Does Federal Financial Aid Affect College Enrollment? Evidence from Drug Offenders and the Higher Education Act of 1998

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Abstract

In 2001, amendments to the Higher Education Act made people convicted of drug offenses ineligible for Federal financial aid for up to two years prior to their conviction. Using rich data on educational outcomes and drug charges in the NLSY 1997, we show that this law change had a large negative impact on the college attendance of students with drug convictions. On average, the temporary ban on Federal financial aid increased the amount of time between high school graduation and college enrollment by about two years, and we also present suggestive evidence that affected students were less likely to ever enroll in college. Importantly, we find that the law did not deter young people from committing drug felonies nor did it substantively change the probability that high school students with drug convictions graduated from high school. In contrast to much of the existing research, we conclude that, for this high-risk group of students, eligibility for Federal financial aid strongly impacts college investment decisions.

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1. Introduction

Changes in the United States economy over the past several decades have led to historically high returns to obtaining a college degree (e.g., Autor, Katz and Kearney 2008). At the same time, because the cost of obtaining a college degree is high and growing, liquidity constrained students may under-invest in higher education. This concern is particularly salient for fragile populations, such as low-income families and students whose parents do not have a college education. In order to support lower-income students' ability to afford college, the Federal government provides financial assistance to these students, typically in the form of Pell Grants and Stafford Loans. Federal aid is quite generous – in the 2009-2010 school year alone, the U.S. government gave out over $42 billion in grant aid and over $100 billion in loan aid to college students (College Board 2011). A question of central concern is how this aid impacts the likelihood that high school graduates, in particular those with disadvantaged backgrounds, enroll in and graduate from college.

Although identifying how financial aid affects college investment decisions is important, due to the high cost of college and the large amount of money spent on student aid, obtaining credible estimates of the impact of Federal financial aid on college-going has proved difficult. Early surveys of college students suggested a positive relationship between student aid and college enrollment (Leslie and Brinkman 1988). However, quasi-experimental research on the introduction of the Pell Grant system in 1973 finds little effect of Federal financial aid on college enrollment (Hansen, 1983; Kane 1994).[1] Faced with these disparate results, education

[1] A noted exception to this characterization of the literature is Seftor and Turner (2002), who show that the introduction of the Pell Grant had a sizable impact on college enrollment among "non-traditional" students, who begin college in their early 20s and 30s.

researchers reviewing the literature typically conclude that the topic "deserves further study" (Heller 1997).

The lack of strong evidence that Federal financial aid affects college enrollment is in some ways surprising. Studies that examine state merit aid, or other student aid that does not operate through the typical Federal formula, tend to find large impacts of financial aid. Dynarski (2003) examined the 1982 revocation of SSI benefits for college-age children with deceased parents. Using a difference-in-difference methodology, she finds that each $1000 in SSI aid increased college enrollment by 3.6 percentage points. Dynarski (2000) studies the introduction of the Georgia HOPE scholarship, which was introduced in 1993 and provides free in-state tuition to all Georgia high school graduates with above a 3.0 grade point average. She shows that the introduction of this program led to an increase in college attendance of between 7 and 8 percentage points. Scott-Clayton (2011) examines a similar program in West Virginia. Using discontinuities around the GPA cutoff, she estimates that the provision of free tuition significantly increases postsecondary attainment. Dynarski (2008) also shows evidence that state merit aid programs increase collegiate attainment.

Of the many plausible explanations for why the Pell and Stafford programs seem to be less effective than other source of aid, two have gained significant attention. The first explanation is institutional. In order to receive a Pell grant or Stafford loan, students must fill out the Free Application for Federal Student Aid, or FAFSA. The 2010-2011 FAFSA consists of over 130 questions about assets and income, similar to the IRS 1040 form. Experimental studies have found that the effort required to fill out the FAFSA constitutes a substantial barrier to college enrollment (Bettinger et al. 2009).[2] Perversely, this barrier seems to disproportionately affect the most disadvantaged students, who are the primary targets of the programs (Dynarski and Scott-Clayton 2007).

The second explanation is methodological. By construction, cross-sectional variation in Federal student aid is correlated with differences across students in family finances. The amount of aid available to any particular student is therefore almost certainly correlated with unobserved factors that also affect the likelihood of investing in college. The 1973 introduction of the Pell Grant creates a compelling source of variation in aid eligibility, but it is not obvious that the estimates from the 1970s can be generalized to the current higher education environment.[3] A lack of exogenous variation across students in aid eligibility makes it difficult to credibly identify the effect of Federal student aid for college using the current Federal financial aid system.

This paper contributes to the financial aid literature by identifying the impact of a quasi-experimental change in financial aid eligibility for a particular group of disadvantaged students. The 1998 amendments to the Higher Education Act (HEA98) specified that, beginning in 2001, any student convicted of a drug offense was ineligible for Federal financial aid for one to two years post-conviction, depending on whether it was one's first conviction. We use this change in a difference-in-difference framework, comparing college attendance among those with and without drug convictions in the years surrounding the law change, using the 1997 National Longitudinal Survey of Youth. While not without limitations, this data set allows us to observe

[2] The private company Student Financial Aid Services.com charges $80 to $300 for FAFSA assistance. See http://www.fafsa.com/fafsa-services/pricing-packages.

[3] The College Board estimates that, in 2011 dollars, average college tuition in a 4-year public institution was $2,242 in 1981 and $10,144 at a private institution. In 2011, tuition and fees at these institutions had increased to $8,244 and $28,500, respectively (see http://trends.collegeboard.org/college_pricing/report_findings/indicator/884#f8006). Since the 1990s, the maximum Pell grant has been roughly equal to its original 1973 value relative to average tuition costs, but the number of people receiving Pell grants has increased over six fold since 1976 (http://www.finaid.org/educators/pellgrant.phtml).

detailed criminal history information as well as data on student characteristics, cognitive ability, and postsecondary investment. The rich individual level data, along with our quasi-experimental identification strategy, allow us to produce some of the first credible evidence of the causal impact of Federal financial aid, as it currently is experienced by high school students, on investment in college.

While the population affected by HEA98 is a select sample of disadvantaged students, this group is of particular policy interest. Since the law was passed, the fraction of adults with a criminal history has grown by over 20% (Guerino et al. 2010). At the same time, Federal and state policy has continued to evolve in a way that has deliberately excluded ex-felons from many sources of employment and social support, including federal contract work, federally funded housing assistance, TANF, and food stamps (GAO 2005). To the extent that these policies make it more difficult for those with criminal records to participate in the legitimate sector, they may increase the likelihood that these people return to illegal activity. The high social cost of crime makes any policy affecting outcomes for this high-risk group of individuals worth examining (Bushway and Sweeten 2007). In addition, it is not clear *ex ante* that the response of students with drug convictions to financial aid will differ systematically from the response of disadvantaged students more generally.

