Reversing the School to Prison Pipeline: The Impact of an Adult High School Program*

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Abstract

The criminal justice system disproportionately affects individuals without high school diplomas. This study estimates the causal effect of earning a diploma on criminal justice contact among adults who previously dropped out but then attended The Excel Center (TEC). Compared to applicants who do not enroll, those who enroll but don't graduate show no change in criminal charges. However, diploma earners see a 49% decline in charges in the year after applying, with effects lasting at least five years. Social benefits from reducing crime exceed the cost of the program. Earning a diploma, not just attending school, reduces criminal justice involvement.

JEL Classification: K42, I24, I26

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1 Introduction

Among people over age 25 in state and federal prisons in 2016, nearly 60 percent did not have at least a high school diploma upon entry,[1] while only 14 percent of adults over age 25 in the general US population lacked a diploma.[2] Low educational attainment among people in the criminal justice system at least partly results from the direct effect of education, and policies that prevent teenagers from dropping out of high school can reduce criminal justice contact (Lochner and Moretti, 2004; Bell et al., 2022).

However, many of the young adults who are at the highest risk of entering the criminal justice system have already exited school. Attending traditional schools may have even had negative effects for them (Jacob and Lefgren, 2003) via harmful interactions with peers (Carrell and Hoekstra, 2010; Billings and Phillips, 2017; Billings et al., 2019; Billings and Hoekstra, 2022) or school discipline (Bacher-Hicks et al., 2024). For these individuals, public policy primarily encourages GEDs as the path for further educational attainment, and this is especially true for individuals who become incarcerated. While no more than three percent of the general population has completed a GED (Heckman and LaFontaine, 2010), 23 percent of inmates have done so, with two-thirds completing the test while incarcerated (Harlow, 2003). Prior research about the GED shows that labor market returns to the credential are relatively disappointing (Heckman et al., 2011; Jepsen et al., 2016), with perhaps some benefits for people with the greatest labor market challenges (Murnane et al., 2000; Darolia et al., 2021). Without even much labor market benefit to the most common educational option, it is unclear if educational interventions for people who have already dropped out of high school can do much to reduce criminal justice system involvement.

We study a network of tuition-free charter schools that helps adults return to school and complete a traditional high school diploma. The Excel Center (TEC) is a network of high schools that originated out of Goodwill of Central and Southern Indiana and is led by Goodwill Education Initiatives. TEC helps adult students complete a standard high school diploma, including traditional

[1] https://spi-data.bjs.ojp.gov/dashboard

[2] https://www.census.gov/data/tables/2016/demo/education-attainment/cps-detailed-tables.html

coursework and graduation tests, by providing supports tailored specifically to adult students. The schools operate on shorter, intensive terms with course schedules built around the gaps in the student's prior experience and current knowledge. They provide additional support to adults through free, on-site childcare and life coaches who help with non-academic barriers. Prior research shows that, for the first set of TEC schools in Indiana, this alternative approach helps adults obtain a high school diploma and succeed in the labor market, closing half of the descriptive gap in earnings between people who graduate from versus drop out of high school (Brough et al., 2024).

This paper tests whether helping people who previously dropped out of high school complete a full high school diploma reduces their likelihood of contact with the criminal justice system. To measure the effect of a full high school diploma, we compare three groups of adults who apply to TEC: 'graduates' who complete the diploma; 'exiters' who enroll at TEC but do not finish; and 'non-enrollees' who apply but do not enroll. Because individuals in all three groups submitted an application to TEC, we implicitly control for selection into interest in further education. The three groups differ in levels of pre-period criminal justice contact, so we implement a 'doubly robust' difference-in-differences approach including individual fixed effects, year fixed effects, and inverse propensity weights based on demographics and pre-application case filings.[3]

Graduating with a diploma reduces contact with the criminal justice system. We measure outcomes for TEC applicants by connecting their records to administrative court filings from the State of Indiana. Across various specifications, graduates of TEC are less likely to have a court filing than non-enrollees. Graduates of TEC experience a 6.3 percentage point (49 percent) decline in the likelihood of having a court filing relative to non-enrollees in the year following application. Over half of this effect persists for an additional five years. This benefit results from graduating rather than enrolling; exiters experience small, statistically insignifcant declines in the year after application, but largely remain similar to non-enrollees. The persistent declines are spread across charges of various types and severity, ranging from criminal traffic charges to violent felonies, and

[3]Because we observe application date for all three groups we avoid the potential econometric issues raised in the recent difference-in-differences literature (Borusyak et al., 2024; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021) by analyzing the data in time relative to application.

are about equally split between charges that end in guilty versus not guilty dispositions. Treatment effects are typically larger for sub-groups at higher risk of justice involvement, such as people who are younger, identify as male, and who have past court filings.

