[4]	0.7382	0.4899	0.3948	0.4517[4]	0.9851
Excl. Estimates Sensitive to BW	0.6155	—	0.8990[5]	0.9021	0.9170	0.5110	0.9273[5]	0.8344	0.9129	0.7700	0.5792[5]	0.7739	0.1823	0.7076	0.1860[5]	0.8987

Notes: Discontinuities estimated with local linear regressions and uniform kernel. Analyses exclude observations with family income at a multiple of $1,000. Estimates in the Micro columns were estimated on individual student micro data. Estimates in Collapsed columns were estimated on data collapsed in $100 bins of family income. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Conventional standard errors are in parentheses. Pell eligibility amounts are in 2012 dollars.

*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

† Estimate is sensitive to choice of bandwidth: estimates using between 50-150% of the chosen bandwidth differ by more than one standard error from the main estimate.

1. Averages weighted by inverse of the variance of the Collapsed estimates.
2. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
3. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.
4. χ^2-statistic estimated using standard error of the Collapsed estimates.
5. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
6. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.

Table A.11: IV Estimate of Effect of $1000 of Pell Eligibility on Degree Completion Outcomes, Independent Students With Depenents, All Years

	Deg. from Original Inst. Within 4 Years				Deg. from Original Inst. Within 6 Years				Deg. from Any Inst. Within 4 Years				Deg. from Any Inst. Within 6 Years			
	Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates	
Year	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed
2002	0.0016†	0.0096	0.0380	0.1367	0.0022†	0.0128	0.0531	0.1770	0.0095†	0.0090	0.2267	0.0782†	0.0049†	0.0059	0.1170	0.0073†
	(0.0075)	(0.0091)		(0.3279)	(0.0076)	(0.0088)		(0.3094)	(0.0068)	(0.0080)		(0.2988)	(0.0065)	(0.0086)		(0.2822)
2003	0.0007†	-0.0010	0.0161	-0.0330	-0.0005†	-0.0015	-0.0120	-0.1489	-0.0030†	-0.0011	-0.0727	-0.0678	-0.0014†	-0.0024	-0.0344	-0.2422
	(0.0066)	(0.0089)		(0.3220)	(0.0069)	(0.0102)		(0.3057)	(0.0073)	(0.0073)		(0.2621)	(0.0068)	(0.0089)		(0.2810)
2004	—	0.0127†	—	0.1783	—	0.0088	—	0.1524	—	0.0128†	—	0.1052	—	0.0169*†	—	0.1227†
		(0.0084)		(0.1314)		(0.0094)		(0.1438)		(0.0093)		(0.1404)		(0.0098)		(0.1415)
2005	0.0065†	0.0101†	0.0991	0.1730	0.0108	0.0154*†	0.1657	0.1550	0.0046	0.0071	0.0708	0.1018	0.0061	0.0070†	0.0932	0.1381
	(0.0069)	(0.0079)		(0.1481)	(0.0075)	(0.0093)		(0.1539)	(0.0071)	(0.0078)		(0.1590)	(0.0075)	(0.0083)		(0.1537)
2006	0.0082	0.0089	0.0923	0.1200	0.0090	0.0069	0.1007	0.1279	0.0046†	0.0035	0.0511	0.0435	-0.0012	-0.0010	-0.0136	-0.0070
	(0.0067)	(0.0062)		(0.0887)	(0.0067)	(0.0058)		(0.0846)	(0.0067)	(0.0060)		(0.0838)	(0.0064)	(0.0057)		(0.0895)
2007	—	0.0069	—	0.0213†	—	0.0057	—	0.0241	—	0.0141**†	—	0.0383*	—	0.0059	—	0.0219
		(0.0056)		(0.0183)		(0.0056)		(0.0184)		(0.0060)		(0.0212)		(0.0056)		(0.0203)
2008	—	0.0027†	—	0.0038†	—	-0.0002†	—	-0.0100	—	0.0008	—	-0.0112†	—	0.0005	—	-0.0090†
		(0.0051)		(0.0237)		(0.0049)		(0.0223)		(0.0050)		(0.0239)		(0.0050)		(0.0236)
2009	0.0078*	0.0125**†	0.0363	0.0327*†	—	—	—	—	0.0062	0.0066	0.0287	0.0300	—	—	—	—
	(0.0045)	(0.0056)		(0.0196)					(0.0042)	(0.0058)		(0.0262)				
2010	0.0057	0.0054	0.0136	0.0154	—	—	—	—	0.0035	0.0050	0.0084	0.0079	—	—	—	—
	(0.0050)	(0.0056)		(0.0133)					(0.0049)	(0.0056)		(0.0153)				
Weighted Averages																
All Estimates	0.0056**	0.0073***	0.0223**[1]	0.0209**	0.0054	0.0054**	0.1058***[1]	0.0160	0.0046**	0.0057***	0.0150[1]	0.0159	0.0019	0.0033	0.0171[1]	0.0101
	(0.0024)	(0.0022)	(0.0087)	(0.0087)	(0.0036)	(0.0026)	(0.0139)	(0.0139)	(0.0024)	(0.0021)	(0.0100)	(0.0100)	(0.0034)	(0.0026)	(0.0150)	(0.0150)
Excl. Estimates Sensitive to BW	0.0071**	0.0064**	0.0218**[2]	0.0206	0.0098**	0.0067**	0.1158[2]	0.0160	0.0050*	0.0040*	0.0140[2]	0.0216*	0.0019	0.0017	0.0135[2]	0.0211
	(0.0030)	(0.0030)	(0.0109)	(0.0130)	(0.0050)	(0.0032)	(0.0742)	(0.0139)	(0.0029)	(0.0024)	(0.0131)	(0.0110)	(0.0049)	(0.0028)	(0.0774)	(0.0196)
p-value From Test That Individual Year Coefficients are Different from the Mean Across Years																
All Estimates	0.9392	0.9108	0.8723[4]	0.7639	0.6325	0.6931	0.9538[4]	0.4734	0.8744	0.7893	0.9263[4]	0.8652	0.7992	0.7152	0.9142[4]	0.7887
Excl. Estimates Sensitive to BW	0.9364	0.9067	0.4596[5]	0.5409	0.8520	0.8771	0.7115[5]	0.4734	0.9194	0.9468	0.7498[5]	0.8883	0.4582	0.8611	0.5483[5]	0.6689

Notes: Discontinuities estimated with local linear regressions and uniform kernel. Analyses exclude observations with family income at a multiple of $1,000. Estimates in the Micro columns were estimated on individual student micro data. Estimates in Collapsed columns were estimated on data collapsed in $100 bins of family income. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Conventional standard errors are in parentheses. Pell eligibility amounts are in 2012 dollars.