In contrast to the early research on Federal aid, we find that HEA98 significantly and substantially reduced the probability that students with drug convictions attended college immediately after graduating from high school. Further tests suggest this is directly related to the law change. Specifically, we estimate that students with drug convictions took an average of 28 additional months to enroll in college post-HEA98, which is statistically indistinguishable from the two year ineligibility period specified in the law.

The data indicate that students did not delay high school graduation in response to the eligibility change, and there is no evidence that the HEA98 deterred students from committing drug crimes. We also conduct a series of permutation tests to verify that our estimated effects are not driven by outliers. Our estimates are consistent with a small effect of the eligibility change on the probability students with drug convictions ever attend college or obtain a bachelor's degree (BA). However, these estimates are imprecise due to the small number of individuals with drug convictions who actually attend college. Overall, the largest impact of the law is on delayed entry into college: HEA98 created an involuntary "double gap year" for the most at-risk students of an at-risk group. Such delays reduce the returns to a college education, as the higher wages that accompany collegiate attainment are realized for fewer years.

The paper proceeds as follows: in the next section, we provide additional background on the HEA98 as well as long run trends in drug convictions. We then describe our data and analytic approach in section three. Section four presents results, and we conclude with a discussion in Section five.

2. The Higher Education Act of 1998 and Juvenile Drug Convictions

The main purpose of the HEA98 was to re-authorize the Higher Education Act of 1965, which was the original act that set up the current Federal financial aid system. In an apparent effort to dissuade high school students from engaging in drug-related activities, the HEA98 included a provision that restricts the Federal financial aid eligibility of students who have been convicted of drug-related offenses. The law states that:

"A student who has been convicted of any offense under any Federal or State law involving the possession or sale of a controlled substance shall not be eligible to receive any grant, loan, or work assistance under this title during the period beginning on the date of such conviction…"

The ineligibility period is one year for the first conviction, two years for the second, and an indefinite ban for a third offense. Students can regain eligibility earlier if they complete a drug rehabilitation program that includes unannounced drug tests. While passed in 1998, the provision was not enforced until 2001. Between 2001 and 2007, students were asked whether they have been convicted of possession or sale of illegal drugs in state or federal court. Failure to answer the drug conviction question makes one ineligible for financial aid (GAO 2005).[4]

The eligibility provision in the law does not distinguish between sale and possession convictions, nor does it distinguish between felony and misdemeanor convictions or State versus Federal convictions. *Any* drug offense makes one ineligible. It is important to note, however, that the Federal government in general, and in particular the Department of Education, lacks the ability to validate one's response to this question. Especially for a state-level drug conviction, it would be very difficult for the government to check whether each student does not have a drug conviction.[5] While it is illegal to lie on the FAFSA, some students may do so in order to obtain aid. Using the 2003-2004 National Postsecondary Student Aid Survey (NPSAS), the GAO (2005) reports that about 41,000 students were denied aid due to a drug-related conviction, which excludes those who do not apply because of the drug conviction. While a small proportion of the population, this provision impacts a non-trivial number of students, who as we show below, are drawn disproportionately from lower socioeconomic backgrounds.

Based on a sample of felony defendants convicted in state courts in 2000, roughly 35% of all felony convictions were for drug offenses. Roughly 53% of drug offenders were black, and 83%

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[4] From 2007 onward, students were only asked about convictions that occurred while they were receiving federal financial aid, meaning that all first-time applicants were eligible on this margin. Our period of analysis extends through 2003.

[5] Bushway et al. (2007) highlight the prevalence of both false negatives and false positives in most criminal background checks.

were male. For sake of comparison, 44% of violent offenders and 39% of property offenders in that year were black, and 91% and 75%, respectively, were male. The black and male students most likely to be affected by HEA98 are also the least likely to attend college, ceteris paribus. [6] Also important for our analysis is the fact that drug offenders are more likely than other felons to have pre-existing criminal records; 44% of those convicted of drug offenses had previously been convicted of a felony, compared to 39% of property offenders and 33% of violent offenders.[7]

Ex Ante, the net social cost of HEA98 is unclear for a number of reasons. First, it is unclear what, if any effect, this regulation has on college investment. While any increase in the cost of enrolling in college should reduce the probability that students go, existing research has failed to find large impacts of Federal aid on college attendance. Furthermore, even if the marginal impact of Federal aid on attendance was non-zero, the average treatment effect of HEA1998 may be particularly small, as those with drug convictions may be unlikely to attend college regardless of financial aid eligibility.[8]

Second, even if HEA98 did impact college enrollment, there may be both social costs and social benefits to this law. If HEA98 discouraged high-risk students from enrolling in college, any standard model of human capital formation and crime would predict that this would increase criminal behavior of affected students (Becker 1968, Lochner and Moretti 2003, Mocan et al. 2005). At the same time, to the extent that HEA98 increased the cost of engaging in drug related

[6] See Kane (1994) for trends in black-white differences in college enrollment and Bound, Lovenheim and Turner (2010) and Goldin, Katz, and Kuziemko (2006) for trends in male-female college enrollment. These papers show that women are more likely to attend college than men and that African American students are less likely to attend than are white students.
[7] See http://www.albany.edu/sourcebook/pdf/t553.pdf.
[8] People convicted of drug felonies are generally less civically engaged for reasons besides the conviction (Sweeten et al. 2009, Hjalmarsson and Lopez 2010).

activity, forward looking students may have been deterred by this law. From this deterrence standpoint, the law could have positive benefits to society as well.

The zero-tolerance approach of HEA98 was consistent with trends in federal drug policy at the time and was counter to state level movements lowering the expected cost of drug use. In 2000, Nevada, Colorado, and Hawaii became the seventh, eighth, and ninth states to decriminalize the possession of small amounts of marijuana for personal medical use,[9] which the U.S. Supreme Court ruled in 2001 was still an unlawful act *(USA v. Oakland Cannabis Buyers' Cooperative (OCBC) and Jeffrey Jones).* Neither the state laws nor the Supreme Court ruling had a clear impact on the likelihood that student with drug convictions attended college. These states also represent a very small fraction of our sample, and we demonstrate below that the likelihood of a drug conviction does not shift in 2001, which suggests that these state changes are unlikely to produce a bias in our estimates. The remainder of this paper examines empirically how HEA98 affected college investment and drug convictions in order to provide evidence on the empirical relevance of these costs and benefits of HEA98.

3. Estimating the Impact of HEA98 on College Attendance

3.1. Data

We estimate the impact of HEA98 on the college attendance of drug convicts using the NLSY97. This nationally representative sample of 12-24 year olds in 1997 contains self reported data on educational attainment, interaction with the criminal justice system, and a rich set of demographic characteristics. As in all nationally representative surveys, roughly 1% of respondents report being convicted of a drug offense, but the fact that we observe both

[9] See http://www.npr.org/2011/07/12/126137481/medical-marijuana-laws-a-state-by-state-comparison for a summary of state marijuana laws.

educational and criminal justice outcomes makes it uniquely suited to evaluate the HEA98.[10] However, we do make some minor adjustments to the data set to address issues of plausible effect size and endogenous selection into the "treatment."