Our identification strategy assumes that any selection into enrollment and graduation can be summarized by past court contact. If post-application criminal justice contact itself prevents graduation or if students select out of graduation due to post-application negative shocks (e.g. a family crisis) that are correlated with appearing in court, this strategy will measure treatment effects with bias. To test the robustness of our approach, we first show that we continue to detect large reductions in criminal justice system contact for a wide variety of specifications of the inverse propensity weights. Second, we provide a lower bound for treatment effects by estimating a specification that limits the comparison group to those exiting due to a known, positive shock (leaving for employment). Even this lower bound yields estimates that retain two-thirds of the effect of graduating. In additional robustness checks, we verify that the primary results for our main sample hold across different event window lengths, and conduct a placebo test to rule out spurious findings. Our results remain consistent when varying the pre-period and post-period lengths. For the placebo test, we randomly assign fake application dates to students within their actual pre-application period and apply the same estimation strategy. Across these placebo iterations, we find no significant effects for any outcome comparison (graduates vs. exiters, exiters vs. non-enrollees, and graduates vs. non-enrollees), supporting that our main results represent a genuine treatment effect of TEC.

Our results suggest that educational attainment itself can be a mechanism for reducing criminal justice system contact among adults. We observe an independent, persistent effect of completing a diploma, much larger than the effect of simply enrolling. This pattern of results suggests that education itself, rather than the direct incapacitation effect of being in class, leads to less criminal justice system involvement. What about a high school diploma drives crime reductions is more difficult to pin down. Although some of the effect of education likely results from the way in which human capital accumulation affects the opportunity cost of crime (Becker, 1968), the employment effects observed in Brough et al. (2024) are not large enough to account for the magnitude of effects we observe. Thus, education for adults likely reduces crime for a broad range of reasons, possibly including effects on preferences and identity that are hard to measure.

High school diplomas for adults provide particularly high returns for a group of people who would otherwise be at high risk of criminal justice system involvement. Despite the benefits of early childhood investments (Belfield et al., 2006), traditional education (Lochner and Moretti, 2004; Cook and Kang, 2016; Bell et al., 2022) and programs designed to reduce criminality among high school students (Heller, 2014; Heller et al., 2017; Modestino, 2019; Davis and Heller, 2020; Lavecchia et al., 2024), these interventions come too late for people who have already dropped out of high school. The options that are available tend to be ineffective (Heckman et al., 2011; Jepsen et al., 2016). Getting adults to a high school diploma via TEC is much more ambitious and expensive than a GED, but the investment generates large benefits. We use the estimates from this study, valuations from the criminology literature on the social cost of crime (Cohen and Piquero, 2009; Bhatt et al., 2024), and a marginal value of public funds framework (Hendren and Sprung-Keyser, 2020) to compare these costs and benefits. Crime reduction at least doubles the marginal value of public funds, compared to valuing only labor market benefits. Under our preferred set of assumptions, spending \$1 of public funds on adult high school diplomas yields about \$20 of benefits when only counting labor market gains but about \$60 when including criminal justice benefits. While there is some evidence that workforce training for adults might reduce criminal justice system contact (Schochet et al., 2008; Barden et al., 2018; Anwar et al., 2022), the effects we observe place TEC among the most cost-effective human capital generation programs for adults (Hendren and Sprung-Keyser, 2020). Thus, our results provide an example of success for a very traditional educational goal among a very non-traditional population. A high school diploma can reduce criminal justice system contact for adults who previously left school without a diploma.

2 Background

2.1 Education and Crime

The criminal justice system largely confines people with low levels of formal education. For example, Figure 1 summarizes nationally representative data from the National Longitudinal Survey of Youth, 1997 cohort. As of 2019, the average respondent was 37 years old. Among people without a high school diploma or GED, 23 percent had ever been incarcerated by that time, and the rate for people with a GED was actually somewhat greater, 31 percent. In contrast, people with a high school diploma have much lower rates of incarceration, only eight percent for people ending with the diploma and even lower for those completing higher education. Arrest rates show a similar pattern: 52 percent of those with no degree and 69 percent of GED recipients have ever been arrested, compared to 35 percent of those holding a high school diploma as their highest degree. Of course, the criminal justice system arrests and incarcerates people in ways that correlate with pre-existing characteristics. Correctional institutions also support GED completion (Darolia et al., 2021), which will mechanically inflate lifetime incarceration rates for GED recipients relative to those with no degree. So, whether education itself generates the sharp contrast between people with and without high school diplomas remains an empirical question.