*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

† Estimate is sensitive to choice of bandwidth: estimates using between 50-150% of the chosen bandwidth differ by more than one standard error from the main estimate.

1. Averages weighted by inverse of the variance of the Collapsed estimates.
2. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
3. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.
4. χ^2-statistic estimated using standard error of the Collapsed estimates.
5. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
6. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.

Table A.12: IV Estimate of Effect of $1000 of Pell Eligibility on Degree Completion Outcomes, Independent Students Without Depenents, All Years

	Deg. from Original Inst. Within 4 Years				Deg. from Original Inst. Within 6 Years				Deg. from Any Inst. Within 4 Years				Deg. from Any Inst. Within 6 Years			
	Reduced Form Kinks		IV Estimates		Reduced Form Kinks		IV Estimates		Reduced Form Kinks		IV Estimates		Reduced Form Kinks		IV Estimates	
Year	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed
2002	-0.0000	0.0000†	-0.2018†	-0.0846†	-0.0001	-0.0000†	-0.2994†	-0.1682†	-0.0000	0.0000†	-0.0573†	-0.0579†	-0.0000	0.0000†	-0.0541†	-0.0090
		(0.0000)	(0.1858)	(0.0902)		(0.0000)	(0.1863)	(0.1191)		(0.0000)	(0.1531)	(0.1351)		(0.0000)	(0.1499)	(0.0994)
2003	0.0000	-0.0000†	0.0293	0.0217†	0.0000	0.0000†	0.0670†	0.0787†	0.0000	-0.0000†	0.0352†	0.0264†	0.0000	-0.0000†	0.0605†	0.0284†
		(0.0000)	(0.0843)	(0.0848)		(0.0000)	(0.0867)	(0.0919)		(0.0000)	(0.0775)	(0.1416)		(0.0000)	(0.0799)	(0.0914)
2004	0.0000	0.0000†	0.0069	0.0702†	0.0000	0.0000†	0.0199†	0.0524†	0.0000	-0.0000†	0.0086†	0.1303†	-0.0000	-0.0000†	-0.0500†	0.0895†
		(0.0000)	(0.0876)	(0.0975)		(0.0000)	(0.0630)	(0.0968)		(0.0000)	(0.1038)	(0.1333)		(0.0000)	(0.0939)	(0.1212)
2009	0.0000	-0.0000†	0.0117†	-0.0099†	—	—	—	—	0.0000	-0.0000†	0.0021†	-0.0061†	—	—	—	—
		(0.0000)	(0.0346)	(0.0336)						(0.0000)	(0.0329)	(0.0324)				
2010	0.0000	-0.0000†	0.0239†	-0.1006**†	—	—	—	—	0.0000	0.0000†	0.0530†	0.0518†	—	—	—	—
		(0.0000)	(0.0498)	(0.0421)						(0.0000)	(0.0488)	(0.0560)				
Weighted Averages																
All Estimates	0.0000[1]	-0.0000	0.0121	-0.0361	-0.0000[1]	0.0000	0.0128	0.0103	0.0000[1]	-0.0000	0.0172	0.0113	0.0000[1]	-0.0000	0.0044	0.0297
	(0.0000)	(0.0000)	(0.0255)	(0.0235)	(0.0000)	(0.0000)	(0.0491)	(0.0582)	(0.0000)	(0.0000)	(0.0246)	(0.0264)	(0.0000)	(0.0000)	(0.0564)	(0.0588)
Excl. Estimates Sensitive to BW	0.0000[3]	—	0.0185	—	—	—	—	—	—	—	—	—	—	—	—	-0.0090
	(0.0000)		(0.0607)													(0.0994)
p-value From Test That Individual Year Coefficients are Different from the Mean Across Years																
All Estimates	0.4565[4]	0.9735	0.8395	0.2983	0.0002[4]	0.9229	0.2006	0.2241	0.8201[4]	0.8040	0.9023	0.7573	0.2224[4]	0.5090	0.6122	0.8205
Excl. Estimates Sensitive to BW	0.7746[6]	—	0.8535	—	—	—	—	—	—	—	—	—	—	—	—	—

Notes: Kinks estimated with local linear regressions and uniform kernel. Estimates in the Micro columns were estimated on individual student micro data. Estimates in Collapsed columns were estimated on data collapsed in $100 bins of family income less taxes paid. All estimates with an absolute value less than one are rounded to the fourth decimal place. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Conventional standard errors are in parentheses. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

† Estimate is sensitive to choice of bandwidth: estimates using between 50-150% of the chosen bandwidth differ by more than one standard error from the main estimate.

1. Averages weighted by inverse of the variance of the Collapsed estimates.
2. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
3. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.
4. χ^2-statistic estimated using standard error of the Collapsed estimates.
5. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
6. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.

Table A.13: IV Estimate of Effect of $1000 of Pell Eligibility on Employment Outcomes, Dependent Students, All Years