Any impact of HEA98 on college enrollment may be statistically muted by the inclusion of NLSY97 respondents who, by construction, either will always or never attend college. We therefore trim the original NLSY97 sample to include only those respondents who were 12-18 in 1997, because those over the age of 18 likely already have made a decision about college entry. We also restrict the sample to high school graduates, because these are the students who are on the margin of college enrollment. While this restriction could bias our estimates if students with drug convictions are more or less likely to graduate from high school after HEA98, we show below that no such response is evident in the data.

Our identification strategy consists of comparing the change in college enrollment likelihoods of students convicted of drug offenses to those who were not before and after 2001. Determining treatment status thus is critical to accurately identifying any effects of this rule change on college attendance. Because of the three year delay in when HEA98 went into effect, students may have anticipated the 2001 change and altered their schooling investment decisions as a result. We therefore assign each student to a "synthetic cohort" based on the year and month of birth and then assign students to treatment status based on the year in which they turn 18.

[10] For example, in the 2003 Monitoring the Future, roughly 8% of high school seniors reported ever being arrested (http://www.albany.edu/sourcebook/pdf/t343.pdf). According to the Uniform Crime Reports, overall drug offense arrest rates were about 0.4% in this time period (http://bjs.ojp.usdoj.gov/content/pub/pdf/aus8009.pdf).

Students who turn 18 in the 2000-2001 school year or after are considered to be subject to the HEA98 provisions.[11]

Using the information of the type and dates of convictions we identify, for each student, whether or not he was convicted of a drug offense in the two years prior to the date of expected graduation, which is based on his synthetic cohort. For example, if a student is in the synthetic cohort expected to graduate in spring 2002, he is "treated" if he was convicted of a drug offense in any time between 2000 and 2002. About 1.2% of the high school graduates in NLSY97 have a drug conviction within two years of their predicted graduation date. Overall, there are 41 high school graduates with a drug conviction pre-2001 and 46 in the post-2001 period.

As discussed in Section 2, HEA98 prohibits drug offenders from obtaining financial aid for different time periods, depending on how many convictions they have. Given the small sample of students with drug convictions, we use the most expansive definition in order to maximize power. We therefore use a two-year window, which corresponds to one's second offense, because it catches the most students who are potentially affected by the restriction. To the extent that we mis-classify first offenders with convictions more than a year prior to their FAFSA application, our estimates will be biased towards zero.

The NLSY97 also contains a large amount of information about student academic and socioeconomic background that allows us to both test for and control for differential selection into drug convictions surrounding HEA98. In particular, all respondents are given the Armed Services Vocational Aptitude Battery (ASVAB), which is a cognitive skills test used by the military. It is widely used in empirical work to measure student cognitive ability (e.g., Belley and

[11] We also have assigned treatment status based on the expected year of graduation using one's grade in 1997, assuming each student does not skip or repeat a grade from the time of first observation. This method of assigning treatment status yields similar results, and these estimates are available upon request from the authors.

Lochner, 2007; Lovenheim and Reynolds, 2011a, Lovenheim and Reynolds, 2011b). We also construct measures of household composition in 1997 (single parent, two parents or other parental structure), the number of household members under 18, mother's age at 1st birth, mother's age at respondent's birth, Census region, urban status and household income in 1997. We control separately for mother's and father's educational attainment (less than high school, high school diploma, some college, college graduate) as well as for gender and race. Table 1 presents descriptive statistics for all controls used in the analysis, which we show separately by pre- and post-2001 as well as by drug conviction status.

3.2. Estimation Strategy

The estimation strategy we employ is to compare the change in the likelihood of college enrollment between drug offenders and non-offenders when HEA98 goes into effect in 2001. The central difference-in-difference model we estimate is as follows:

$$P(Atttend)_{it} = \alpha + \delta_1 Post_t + \delta_2 Convict_i + \beta(Post_t * Convict_i) + \vartheta X_{it} + \lambda(Post_t * X_{it}) + \gamma(Convict_i * X_{it}) + \varepsilon_{it} \qquad (1)$$

where *Attend* is an indicator equal to 1 if the student attends college within two years of high school graduation.[12] We examine enrollment within two years because of our use of a two-year window for convictions and in order to focus on "traditional" students who attend college directly after high school. The variable *Post* is an indicator for the 2001-2003 synthetic cohorts, *Convict* is an indicator for whether the student was convicted of a drug offense in the previous two years, and *X* is a vector of observable characteristics that were described in Section 3.1.

Recent work by Bound, Lovenheim and Turner (2010) and Bailey and Dynarsky (2011) demonstrate a changing relationship between gender, race, family income and college attendance

[12] College enrollment includes any enrollment in a two- of four-year school, whether public, private or non-profit.

over time. It also is plausible that the impact of having a drug conviction varies by socioeconomic status; being convicted of a drug offense may matter less for high performing students, or for those from high income families. We therefore interact all of our control variables in X_{it} with $Post_t$, and with $Convict_i$, allowing for a more flexible relationship between demographics and college attendance that may be correlated with treatment status. These interactions control for any secular shifts in the observable characteristics of student post-HEA98 and control for differences in the relationship between observable characteristics and college enrollment by drug conviction status.

The coefficient of interest in this model is β, which is the difference-in-difference estimate conditional on the observable characteristics as well as on those characteristics interacted with *Post* and *Convict* indicators. The central identifying assumption required to interpret β as causal is that the only reason for a change in the relative enrollment rates between drug offenders and non-offenders post-2001 is due to the financial aid restrictions in HEA98. The threats to identification come from the fact that HEA98 did not go into effect for three years after its passage and only applied to people convicted of drug felonies after 1999 (who would be filling out the FAFSA in 2001). This lag between the enactment and implementation could cause a potentially endogenous reduction in the fraction of students committing drug crimes and/or a strategic manipulation of when someone with a drug arrest was convicted.

We will address this selection problem in several different ways. First, any change in the types of students who are convicted of drug offenses when HEA98 goes into effect should become apparent in the extensive set of observable characteristics we observe. For example, if relatively higher-achieving students were less likely to have drug convictions post-2001, then we might estimate β to be negative, but this positive effect could be due to the fact that convicts post-2001 were less qualified for college and would not be due to less access to financial aid.