	Positive Earnings in 2010				Log Earnings in 2010				Positive Earnings in 2012				Log Earnings in 2012			
	Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates	
Year	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed
2002	0.0039	0.0052	0.0188	0.0118	0.0183	0.0379	0.0879	0.0982	0.0024	0.0022†	0.0115	0.0105†	0.0162	0.0193	0.0779	0.1863
	(0.0037)	(0.0037)		(0.0196)	(0.0387)	(0.0384)		(0.1916)	(0.0041)	(0.0040)		(0.0189)	(0.0421)	(0.0391)		(0.1845)
2003	0.0032†	0.0022†	0.0143	0.0065†	0.0189†	-0.0074†	0.0851	0.0580†	-0.0063*†	-0.0070*	-0.0283	-0.0467**†	-0.0531†	-0.0877**†	-0.2391	-0.4492**†
	(0.0041)	(0.0042)		(0.0207)	(0.0422)	(0.0442)		(0.1964)	(0.0038)	(0.0038)		(0.0192)	(0.0427)	(0.0420)		(0.2034)
2004	0.0000	-0.0010	0.0001	-0.0011	-0.0088	0.0102	-0.0314	-0.0489	0.0060*†	0.0054†	0.0213	0.0048†	0.0056	0.0358†	0.0201	-0.0567†
	(0.0036)	(0.0035)		(0.0159)	(0.0325)	(0.0311)		(0.1679)	(0.0033)	(0.0036)		(0.0146)	(0.0375)	(0.0333)		(0.1410)
2005	-0.0069**†	-0.0038†	-0.0277	-0.0105†	-0.0157†	-0.0187†	-0.0632	-0.1490	-0.0013†	0.0016	-0.0053	-0.0005†	0.0063†	0.0316	0.0255	0.1619†
	(0.0034)	(0.0033)		(0.0128)	(0.0301)	(0.0297)		(0.1334)	(0.0040)	(0.0032)		(0.0132)	(0.0360)	(0.0348)		(0.1344)
2006	-0.0072**†	-0.0035†	-0.0254	-0.0304**†	-0.0733**†	-0.0291†	-0.2584	-0.3265**†	-0.0037	-0.0022	-0.0130	-0.0073†	-0.0462	-0.0208†	-0.1629	-0.1252†
	(0.0033)	(0.0036)		(0.0146)	(0.0309)	(0.0321)		(0.1310)	(0.0028)	(0.0026)		(0.0100)	(0.0300)	(0.0289)		(0.1069)
Weighted Averages																
All Estimates	-0.0020	-0.0005	-0.0104[1]	-0.0083	-0.0184	-0.0040	-0.0745[1]	-0.1229*	-0.0008	-0.0002	-0.0040[1]	-0.0058	-0.0168	-0.0009	-0.0621[1]	-0.0414
	(0.0016)	(0.0016)	(0.0071)	(0.0071)	(0.0152)	(0.0152)	(0.0702)	(0.0702)	(0.0016)	(0.0015)	(0.0062)	(0.0062)	(0.0164)	(0.0155)	(0.0637)	(0.0637)
Excl. Estimates Sensitive to BW	0.0020	0.0019	0.0076[2]	0.0040	0.0024	0.0212	0.0204[2]	-0.0625	-0.0018	-0.0021	-0.0076[2]	—	-0.0160	0.0262	-0.0655[2]	0.1863
	(0.0026)	(0.0025)	(0.0123)	(0.0123)	(0.0249)	(0.0242)	(0.1263)	(0.0917)	(0.0023)	(0.0018)	(0.0089)		(0.0205)	(0.0260)	(0.0773)	(0.1845)
p-value From Test That Individual Year Coefficients are Different from the Mean Across Years																
All Estimates	0.0582	0.3393	0.1364[4]	0.3955	0.2936	0.6885	0.4896[4]	0.3052	0.0925	0.1418	0.1920[4]	0.2007	0.5460	0.1311	0.5602[4]	0.0762
Excl. Estimates Sensitive to BW	0.4524	0.2273	0.4580[5]	0.6070	0.5911	0.5763	0.6393[5]	0.5680	0.2196	0.2213	0.2518[5]	—	0.3805	0.8150	0.4062[5]	—

Notes: Discontinuities estimated with local linear regressions and uniform kernel. Analyses exclude observations with family income at a multiple of $1,000. Estimates in the Micro columns were estimated on individual student micro data. Estimates in Collapsed columns were estimated on data collapsed in $100 bins of family income. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Conventional standard errors are in parentheses. Pell eligibility amounts are in 2012 dollars.

*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

† Estimate is sensitive to choice of bandwidth: estimates using between 50-150% of the chosen bandwidth differ by more than one standard error from the main estimate.

1. Averages weighted by inverse of the variance of the Collapsed estimates.
2. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
3. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.
4. χ^2-statistic estimated using standard error of the Collapsed estimates.
5. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
6. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.

Table A.14: IV Estimate of Effect of $1000 of Pell Eligibility on Employment Outcomes, Independent Students With Depenents, All Years

	Positive Earnings in 2010				Log Earnings in 2010				Positive Earnings in 2012				Log Earnings in 2012			
	Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates		Reduced Form Discontinuities		IV Estimates	
Year	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed
2002	0.0018†	0.0013†	0.0437	-0.0566†	0.0333	0.0480†	0.7928	0.9720	0.0056	0.0044	0.1339	0.1708	0.0748	0.0595	1.78	1.83
	(0.0046)	(0.0042)		(0.1300)	(0.0445)	(0.0425)		(1.4287)	(0.0046)	(0.0053)		(0.1575)	(0.0467)	(0.0502)		(1.57)
2003	-0.0006	-0.0012†	-0.0151	-0.0205	-0.0070	-0.0370†	-0.1673	-0.0977	0.0007†	0.0009	0.0161	0.1931†	-0.0048	-0.0340†	-0.1160	-0.3180
	(0.0047)	(0.0042)		(0.1207)	(0.0457)	(0.0458)		(1.3009)	(0.0048)	(0.0047)		(0.1583)	(0.0468)	(0.0466)		(1.4984)
2004	0.0085**†	0.0069**†	0.1341	0.1118*†	0.0707*†	0.0602†	1.12	1.06*†	0.0095**†	0.0088***†	0.1505	0.1449**†	0.0723*	0.0769**†	1.15	0.8103
	(0.0042)	(0.0034)		(0.0600)	(0.0415)	(0.0379)		(0.61)	(0.0043)	(0.0033)		(0.0578)	(0.0427)	(0.0389)		(0.6566)
2005	0.0053†	0.0068†	0.0807	0.1169	0.0360†	0.0378†	0.5497	0.8378	-0.0010†	-0.0024	-0.0147	-0.0524	-0.0142	-0.0366	-0.2167	-0.7544
	(0.0039)	(0.0046)		(0.0737)	(0.0375)	(0.0414)		(0.7545)	(0.0037)	(0.0039)		(0.0709)	(0.0367)	(0.0392)		(0.7208)
2006	-0.0019†	-0.0005†	-0.0209	-0.0785*†	-0.0033†	0.0093†	-0.0369	-0.3881†	0.0006	-0.0002	0.0067	0.0091	0.0267†	0.0179	0.2997	0.3655
	(0.0034)	(0.0033)		(0.0467)	(0.0329)	(0.0325)		(0.4623)	(0.0038)	(0.0037)		(0.0558)	(0.0383)	(0.0360)		(0.5537)
Weighted Averages																
All Estimates	0.0024	0.0026	0.0420[1]	0.0113	0.0241	0.0251	0.3987[1]	0.2839	0.0027	0.0027	0.0560*[1]	0.0561*	0.0275	0.0165	0.4653[1]	0.2660
	(0.0018)	(0.0017)	(0.0309)	(0.0309)	(0.0177)	(0.0175)	(0.3136)	(0.3136)	(0.0019)	(0.0018)	(0.0333)	(0.0333)	(0.0186)	(0.0184)	(0.3459)	(0.3459)
Excl. Estimates Sensitive to BW	-0.0006	—	-0.0151[2]	0.0796	0.0137	—	0.2679[2]	0.6662	0.0026	0.0001	0.0209[2]	-0.0011	0.0277	0.0074	0.5713[2]	0.2660
	(0.0047)		(0.1207)	(0.0629)	(0.0319)		(0.9619)	(0.5937)	(0.0029)	(0.0021)	(0.0526)	(0.0422)	(0.0213)	(0.0234)	(0.4429)	(0.3459)
p-value From Test That Individual Year Coefficients are Different from the Mean Across Years																
All Estimates	0.3230	0.3729	0.3237[4]	0.0640	0.6334	0.5066	0.6315[4]	0.3346	0.3608	0.2013	0.3130[4]	0.1531	0.4200	0.1878	0.5729[4]	0.4235
Excl. Estimates Sensitive to BW	—	—	—	0.3312	0.5277	—	0.6193[5]	0.8016	0.3950	0.7780	0.4463[5]	0.4173	0.2728	0.2976	0.4295[5]	0.4235