Table 1 presents descriptive statistics of the students in our sample by predicted graduation date and offender status. The final column of the table provides difference-in-difference estimates that test whether observable differences between drug offenders and non-offenders shifted post-2001. First, note that relative to their peers, students with drug convictions are disadvantaged on almost all margins – their parents have less education, their family incomes are lower, they are more likely to live in single-parent households, and they are more likely to live in urban areas. Those convicted of drug offenses also are much more likely to be male. However, as the D-D estimates show, we cannot reject the null hypothesis that there was no change in this difference surrounding the implementation of HEA98 in 2001. The difference-in-difference estimates typically are small relative to the underlying means, and in only one case is this estimate statistically significantly different from zero at the 5% level. Furthermore, the point estimates suggest drug offenders are becoming *more* advantaged over time rather than less relative to their non-offender counterparts. Drug offenders also are slightly more likely to be white over time. These differences will serve to bias our estimate of β towards zero in models in which these observable characteristics are not controlled for. To the extent that there are unobserved changes in the likelihood of enrolling for college that are correlated with these small demographic differences, it suggests our difference-in-difference model may understate the importance of financial aid for college enrollment.

<table 1 about here>

A common criticism of the NLSY97 is that item non-response for key demographic variables is prevalent. If the pattern of missing data is systematically related to our treatment variables, such non-response is problematic. In Table 2, we present group-level means and difference-in-difference estimates of the probability that missing parental education, income, ASVAB score, and mother's fertility history are related to the effective date of HEA98. As in Table 1, we find no evidence of a differential change in the probability that drug offenders and non-offenders do not report this information, and the estimated magnitude of the differences are small relative to the group means.[13]

<table 2 about here>

Another way to test for selection is to see whether there were differential pre-treatment trends in college enrollment between drug offenders and non-offenders. In particular, if college enrollment among drug offenders was declining over time, we could confound this secular decrease with the effect of the change in the financial aid rule. In Figure 1, we plot the fraction of people attending college within two years after they graduate from high school, again based on their predicted year of graduation. Students without drug convictions should not be affected by HEA98, and indeed there is no shift in college attendance among this group in 2001. Prior to 2001, there is a modest upward trend in the fraction of people with drug convictions that go to college. But after 2001, the first year that drug offenders were ineligible for federal aid, this trend reverses. Based on the graphical evidence, there is a roughly 12 percentage point (or 33%) drop in college attendance, with no recovery by 2003. Furthermore, the same drop is not present for those charged with a drug offense, as Figure 1 also demonstrates. In fact, college attendance rises slightly throughout our sample for drug offenders, while it declines precipitously among those convicted of drug offenses beginning in the 2001 cohort.

[13] Note that we include missing indicators in equation (1) interacted with conviction status and the *Post* dummy. While it is potentially problematic to include missing data indicators, the fact that data are missing at random with respect to treatment status suggests there should be little bias from handling missing data in this manner.

<figure 1 about here>

Together, Table 1 and Figure 1 show that any bias in our estimate of β must be due to a shift in the underlying likelihood of attending college among those convicted of drug offenses in the 2001-2003 cohorts relative to the previous cohorts in a manner that is (1) unrelated to the extensive characteristics we observe and (2) not forecasted by pre-treatment trends. One possible shift could be an endogenous change in the likelihood of engaging in drug-related behaviors due to the law. Indeed, the main argument for the drug provisions in HEA98 was to deter teens from such activities; by raising the cost of conviction, HEA98 may have deterred some individuals from engaging in drug crimes.

The marginal drug offender deterred by the changes in federal law, including HEA98, would arguably be more likely to attend college than someone who would engage in drug sales no matter what, and therefore such a deterrent effect could lead to a spurious negative correlation between HEA98 and college attendance. The same would be true if police officers, prosecutors, or judges were less likely to convict marginal drug offenders if it would impact their ability to receive Federal financial aid.

We can explicitly test for such a deterrent effect by estimating a slight modification of equation (1), where we analyze whether or not the effective date of HEA1998 changed the probability that an individual has a drug conviction or was charged with a drug offense. We limit this analysis to high school graduates, as these are the individuals included in our estimation of equation (1).

<Table 3 about here>

The results from this analysis are presented in Table 3. When we do not condition on observables, we actually find that high school graduates are almost 50% *more* likely to be convicted of a drug felony after HEA98 (column i) and are 30% more likely to be charged with a drug offense (column iii). This is exactly the opposite effect we would expect in any standard model of deterrence or compensating behavior on the part of prosecutors.[14] When we condition on demographic characteristics (columns ii and iv), the direction of the selection effect reverses, but the magnitude of the selection is less than 1/10 the sample mean and is not statistically different from zero at conventional levels. We therefore conclude that any contamination of our treatment effect due to endogenous compositional change is minimal at most. Table 3 also suggests that the law did not have the intended effect: drug crimes among prospective college students were not reduced.

Thus, there is little evidence of a demographic shift in the characteristics of drug offenders surrounding HEA98. Visually, there is a sharp dropoff in college attendance among offenders when the financial aid provisions of HEA98 come into effect that is not forecasted by pre-treatment trends. And, there is no evidence of an endogenous reduction in the likelihood of getting charged or convicted of a drug crime due to the law. These findings give us confidence that our estimate of β is identifying the causal estimate of the financial aid restrictions embedded in HEA98.

4. Results

Table 4 presents our central difference-in-difference estimates of the effect of HEA98 on college enrollment. In column (i), we do not condition our estimates on demographic changes, essentially replicating estimate in the first row of Table 1, and we find a statistically imprecise 12 percentage point reduction in the probability that drug offenders enrolled in college after 2001. While not precise, this is a large change relative to the pre-2001 attendance rate among drug

[14] However, it is consistent with Pfaff (2011), who argues that a large fraction of the increased prison population is due to prosecutors filing more felony charges, conditional on arrest.

offenders (36%), and it also is large relative to the 27 percentage point difference in the college attendance rate across convicts and their peers (see Table 1). While suggestive of a large impact of financial aid on two-year enrollment, the small number of convicts renders this estimate rather imprecise.

We next add in controls for observable characteristics and their interactions with the treatment indicators sequentially. In column (ii), we include our basic controls for demographic characteristics. Variation in the composition of high school graduates explains much of the trend in attendance rates over time and slightly mitigates the differences in college attendance for offenders and non offenders. Conditional on demographics, we estimate that excluding drug offenders from aid eligibility reduces the probability they enrolled in college within two years of graduating high school by 16 percentage points, which is statistically different from zero at the 10% level. This is a 44% decline relative to the baseline of 36%.

<Table 4 about here>

Finally, in column (iii), we show our preferred specification, in which we relax our constraints on the relationship between individual characteristics and college enrollment over time and across groups. In contrast to the existing literature that finds negligible effects of FAFSA-based Federal aid on college enrollment, we estimate that when drug felons were excluded from Federal aid, there was a 22 percentage point (or 61%) reduction in the probability that they attended college immediately after high school. We can reject the null hypothesis that this effect is different from zero at the 5% level, but given our small sample sizes the standard error bounds still encompass small and very large effects. Given the lack of findings in previous work that Federal financial aid affects college-going, this estimate is almost incredibly large. Based on college attendance in the NLSY97, denying drug offenders Pell grants and Stafford loans may have been functionally equivalent to preventing them from attending college for at least two years after graduating from high school.