Notes: Discontinuities estimated with local linear regressions and uniform kernel. Analyses exclude observations with family income at a multiple of $1,000. Estimates in the Micro columns were estimated on individual student micro data. Estimates in Collapsed columns were estimated on data collapsed in $100 bins of family income. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Conventional standard errors are in parentheses. Pell eligibility amounts are in 2012 dollars.

*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

† Estimate is sensitive to choice of bandwidth: estimates using between 50-150% of the chosen bandwidth differ by more than one standard error from the main estimate.

1. Averages weighted by inverse of the variance of the Collapsed estimates.
2. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
3. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.
4. χ^2-statistic estimated using standard error of the Collapsed estimates.
5. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
6. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.

Table A.15: IV Estimate of Effect of $1000 of Pell Eligibility on Employment Outcomes, Independent Students Without Depenents, All Years

	Positive Earnings in 2010				Log Earnings in 2010				Positive Earnings in 2012				Log Earnings in 2012			
	Reduced Form Kinks		IV Estimates		Reduced Form Kinks		IV Estimates		Reduced Form Kinks		IV Estimates		Reduced Form Kinks		IV Estimates	
Year	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed	Micro	Collapsed
2002	0.0000	0.0000†	0.1317†	0.0828†	0.0003	0.0001†	1.21†	1.34*†	0.0000	0.0000†	0.0465†	0.0193†	0.0002	0.0000†	0.6734	0.1301†
		(0.0000)	(0.0864)	(0.0615)		(0.0002)	(1.04)	(0.76)		(0.0000)	(0.1026)	(0.0533)		(0.0001)	(0.9435)	(0.6021)
2003	-0.0000	0.0000†	-0.0093†	-0.0404†	-0.0000	0.0000†	-0.1418	0.1081†	-0.0000	0.0000†	-0.0027†	0.0151†	-0.0002	0.0000†	-0.5633†	-0.1174†
		(0.0000)	(0.0239)	(0.0367)		(0.0001)	(0.3970)	(0.2924)		(0.0000)	(0.0751)	(0.0247)		(0.0001)	(0.6944)	(0.3468)
2004	0.0000	0.0000†	0.0377	0.0463†	0.0001	-0.0000†	0.3033†	0.0809†	-0.0000	-0.0000*†	-0.0407†	-0.0024†	-0.0001	-0.0002†	-0.3081†	-0.1896†
		(0.0000)	(0.0725)	(0.0599)		(0.0001)	(0.7147)	(0.4459)		(0.0000)	(0.0755)	(0.0556)		(0.0001)	(0.7673)	(0.3558)
Weighted Averages																
All Estimates	0.0000**[1]	0.0000	0.0041	0.0038	0.0001[1]	0.0000	0.0884	0.2165	0.0000[1]	0.0000	-0.0071	0.0133	0.0000[1]	-0.0000	-0.1910	-0.1115
	(0.0000)	(0.0000)	(0.0220)	(0.0279)	(0.0001)	(0.0001)	(0.3292)	(0.2327)	(0.0000)	(0.0000)	(0.0473)	(0.0208)	(0.0001)	(0.0001)	(0.4520)	(0.2296)
Excl. Estimates Sensitive to BW	0.0000[3]	—	0.0377	—	-0.0000[3]	—	-0.1418	—	—	—	—	—	0.0002**[3]	—	0.6734	—
	(0.0000)		(0.0725)		(0.0001)		(0.3970)						(0.0001)		(0.9435)	
p-value From Test That Individual Year Coefficients are Different from the Mean Across Years																
All Estimates	0.0425[4]	0.5866	0.2580	0.1650	0.3608[4]	0.8852	0.4498	0.2987	0.3021[4]	0.1292	0.7890	0.9524	0.0302[4]	0.2937	0.5627	0.9005
Excl. Estimates Sensitive to BW	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—

Notes: Kinks estimated with local linear regressions and uniform kernel. Estimates in the Micro columns were estimated on individual student micro data. Estimates in Collapsed columns were estimated on data collapsed in $100 bins of family income less taxes paid. All estimates with an absolute value less than one are rounded to the fourth decimal place. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Conventional standard errors are in parentheses.
*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.
† Estimate is sensitive to choice of bandwidth: estimates using between 50-150% of the chosen bandwidth differ by more than one standard error from the main estimate.
1. Averages weighted by inverse of the variance of the Collapsed estimates.
2. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
3. Averages weighted by inverse of the variance of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.
4. χ^2-statistic estimated using standard error of the Collapsed estimates.
5. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where reduced form estimates on individual micro data were sensitive to bandwidth.
6. χ^2-statistic estimated using standard error of the Collapsed estimates, excluding years where IV estimates on individual micro data were sensitive to bandwidth.