Because our college attendance estimate examines two-year enrollment, it may confound the effects of HEA98 on delayed attendance and on ever attending college. In Table 5, we exploit the richness of the education data in the NLYS97 to better understand how HEA98 affected drug offenders. We include the full set of demographic controls and their interactions with $Conviction_i$ and $Post_t$ in all reported specifications.

<Table 5 about here>

In column (i), we show that, while HEA98 prevented drug offenders from attending college immediately after high school, there is weaker evidence that a temporary restriction on Federal aid permanently reduced the chances that someone attended college.[15] Drug offenders were 8 percentage points less likely to ever attend college, but this point estimate is smaller than the standard error and about 1/5 of the underlying mean likelihood of ever attending college. This estimate is suggestive of a long-run effect, but given the wide confidence interval this result is not definitive. Consistent with a small long-run effect, we also find only suggestive evidence that HEA98 reduces the probability that high school students with drug offenses graduate from a four- year college (column ii). While our estimate is large relative to the baseline graduation rate pre-2001 among drug offenders, the fact that very few students who have drug convictions obtain a BA regardless of financial aid eligibility implies that we have a limited ability to detect any effect on college completion given our small sample size of drug offenders.

[15] The most recent year of NLSY97 data is 2009. So, by "ever attend college" we refer to the likelihood our respondents attend by 2009. In 2009, the youngest cohort is 25, and while college attendance among older students is rising, the vast majority of college students first attend college by their early 20s (Fitzpatrick and Turner 2007).

The HEA98 stipulated that after one to two years drug offenders would once again be eligible for Federal aid, depending on the number of past offenses. The temporary nature of the ban makes time to enrollment a natural outcome of interest. In column (iii), we estimate that, on average, after 2001, drug offenders delayed college enrollment by 28 months (se=8.01).[16] There is an 80% chance that drug offenders waited exactly 24 months, the amount of time HEA98 required them to wait before becoming eligible for aid, to enroll in college. Thus, it appears that the main effect of the law was to delay college entry, which creates potentially significant costs for students who have to wait several years longer to obtain the returns to a college education.[17]

Drug offenders may have strategically manipulated when they graduated from high school in anticipation of being temporarily ineligible for Federal aid. In column (iv) of Table 5, we estimate the amount of time it took each individual to complete high school and find some evidence consistent with strategic behavior. On average, students with drug offenses who are predicted to graduate after 2001 took almost 4 more months to complete high school. This effect is relatively small but is imprecisely estimated. At the same time, in column (v) we show that HEA98 did not reduce the long-run likelihood of ever graduating from high school. This is an important result, as it suggests that our decision to condition on high school graduation should not introduce a large bias to our estimates. Furthermore, these results imply that the vast majority of the increased delay in time to college enrollment among offenders in the 2001-2003 cohorts is due to waiting to enroll in college post-high school, rather than due to later high school graduation.

[16] Note that time between high school and college enrollment is measured as of the actual month of high school graduation, not of the predicted month of graduation based on one's synthetic cohort.
[17] While most of these students will not obtain a BA, there is ample evidence in the literature of sizable returns to sub-baccalaureate training (e.g., Kane and Rouse 1995; Andrews, Li and Lovenheim 2011; Jepsen, Troske and Coomes 2011).

< Figure 2 about here >

One potential limitation of our analysis is that, because of the small number of “treated” individuals, our estimates may be driven by a few outliers. In order to address this issue, we conducted a series of permutation tests. First, we re-estimated our preferred model, Table 4 column (iii), 46 times, excluding one treated individual each time. Panel A of Figure 2 presents all of estimates treatment effects from these regressions along with the bounds of the 95% confidence interval. All estimates are statistically different from zero at the 5% level and range from -0.24 to -0.18. We then systematically eliminate each possible pair of treated individuals from our sample. These 1,036 different regressions, presented in Panel B of Figure 2, produce average treatment effects ranging from -0.27 to -0.15, and 97.4% of them are statistically different from zero. Based on these tests, we conclude that, even with our limited sample, our results are unlikely to be statistical anomalies.

<Table 6 about here >

Finally, in order to verify that we are picking up the impact of the HEA98 financial aid restrictions rather than some unobserved shift in the composition of drug offenders or some contemporaneous unobserved shock, we conduct a series of falsification tests in which we vary our definition of “drug offenders.” Again, the richness of the NLYS97 allows us to differentiate people who interacted with the criminal justice system but did not in the specific way that would make them ineligible for Federal loans and grants.

First, in columns (i) and (ii) of Table 6, we replace $Convicted_i$ with $Charged_i$. Since our treated group now includes individuals who were arrested and charged with drug crimes in addition to those who ended up being convicted, we expect that our treatment effect will be attenuated. Indeed, this is what we find. Columns (i) and (ii) of Table 6 show that the impact of delayed eligibility falls by 75% under this definition, and it no longer is statistically significant at even the 10% level. In columns (iii) and (iv), we verify that the difference between column (ii) of Table 6 and column (iii) of Table 4 is driven by these unaffected drug users by restricting our treated group to those who are charged, but not convicted, of drug offenses. We estimate the HEA98 caused an imprecise 6 percentage point increase in the probability that these students attended college the year after high school when we control for observables. Thus, the effects we estimate in our baseline models are not driven by differential impacts of interaction with the criminal justice system post-HEA98 but are driven by the differential impact of being convicted, which is consistent with the structure of HEA98.

In columns (v) and (vi) of Table 6, we focus on students who were convicted of drug offenses at least three years before they were predicted to graduate high school. Consistent with the language of HEA98, we find no evidence that these students were any less likely to enroll in college after high school. If anything, students with older drug convictions are more likely to attend college after 2001, but this estimate is very imprecisely estimated. Overall, Table 6 shows that the effects we estimate in Tables 4 and 5 are due to the differential effects of having a drug conviction within two years in the 2001-2003 cohorts, which gives us some confidence our estimates are identifying the causal impact of HEA98 on college enrollment behavior because this is the group one would expect to be affected given how the law is written.

Finally, in the last two columns, we perform a final falsification test, exploring the impact of HEA98 on students convicted of non-drug offenses, such as violent crimes (e.g., assault or robbery), property offenses (e.g., burglary) and major driving offenses (e.g., drunk driving). While conviction for any of these offenses could result in incarceration, HEA98 did not affect the federal financial aid eligibility of these students. We estimate that, after the effective date of

HEA98, high school seniors convicted of serious crimes are roughly 7 percentage points less likely to attend college within two years of graduating high school, which is 1/3 of the change for drug offenders.

Recall that, in a given year, roughly 40% of adults convicted of a drug offense have been convicted of some sort of felony in the past. Since criminals, especially juveniles, tend to commit multiple types of crimes (Blumstein et. al 1988), it is not surprising that about 20% of students in our sample with a drug conviction also have been convicted of another crime. In the final column of Table 6, we control for both the presence of a drug conviction and a non-drug conviction, allowing us to better address this confounding issue. These controls reduce the magnitude of the statistically imprecise impact of non-drug convictions by almost half, implying that the larger “treatment effect” estimated in column (vii) was due to non-drug offenders also having drug convictions. Column (viii) shows little evidence of a shift in the likelihood of a student with a non-drug offense attending college in 2001, while the estimate for drug offenders still is large and is statistically significant at the 10% level. Thus, consistent with the provisions in HEA98, only students with drug convictions reduced college enrollment in 2001, suggesting this reduction was a response to lack of access to Federal financial aid rather than due to underlying trends in college participation among those with criminal convictions.

5. Conclusion

Due to the rising costs of college attendance and the growing importance of collegiate attainment for life outcomes, understanding how financial aid impacts postsecondary enrollment is of high importance. It is particularly important identify the role of financial aid for underprivileged students, who attend college at much lower rates than their wealthier counterparts. Students who are convicted of felony drug charges are arguably among the most at-risk students in America. While their performance on achievement tests is comparable to their peers, these student come from poorer families with non-traditional parental arrangements, and their parents are less likely to have attended college. Furthermore, being convicted of any felony offense at a young age is a strong predictor of future violent criminal behavior (Blumstein and Cohen 1987). The opportunity cost of allowing these young people to continue to fail, while difficult to quantify, is inarguably large.

Despite the importance of identifying the effect of financial aid in general and Federal financial aid in particular on college enrollment, the previous literature has not reached a consensus, due predominantly to the difficulty in generating exogenous variation in aid access. This paper exploits a rule change from the Higher Education Act of 1998 that temporarily eliminated Federal financial aid eligibility for students convicted of a drug offense in the previous two years. We show extensive evidence that this rule change created an exogenous decrease in financial aid eligibility for students with a drug conviction.

Using NLSY97 data on college enrollment, student socioeconomic and cognitive backgrounds and criminal histories, we employ a difference-in-difference methodology that examines how college enrollment among drug offenders relative to non-offenders changed surrounding the implementation of HEA98 in 2001, the first year this rule went into effect. We find evidence that the temporary prohibition on Federal aid caused a large decline in the fraction of drug offenders who enrolled in college within two years of graduating from high school. This decline was driven predominantly by elongating the time between high school and college; drug offenders on the margin of college enrollment simply waited to enroll until they were eligible again for aid. The effects on ever attending college (by 2009) and on BA completion are more modest. This elongation has costs, however, in the form of delaying the returns to collegiate attainment. In addition, we find no evidence that the law had a deterrent effect on drug offenses. Thus, by forcing drug offenders to wait two years before enrolling in college, HEA1998 lowered the lifetime earnings of these at-risk students without generating benefits to society through reduced crime.

While this paper identifies the effect of Federal financial aid on college enrollment among a distinct set of students – drug offenders – they are of high interest because they are from low socioeconomic backgrounds and because they are at-risk of committing more crime in the future. Attending college may be an important "turning point" in the lives of delinquent youths, on par with marriage or employment (Sampson and Laub 1990, Uggen 2000); education, in particular post-secondary education, is strongly correlated with desistance from crime (Nuttall et al. 2003, Johnson 2001, Clark 1991). By restricting access to financial aid, HEA98 may have inadvertently harmed the long-run life outcomes of these at-risk students. Indeed, in our sample students with drug convictions are 0.8 of a percentage point (se=0.5), or 60%, more likely to be convicted of another drug crime in the three years after high school graduation if they are subject to the HEA98 financial aid restrictions.

Despite the selected sample, this paper is the first in the literature to show evidence that modern Federal financial aid impacts college decisions, and there is little reason to believe low-income students more generally respond in a fundamentally different way to the availability of this aid. Given the importance of understanding how Federal financial aid impacts college-going decisions, more research on the impact of this aid on a more general group of students is needed.

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Table 1: Means and Standard Deviations of Analysis Variables

Variable	Pre-Change Conviction	Pre-Change No Conviction	Post-Change Conviction	Post-Change No Conviction	D-D
Attend College	0.623 (0.485)	0.358 (0.485)	0.651 (0.477)	0.269 (0.448)	-0.117 (0.104)
ASVAB (10,000 units)	5.390 (2.798)	5.027 (2.543)	5.435 (2.773)	5.753 (2.475)	0.681 (0.710)
Household Income ($10,000)	5.647 (4.599)	4.419 (2.635)	5.500 (4.558)	4.137 (2.707)	-0.134 (1.167)
Single Parent	0.262 (0.440)	0.397 (0.495)	0.241 (0.428)	0.367 (0.487)	-0.009 (0.094)
Two Parents	0.687 (0.464)	0.462 (0.505)	0.719 (0.450)	0.605 (0.494)	0.111 (0.099)
Other Parental Structure	0.051 (0.220)	0.141 (0.352)	0.040 (0.196)	0.028 (0.168)	-0.110** (0.045)
# HH Members < 18	2.219 (1.178)	2.207 (1.154)	2.424 (1.145)	2.360 (0.974)	-0.052 (0.250)
Urban	0.686 (0.464)	0.884 (0.325)	0.690 (0.463)	0.813 (0.394)	-0.074 (0.100)
Mom Age at 1^{st} Birth	23.35 (4.62)	23.86 (4.98)	23.76 (4.78)	24.17 (4.94)	-0.10 (1.06)
Mom Age at Respondent Birth	25.72 (5.38)	25.68 (4.83)	26.40 (5.30)	27.09 (5.36)	0.73 (1.21)
Northeast	0.188 (0.391)	0.301 (0.464)	0.183 (0.387)	0.231 (0.426)	-0.066 (0.084)
North Central	0.274 (0.446)	0.170 (0.380)	0.267 (0.443)	0.229 (0.425)	0.066 (0.096)
South	0.332 (0.471)	0.330 (0.476)	0.333 (0.450)	0.428 (0.500)	0.098 (0.101)
West	0.205 (0.404)	0.199 (0.404)	0.217 (0.412)	0.112 (0.319)	-0.098 (0.088)
Mother < High School	0.148 (0.356)	0.094 (0.295)	0.145 (0.392)	0.174 (0.384)	0.084 (0.080)
Mother High School Diploma	0.381 (0.486)	0.426 (0.502)	0.350 (0.477)	0.256 (0.442)	-0.139 (0.109)
Mother Some College	0.253 (0.435)	0.291 (0.460)	0.268 (0.443)	0.209 (0.412)	-0.097 (0.099)
Mother BA+	0.218 (0.413)	0.190 (0.398)	0.237 (0.425)	0.361 (0.486)	0.153 (0.095)
Father < High School	0.161 (0.368)	0.228 (0.427)	0.152 (0.359)	0.166 (0.378)	-0.053 (0.093)
Father High School Diploma	0.367 (0.482)	0.521 (0.509)	0.385 (0.487)	0.499 (0.508)	-0.041 (0.124)
Father Some College	0.202 (0.401)	0.170 (0.383)	0.201 (0.401)	0.197 (0.404)	0.027 (0.103)
Father BA+	0.271 (0.444)	0.080 (0.277)	0.261 (0.439)	0.138 (0.350)	0.067 (0.113)
Black	0.193 (0.400)	0.144 (0.351)	0.130 (0.340)	0.144 (0.351)	-0.064 (0.076)
Hispanic	0.156 (0.368)	0.121 (0.326)	0.072 (0.262)	0.117 (0.321)	-0.080 (0.070)
Male	0.811 (0.396)	0.503 (0.500)	0.819 (0.390)	0.498 (0.500)	0.013 (0.107)
Observations	3,958	41	3,356	46	7,401