Table A.16: Average IV Estimate of Effect of $1000 of Pell Eligibility on Earnings, Individual Micro Data, By Student and School Characteristics

	Dependent Students				Indep. Students w/ Deps.			
Dependent Var. & Cohort	All Students	With Loans	Top 25% Inst. Med. Earnings	US News Top 200	All Students	With Loans	Top 25% Inst. Med. Earnings	US News Top 200
Log Earnings in 2010	-0.1019	-0.1667**	-0.2157*	-0.2909	0.2807	-0.1341	0.3167	-1.15
	(0.0627)	(0.0758)	(0.1123)	(0.1941)	(0.2727)	(0.3788)	(0.5038)	(2.77)
Log Earnings in 2012	-0.0846	-0.0753	-0.0131	-0.1721	0.2846	0.0570	0.4317	1.30
	(0.0675)	(0.0831)	(0.1054)	(0.1904)	(0.2950)	(0.4233)	(0.5051)	(2.94)

Notes: Table presents the average of individual year estimates, weighted by the inverse variance of each estimate. Averages include estimates for the 2002-2006 cohorts. The average for students with loans only includes estimates for 2002, 2003, 2005, and 2006. IV estimates are calculated as the reduced form effect of the AZ threshold divided by the discontinuity in Pell at the AZ threshold. Standard errors are calculated using the delta method, assuming no covariance between the reduced form and first stage estimates. For each year, discontinuities estimated with local linear regressions and uniform kernel on individual micro data. Observations with family income at a multiple of $ 1,000 are excluded. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Bandwidths range from 2,207.71 to 4,878.26 for dependent students; and 2,574.46 to 4,581.48 for independent students with dependents. Conventional standard errors are in parentheses.
*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table A.17: Average IV Estimate of Effect of $1000 of Pell on Earnings, Independent Students Without Dependents, Individual Micro Data, By Student Characteristics

Dependent Var. & Cohort	All	With Loans	Female	First Generation	Top 25% Inst. Med. Earn.
Log Earnings in 2010	0.0884	0.2801	0.3187	-0.4384	-0.2158
	(0.3292)	(0.5617)	(0.3401)	(0.4424)	(0.3395)
Log Earnings in 2012	-0.1910	0.0544	0.5003	-0.2795	-0.0870
	(0.4520)	(0.3440)	(0.4109)	(0.5609)	(0.4021)

Notes: Table presents the average of individual year estimates, weighted by the inverse variance of each estimate. Averages include estimates for the 2002, 2003, and 2004 cohorts. For each year, kinks estimated with local linear two stage least squares regressions and uniform kernel on individual micro data. Bandwidth chosen using bandwidth selector described in Calonico, Cattaneo, and Titiunik (2014). Bandwidths range from 712.36 to 1,435.08. Conventional standard errors are in parentheses.
*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table A.18: Average of Aid Components for Students Near the Automatic Zero EFC threshold, by Sector and Student Type, 2012

	All	Public 4-year	Public 2-year	Private 4-year	Prop. 4-year	Prop. 2-year	Prop. < 2-year
Dependent Students							
Pell Eligibility	5,275	5,180	5,331	5,175	5,286	5,348	5,299
	(861)	(903)	(751)	(1,065)	(849)	(811)	(872)
Pell Grant	4,075	4,632	3,708	4,791	3,905	4,126	4,153
	(1,786)	(1,526)	(1,880)	(1,401)	(1,818)	(1,710)	(1,776)
Inst. Grants	1,524	1,395	296	11,179	219	37	21
	(4,852)	(3,387)	(940)	(10,111)	(1,128)	(228)	(126)
State Grants	767	1,740	525	1,836	452	109	98
	(1,714)	(2,517)	(1,088)	(2,262)	(1,522)	(761)	(443)
Total Grants	7,173	8,810	4,880	20,598	5,234	4,869	4,499
	(7,211)	(6,201)	(2,906)	(12,647)	(3,761)	(2,795)	(1,874)
Total Loans	4,252	3,426	872	6,543	6,811	6,834	6,368
	(4,435)	(3,246)	(1,893)	(4,632)	(4,443)	(4,287)	(3,611)
Net Tuition	4,723	1,254	160	6,318	8,340	10,863	12,582
	(6,357)	(3,433)	(535)	(7,359)	(5,126)	(7,325)	(6,987)
Net Cost	13,462	10,466	5,881	17,923	19,106	20,637	21,312
	(9,441)	(6,347)	(3,754)	(9,453)	(8,491)	(9,024)	(8,975)
Observations	1,810	280	600	180	480	170	70
Independent Students with Dependents							
Pell Eligibility	415	406	390	546	420	549	16
	(952)	(1,085)	(844)	(1,198)	(1,002)	(854)	(49)
Pell Grant	265	206	197	226	312	483	0
	(737)	(595)	(655)	(643)	(848)	(798)	(0)
Inst. Grants	390	256	223	5,387	76	46	0
	(2,400)	(855)	(792)	(9,421)	(631)	(210)	(0)
State Grants	90	164	123	248	18	42	226
	(407)	(491)	(468)	(884)	(167)	(270)	(552)
Total Grants	923	799	631	5,944	689	761	226
	(2,717)	(1,615)	(1,264)	(9,402)	(1,660)	(1,434)	(552)
Total Loans	5,766	5,681	2,743	8,461	7,190	7,014	8,347
	(5,419)	(5,026)	(3,600)	(5,032)	(5,780)	(6,206)	(3,625)
Net Tuition	7,922	2,610	1,344	8,809	10,904	13,339	21,139
	(7,627)	(2,671)	(1,451)	(6,904)	(6,125)	(7,830)	(8,399)
Net Cost	17,424	11,798	9,926	18,463	21,113	22,391	31,652
	(9,853)	(6,063)	(5,037)	(10,049)	(8,709)	(9,521)	(10,106)
Observations	460	60	140	20	180	40	20

Notes: Data from the National Postsecondary Student Aid Study 2011-12 wave. Table presents average aid amounts for students with family incomes up to $5,000 below the automatic zero threshold. Observations are rounded to the nearest ten.

Table A.19: Reduced Form Estimate of Effect of Automatic Zero Threshold on Federal Aid Components, 2012

	Average	Dependents		Indep. w/ Deps	
	Near Thresh.	h	$.5h$	h	$.5h$
Pell eligibility	4,208	666.6	680	45.6	104.5
		(50.3)	(72.3)	(29.3)	(35.7)
Pell award	3,249	527.2	497.1	70	54.7
		(72.3)	(104.9)	(108.1)	(152.3)
Inst. Grants	2,025	-173.1	-536.7	24.3	121.5
		(219.9)	(310.5)	(60.8)	(80.80)
State grants	842	-85	-120.2	73.60	146.6
		(76.60)	(114.7)	(48.1)	(68.7)
Tot. Grants	7,263	72.90	-621.8	29.2	169.8
		(330.7)	(480.2)	(166)	(232.1)
Total Loans	4,171	106.4	220.1	-59	-13.5
		(176.9)	(250.5)	(294.9)	(421.8)
Net Tuition	4,863	11.2	79.80	98.7	681.4
		(289.2)	(402)	(367.8)	(513.7)
Net Price	13,875	-91.40	150.1	169.7	378.3
		(426.5)	(601.2)	(559.5)	(772.6)
λ		.1094	-.9145	.64	1.6241
		(.4954)	(.7215)	(3.6156)	(2.2344)

Notes: Data from the National Postsecondary Student Aid Study 2011-12 wave. Table shows the estimated discontinuity in aid components at the automatic zero threshold. Columns marked h use a bandwidth of \$15,000. Columns marked $.5h$ use a bandwidth of \$7,500. The average near the threshold is the average for students with family income up to \$5,000 greater than the automatic zero threshold. λ is the IV estimate of Pell eligibility's effect on Total Grant Aid.

B Details of State Grant Simulations

We attempted to model the grant programs for states with large undergraduate enrollment or large need-based grant programs. The states we examined were Arizona, California, Colorado, Florida, Georgia, Illinois, Indiana, Kentucky, Maryland, Massachusetts, Michigan, Minnesota, Missouri, New Jersey, New York, North Carolina, Ohio, Oklahoma, Pennsylvania, Tennessee, Texas, Virginia, Washington, and Wisconsin. In total, Fall 2016 undergraduate enrollment in these states accounted for about 80% of national undergraduate enrollment (National Center for Education Statistics 2018), and in 2016-2017 these states together distributed about 94% of all need-based state grant aid for undergraduates (National Association of State Student Grant & Aid Programs 2018). Of these states, we were only able to successfully simulate a subset of the state grant programs, generally because the state did not have a need-based grant program (Georgia); there was inadequate information on the state grant formula (New Jersey, North Carolina); or institutions had discretion as to the amount of aid awarded to students (Arizona, Colorado, Tennessee, Texas, and Virginia). In addition, for each state we focused only on the largest need-based grant program.

We briefly describe the details of the simulations for each state below. Most states have a baseline set of eligibility criteria relating to students' characteristics like citizenship (or Dreamer status), residency, prior education, and attendance status. In addition, grants may only be available at certain types of institutions. For some states, there is a list of eligible institutions while other states broadly limit grant availability to institutions in certain sectors. In our simulations for dependent students attending four-year public institutions, we ignore most of the restrictions related to students' characteristics, but we do account for how grants may vary by student type (dependent, independent with dependents, independent without dependents) and institution type. Packaging of financial aid offers can be quite complex (U.S. Department of Education 2018), and for simplicity we also ignore other potential sources of financial aid other than Pell grants. Another important point to note is that some of the programs are underfunded. Most states with limited funds seem to either decrease award amounts across the board or award grants on a first-come-first-served basis, but Maryland gives priority to students with higher financial need (lower EFC). We cannot model the latter two approaches but note that they might change our conclusions if they could be modeled.

In all simulations, we assume that the dependent student is seventeen years old and lives with a single parent and one other younger sibling not in college and that the dependent student's parent has no income from assets, only has income from wages, and does not itemize deductions. As noted in the main text, we impose a different AZ threshold for 2018-2019 in order to create a discontinuity in the Pell schedule. The size of the discontinuity varies across states due to different state tax allowances in the EFC formula.

California: Cal Grant A

Cal Grant A covers systemwide fees for University of California and Cal State schools or $9,084 for private not-for-profit and for-profit schools in 2018-2019. To be financially eligible, students must have family income below the maximum income and asset levels for their family size and student type. In addition, their unmet need— calculated as cost of attendance less expected family contribution— must exceed the maximum award amount by at least $1,500 (California Student Aid Commission 2016). The income ceiling for our example dependent student family with three people was $91,000 (California Student Aid Commission 2018). In the state grant simulation figures, we show the estimated Cal Grant A for a student attending a University of California school, assuming cost of attendance is $32,400 and systemwide tuition and fees are $12,570 (University of California Admissions 2018).

Florida: Florida Student Assistance Grant Program

The maximum award amount for the Florida Student Assistance Grant (FSAG) Program is set annually by the Florida legislature (Florida Office of Student Financial Assistance 2018). In 2018-2019, the maximum award for students attending a public institution was $2,610. The actual award amount is the lesser amount of unmet need or the maximum award, where unmet need is calculated as cost of education minus the sum of expected family contribution, Pell grants, and other sources of financial assistance. Students' EFC must also be below a maximum cutoff, which was $5,273 for students attending a public institution in 2015-2016, the latest year where we could find a published maximum (Florida Office of Student Financial Assistance 2015). In our simulations, we assume that the EFC cutoff remained the same in 2018-2019 and that the cost of attendance is $21,210, the cost of attendance for undergraduate students living on campus at the University of Florida in 2018-2019 (University of Florida Office for Student Financial Affairs 2018).