All tabulations include only high school graduates. Standard deviations are in parentheses in the first four columns and standard errors are in parentheses in the D-D column: ** indicates statistical significance at the 5% level and * indicates statistical significance at the 10% level.

Table 2: Means and Standard Deviations of Missing Indicator Variables

	Pre-Change		Post-Change		
Variable	Conviction	No Conviction	Conviction	No Conviction	D-D
Mother's Education Missing	0.066	0.100	0.058	0.073	-0.018
	(0.249)	(0.303)	(0.233)	(0.263)	(0.052)
Father's Education Missing	0.156	0.283	0.142	0.245	-0.025
	(0.363)	(0.456)	(0.349)	(0.435)	(0.077)
Family Income Missing	0.260	0.250	0.222	0.306	0.094
	(0.438)	(0.438)	(0.416)	(0.466)	(0.092)
ASVAB Missing	0.172	0.293	0.163	0.252	-0.032
	(0.378)	(0.461)	(0.369)	(0.439)	(0.081)
Mom Age at 1^{st} Birth Missing	0.068	0.099	0.067	0.069	-0.030
	(0.252)	(0.303)	(0.251)	(0.256)	(0.054)
Mom Age at Respondent Birth Missing	0.058	0.099	0.054	0.069	-0.027
	(0.233)	(0.303)	(0.227)	(0.256)	(0.050)

All tabulations include only high school graduates. Standard deviations are in parentheses in the first four columns and standard errors are in parentheses in the D-D column: ** indicates statistical significance at the 5% level and * indicates statistical significance at the 10% level.

Table 3: Effect of The Financial Aid Policy Change on Drug Charges and Convictions

	I(Convicted)		I(Charged)	
Independent Variable	(i)	(ii)	(iii)	(iv)
Post-Change	0.005*	-0.001	0.007*	-0.001
	(0.003)	(0.007)	(0.004)	(0.010)
Observables	No	Yes	No	Yes
Dep. Var. Mean	0.012		0.023	

All estimates include only high school graduates. Standard errors are in parentheses: ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 4: Effect of Conviction on College Enrollment Surrounding Eligibility Policy Change

Independent Variable	(i)	(ii)	(iii)
Convicted	-0.266**	-0.180**	0.812**
	(0.083)	(0.069)	(0.452)
Post-Change	0.028**	0.005	-0.046
	(0.012)	(0.022)	(0.086)
Post*Convicted	-0.117	-0.160*	-0.220**
	(0.108)	(0.091)	(0.091)
R^2	0.006	0.235	0.242
Observables	No	Yes	Yes
Observables*Post	No	No	Yes
Observables*Convicted	No	No	Yes

All estimates include only high school graduates. Heteroskedasticity-robust standard errors are in parentheses: ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 5: Effect of Conviction on College and High School Outcomes Surrounding Eligibility Policy Change

Independent Variable	Ever Attend College (i)	BA (ii)	Time Between HS & Coll. (iii)	Time to HS Degree (iv)	HS Degree (v)
Convicted	0.928*	0.909**	52.50	16.26	-1.068**
	(0.473)	(0.317)	(32.06)	(15.53)	(0.319)
Post-Change	0.006	-0.002	5.86*	6.98**	-0.109*
	(0.083)	(0.079)	(3.40)	(1.78)	(0.060)
Post*Convicted	-0.080	-0.072	27.98**	3.82	0.075
	(0.105)	(0.051)	(8.01)	(2.79)	(0.068)
Pre-HEA98 Offender Mean	0.410	0.074	8.44	242.85	0.631

[1] The "Pre-HEA98 Offender Mean" is the mean of the drug convict sample pre-2001. All estimates except the final column include only high school graduates. The Time to HS Degree is enumerated by the number of months since January 1, 1980.

[2] Heteroskedasticity-robust standard errors are in parentheses: ** indicates significance at the 5% level and * indicates significance at the 10% level.