Illinois: Monetary Award Program

For each award year, the Illinois Student Assistance Commission (ISAC) releases a preliminary formula for the Monetary Award Program (MAP) and then recalculates awards in the summer before they are awarded once the budget is better known (Illinois Student Assistance Commission 2018). The general form of the eligibility calculation is student budget minus family contribution. The 2018-2019 formula calculates students' budgets as average tuition and fees at all institutions within a given sector in 2009-2010 plus a living allowance of $4,875, regardless of type of school attended. ISAC calculates a students' family contribution based on students' and parents' income and assets, with an income protection allowance and a deduction for taxes paid. Students are expected to contribute at least $1,800 annually to their education. The formula for the award also accounts for students' Pell eligibility, subtracting from the student budget 80% of the Pell grant amount that a student would have been eligible for in 2009-2010 given their EFC in order to calculate MAP eligibility. The effective maximum award in 2018-2019 is $4,869. For more details on the formula, see the 2018-2019 Monetary Award Program Recompute Annual Award Hand Calculation form.

Indiana: Frank O'Bannon Grant

The Frank O'Bannon Grant, also known as the Higher Education Award and the Freedom of Choice Grant, provides grants to students attending public, private, or proprietary schools (Indiana Code 2018). The base award amount is based on the type of school attended, EFC, and credits completed. Students are eligible for larger awards if they are "on-time", completing at least 30 credit hours per award year. There are also additional student performance incentives for academic performance, completing an associates degree, and completing additional credit hours. To calculate the grant amounts, we used the 2018-2019 Frank O'Bannon Grants schedule from the Indiana Commission for Higher Education, which gives award amounts by ranges of EFC. We graph the grant for full-time students, assuming no performance incentive bonuses.

Kentucky: College Access Program Grant

Undergraduate students enrolled at least half-time at a two-year or more program in Kentucky may be eligible for the College Access Program grant in 2018-2019 (Kentucky Higher Education Assistance Authority 2018). We modeled the CAP grant as the maximum award for which a student would be eligible, which is the lesser of either the maximum award or cost of attendance less EFC. The maximum award amount for 2018-2019 was $1,900 and the grant was only available to students with an EFC less than $5,486 (Kentucky Administrative Regulations 2018).

Maryland: Howard P. Rawlings Educational Assistance Grant

A student must attend a two-year or four-year college or university full time to be eligible for the Howard P. Rawlings Educational Assistance (EA) Grant (Maryland Higher Education Commission 2018). The general form of the financial need calculation is the difference between cost of attendance and the sum of EFC, a regional cost of living adjustment, some state scholarship awards, and Pell grants. The cost of living adjustment is based on the student's zip code and serves to decrease EFC for students who live in more expensive areas of the state, decreasing EFC by up to 17% for students in the Washington, D.C. area. For four-year schools, the final amount of the award is the lesser of either 40% of calculated financial need or the maximum award amount, which was $3,000 in 2018-2019 (Maryland Higher Education Commission 2017). For our simulations, we assume an EFC adjustment of 10% and use the average cost of attendance for four-year public schools in Maryland in 2017-2018 (U.S. Department of Education. Institute of Education Sciences, National Center for Education Statistics 2018).

Massachusetts: Massachusetts Assistance for Student Success Program (MASSGrant)

The MASSGrant program provides aid to undregraduate students in Massachusetts with EFC less than $5,486 (Massachusetts Office of Student Financial Assistance 2018b). The award amount is based on type of school attended and EFC. We used the 2018-2019 Final MASSGrant Payment Schedule to find the award amounts corresponding to different levels of EFC (Massachusetts Office of Student Financial Assistance 2018a).

Michigan: Michigan Competitive Scholarship

Undergraduate students attending eligible institutions can qualify for up to $1,000 from the Michigan Competitive Scholarship in 2018-2019 (MI Student Aid 2018). Initial eligibility for the award is calculated as the difference between cost of attendance and 125% of EFC (MI Student Aid 2017). We calculate the award amount as the lesser of $1,000 and this measure of initial eligibility. In our simulations, we use the average COA for Michigan schools in 2017-2018.

Minnesota: Minnesota State Grant Program

The basic formula for the Minnesota State Grant Program is 50% of cost of attendance minus the sum of adjusted EFC and Pell grants (Minnesota Office of Higher Education 2018a). The formula indicates that Minnesota expects students to contribute at least 50% of the cost of attendance, but the grant covers the remaining financial need after accounting for EFC and Pell grants. The EFC adjustment is 94% for dependent students, 50% for independent students without dependents, and 86% for independent students with dependents. In our simulations, we assume that the cost of attendance at public four-year institutions is $18,316, the average cost of attendance at a state university in 2018-2019 (Minnesota Office of Higher Education 2018b).

Missouri: Access Missouri Financial Assistance Program

The Access Missouri Financial Assistance Program provides full-time undergraduate students at Missouri institutions with grants of up to $2,850 (Missouri Department of Higher Education 2018). Students with an EFC less than $7,000 are eligible for the maximum award. Students' eligibility is decreased by 10% of the maximum award for each additional $500 their EFC is above $7,000, with a minimum award of $1,500 for students with an EFC less than $12,000 (Revised Statutes of Missouri 2018). We simulate the maximum award for which a student at a public four-year school would be eligible.

New York: New York Tuition Assistance Program

The New York Tuition Assistance Program (TAP) bases eligibility on New York net taxable income instead of EFC. New York net taxable income differs from federal Adjusted Gross Income by adding or subtracting several types of retirement, education, and bond income and subtracting the New York state standard or itemized deduction and dependent exemptions (New York State Department of Taxation and Finance 2017). The maximum award is $5,165, and eligibility is reduced according to a progressive schedule based on New York net taxable income (New York Education Law 2018).

Ohio: Ohio College Opportunity Grant

The Ohio College Opportunity Grant (OCOG) limits the grant to the difference between average tuition and fees for institutions in a sector and the sum of EFC and Pell grants. As a result, students attending community colleges and public regional campuses are not eligible for the award (Ohio Department of Higher Education 2018). Maximum award amounts vary by sector, with students at other public institutions eligible to receive grants of up to $1,500. To qualify for the OCOG, students must have an EFC of $2,190 or less and no more than $96,000 in household income. We simulate the maximum award for which students may be eligible as the lesser of either the maximum award amount for public main campuses or the average tuition for public four-year schools less the sum of EFC and Pell grants (Ohio Revised Code 2018).

Oklahoma: Oklahoma Tuition Aid Grant

Eligibility for the Oklahoma Tuition Aid Grant (OTAG) is based on EFC (OKcollegestart 2018). To be eligible for the award, students must have an EFC below $1,700 (Oklahoma State Regents for Higher Education 2018). The maximum award amount is the lesser of 75% of tuition and fees or $1,000. The actual award amount cannot exceed cost of attendance minus EFC. In our simulations we display the maximum award for which a student would be eligible, assuming average tuition and fees at public institutions in Oklahoma in 2017-2018.

Pennsylvania: Pennsylvania State Grant

The Pennsylvania State Grant calculates student college costs as the sum of tuition and fees, a $1,000 books and supplies allowance, and a $4,000 educational expense allowance, with a college cost cap of $32,000. Financial need is then college cost less EFC and Pell grants. Initial eligibility is a percentage of this need, where the percentage is based on ranges of EFC, with the lowest range having 50% of need met. Prior to 2008-2009, the award amount would have been the lesser of the initial eligibility or the maximum award for a student's college cost tier. However, since 2008-2009, the award has not been fully funded, so the an adjustment factor is applied to the award. In 2018-2019, this adjustment factor was .8772, meaning that the effective maximum award is $4,123 (Pennsylvania Higher Education Assistance Agency 2018).

Washington: Washington State Need Grant

Students with income at or below 70% of Washington's median family income are eligible for the State Need Grant (Washington Student Achievement Council 2018). The maximum award amounts vary by institution type. In our simulations we use the maximum award amount for the

University of Washington, which is $9,745 in 2018-2019. The actual award that students receive is a percentage of this maximum award based on the median family income range into which their family income falls. Family income is the adjusted gross income reported on the FAFSA (Washington Administrative Code 2010). Students with income at or below 50% of state median family income receive 100% of the maximum award while students with income between 66-70% of median family income receive 50% of the maximum award. Median family income cutoffs vary by family size.

Wisconsin: Wisconsin Grant

We simulate the Wisconsin Grant for University of Wisconsin students, which provides grants to students enrolled at least half-time (Wisconsin Higher Educational Aids Board 2018b). In 2018-2019, the grants were calculated as 41% of the difference between $6,900 and EFC, with a minimum grant of $779 and a maximum of $2,829 (Wisconsin Higher Educational Aids Board 2018a).

States Not Simulated

Arizona: Arizona Leveraging Educational Assistance Partnership

Arizona's largest need-based grant is the Arizona Leveraging Educational Assistance Partnership (AzLEAP), which awards students grants of up to $2,500 and is determined by individual institutions (Arizona Commission for Postsecondary Education 2018).

Colorado: Colorado Student Grant

The Colorado Student Grant is also mostly awarded based on policies set at the institution level although the state imposes stricter rules for how proprietary institutions can allocate grants to their students (Colorado Department of Higher Education 2017).

Georgia: Hope and Zell Miller Grants

Georgia does not have need-based grant programs but instead offers HOPE and Zell Miller grants and scholarships. The scholarships are available to students who graduate high school with at least a certain GPA while the grants are available to students who maintain at least a specific GPA at their postsecondary institution (Georgia Student Finance Commission 2018b). Award amounts vary by institution (Georgia Student Finance Commission 2018a).

New Jersey: New Jersey Tuition Aid Grant

Full-time undergraduate students in New Jersey can qualify for the New Jersey Tuition Aid Grant (TAG) (New Jersey Higher Education Student Assistance Authority 2018b). The amount of the grant is determined by the type of institution attended and a student's New Jersey Eligibility Index (NJEI) (New Jersey Higher Education Student Assistance Authority 2018a). The NJEI is the amount that New Jersey determines students are expected to contribute to their education costs and is based on information on the FAFSA (New Jersey Commission on Higher Education 2006). However, we were unable to find documentation of the actual formula for NJEI, and in communication with the New Jersey Higher Education Student Assistance Authority, we were informed that the formula is proprietary and not shared with the public.

North Carolina: University of North Carolina Need Based Grant

North Carolina's largest need-based grant program is the University of North Carolina (UNC) Need Based Grant, which is available to students attending UNC institutions. While many of North Carolina's other state grant programs base eligibility on federal EFC, the UNC Need Based Grant uses a proprietary formula to calculate eligibility (Lineberry and Albritton 2018). We were unable to find the eligibility formula, but the "Rules and Procedures for the UNC Need Based Grant" notes that the formula mainly uses information from the FAFSA and differs from the federal EFC calculation in several ways including using allowances from the Institutional Methodology of the College Board, treating income and assets differently, accounting for federal education tax credits a student should receive, and assuming that students must contribute at least $4,500 to their education annually.

Tennessee: Tennessee Student Assistance Award

The maximum award amounts under the Tennessee Student Assistance Award (TSAA) vary by institution type, with students at four-year public institutions eligible for up to $2,000 (Tennessee Higher Education Commission and Student Assistance Corporation 2018). Institutions again determine the actual TSAA award amount a student receives.

Texas: Toward EXcellence, Access and Success Grant Program

Students with EFC up to $5,609 are eligible for the Toward EXcellence, Access and Success

Grant Program (TEXAS Grant), but the amount of the grant is determined by the institution (Texas Higher Education Coordinating Board 2018). In 2018-2019, students attending a public university could receive up to $9,348 from the grant. The state requires that institutions cover any tuition and fees that are not covered by the TEXAS Grant with financial aid other than loans (Texas Administrative Code 2015).

Virginia: Virginia Commonwealth Award

The largest need-based grant program in Virginia is the Virginia Commonwealth Award, which is available to students attending a Virginia public two-year or four-year institution. The amount of the grant varies by institution and is limited to the amount of tuition and fees (State Council of Higher Education for Virginia 2017a). The Virginia Guaranteed Assistance Program (VGAP), which has stricter achievement requirements than the Commonwealth Award, can be received in tandem with the Commonwealth Award. The amount of the VGAP also varies by institution, but the neediest students receive at least the amount of tuition (State Council of Higher Education for Virginia 2017b)

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