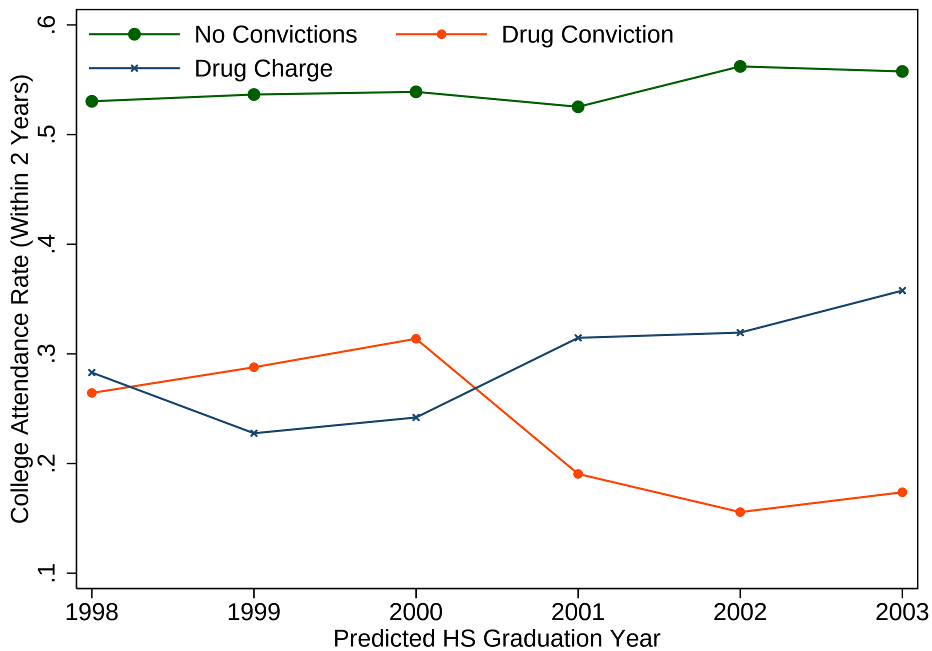
Table 6: Falsification Tests

Independent Variable	Charged in Last Two Years (i)	(ii)	Charged but Not Convicted (iii)	(iv)	Convicted Three or More Years Ago (v)	(vi)	Including Non-Drug Convictions (vii)	(viii)
Convicted/Charged	-0.220**	0.368	-0.176**	0.276	-0.621**	-0.376**	.	-0.091
	(0.061)	(0.325)	(0.085)	(0.432)	(0.008)	(0.083)	.	(0.075)
Post-Change	0.026**	-0.046	0.026**	-0.037	0.025**	-0.046	-0.006	-0.012
	(0.012)	(0.086)	(0.012)	(0.076)	(0.011)	(0.086)	(0.083)	(0.083)
Post*Convicted/Charged	0.015	-0.056	0.172	0.058	-0.025	0.137	.	-0.166*
	(0.083)	(0.076)	(0.117)	(0.102)	(0.304)	(0.103)	.	(0.100)
Non-Drug Conviction	.	.	.	.	.	.	0.035	0.007
	.	.	.	.	.	.	(0.224)	(0.224)
Post*Non-Drug Conviction	.	.	.	.	.	.	-0.069	-0.036
	.	.	.	.	.	.	(0.057)	(0.059)
Observables	No	Yes	No	Yes	No	Yes	Yes	Yes
Observables*Post	No	Yes	No	Yes	No	Yes	Yes	Yes
Observables*Convicted/Charged	No	Yes	No	Yes	No	Yes	Yes	Yes

[1] All estimates include only high school graduates. The estimates in columns (iii) and (iv) examine those who were charged with a drug offense in the past two years but who were not convicted. Estimates in columns (v) and (vi) examine those who have a drug conviction more than two years prior to the synthetic cohort graduation year. In the final two columns, we analyze non-drug offenses on the prior two years and drug and non-drug offenses simultaneously.

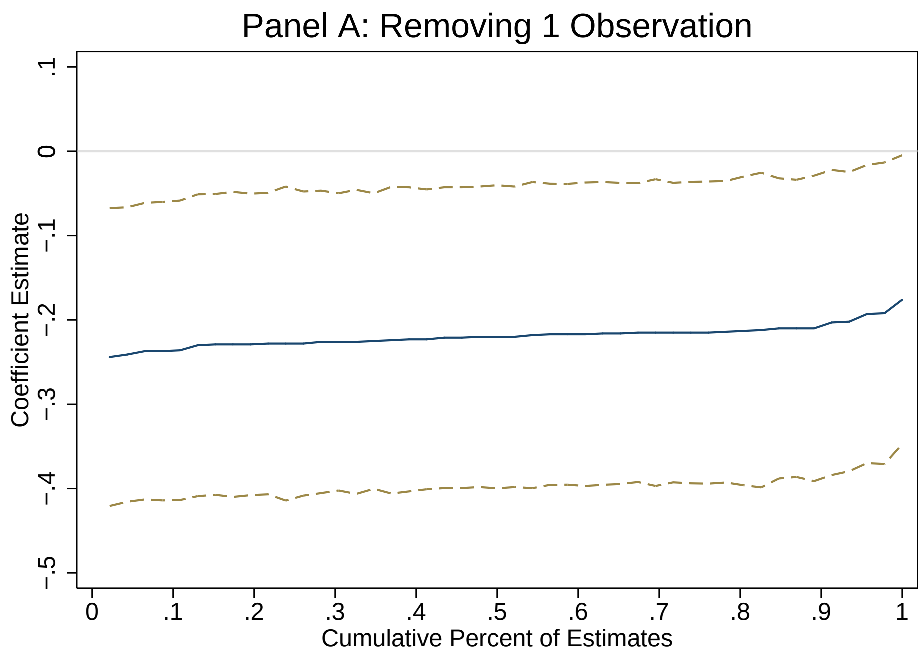
[2] Heteroskedasticity-robust standard errors are in parentheses: ** indicates significance at the 5% level and * indicates significance at the 10% level.

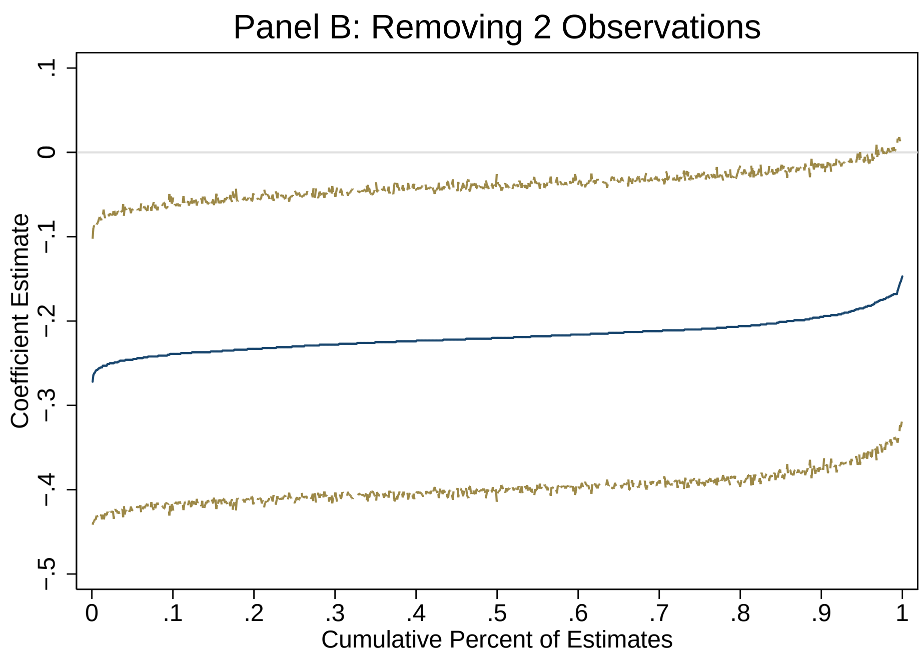
Figure 1: Trends in College Enrollment Rates by Conviction Status and High School Cohort



Source: Author's calculations from the 1997 National Longitudinal Survey of Youth as described in the text.

Figure 2: Permutation Tests





Source: Author's calculations from the 1997 National Longitudinal Survey of Youth as described in the text. In Panel A, we remove each of the 46 treated observations and re-estimate the model. The figure shows the inverse CDF of the resulting estimates with the bounds of the 95% confidence interval. In Panel B, we remove each pair of two treated estimates and plot the inverse CDF of resulting estimates along with the bounds of the 95% confidence interval.