2.2 The Excel Center

The Excel Center (TEC) is a tuition-free, public charter high school for adults operated by Goodwill of Central and Southern Indiana. TEC opened in September 2010, and currently Goodwill operates 15 campuses across Indianapolis and other parts of central and southern Indiana. It operates as a public charter school. During our study period, TEC funded its operations using the same per-student funding from the State of Indiana received by charter schools for traditional high school-aged students. While the Excel Center model has since been replicated by other Goodwill organizations in Arkansas, Arizona, Colorado, Illinois, Kentucky, Maryland, Missouri, Northern Indiana, Tennessee, South Carolina, Texas, Virgina, and Washington DC, this paper focuses on applicants to the original

Excel Centers in Central and Southern Indiana.

Students may be recruited to apply to TEC through TEC's connection with a variety of local actors. First, Goodwill seeks to maintain relationships with local schools meaning that many students are referred to TEC by their previous or local high school. Goodwill also promotes TEC among individuals participating in other Goodwill-sponsored programs, for example, Nurse-Family Partnership, as well as other community programs that target a similar population.

To enroll, students first complete an application, which is available year-round. Though Goodwill has worked to make the application accessible to all, one common reason for being unable to complete the application is not being able to provide an official transcript from a previous high school. In addition to the transcript requirement, applicants must be an Indiana resident, lack a high school diploma, not be a sex offender, and be at least 18 years old, though some age exceptions down to age 16 are made. Once the application is complete, all eligible students are assigned an orientation date. Prior to increasing the number of TEC schools in Indiana, some students had to wait months for an orientation, but in recent years orientation typically follows a few weeks after application. Students who attend orientation can enroll throughout the year in the next available term and continue taking classes until they graduate or drop out. Time to graduation varies, depending on a student's prior credits, level of knowledge, and performance in course work. The typical graduate in our sample enrolled for seven terms, or just over one year, and the typical exiter was enrolled for about six terms.

Students who graduate from The Excel Center in Indiana earn the state's Core 40 diploma, the standard credential earned by most high school graduates across the state. The requirements for obtaining a Core 40 diploma from TEC are the same as at a traditional high school: students must pass 40 specific credits of coursework. For example, the Core 40 requires six mathematics credits resulting from passing Algebra I, Geometry, and Algebra II. Typically, students must also pass graduation tests in both Math and English/Language Arts, though the exact test and rules regarding exceptions have changed over time for TEC students as they have for the entire state.

The Excel Center is designed to help students who previously dropped out of high school over-come barriers to graduation and earn a standard high school diploma. To that end, the school has a number of unique features relative to more traditional high schools.

First, students take a set of courses aimed at their individual level of knowledge and existing credits. Upon enrollment, TEC students take placement exams to assess their knowledge in core subjects. The school, using placement test results and existing transcripts, then assigns the student a course plan that matches courses to the student's actual level in each subject area. Though many TEC students enter having completed a sufficient number of general credits to graduate, students often begin in remedial classes because they start behind in core areas such as Math and English.

Second, class schedules are flexible to accommodate non-traditional students who live adult lives with greater family and work obligations. Students can register for full-day or half-day schedules. Rather than a typical semester system, the school operates on a year-round schedule with five, eight-week terms. Intensive, frequent terms accelerate the path to graduation for students who are far behind in a particular area.

Third, TEC provides extensive support services targeted at common non-academic barriers to graduation. Perhaps most importantly, TEC provides high-quality, on-site child care at no cost to students. Many students have young children, and TEC allows these students to attend school while their children attend daycare at the same location. Campuses are also located near public transit routes and students can receive transportation assistance in the form of bus passes or tokens, gas cards, or carpooling options. Finally, students are paired with a life coach who helps them set goals, navigates graduation requirements, and provides support for issues that occur beyond the classroom. TEC funds these extensive services using the same funding levels received by other high schools.

Fourth, TEC directly connects students to post-secondary education and training. In recent years, TEC has strongly encouraged students to use elective credits to participate in professional and course certificate programs that provide an entryway to higher paying jobs such as pharmacy technician, dental assistant, HVAC maintenance technician, and welder. Other students enroll in dual college-credit programs and use the diploma as a step toward attending college.

3 Empirical Strategy

3.1 Data

We study the set of people who applied to The Excel Center schools in Indiana between January 2013 and December 2017.[4] From Goodwill's administrative records, we can identify by name and date of birth all applicants. We use these internal enrollment and graduation records to categorize applicants into three groups: those who applied but did not enroll, those who enrolled but did not graduate, and those who enrolled and graduated. Throughout, we refer to these groups as non-enrollees, exiters, and graduates, respectively. We also observe some limited demographic information for all applicants including age at application, sex, race, and ethnicity.[5] Finally, we restrict our sample to those who are 19 years or older at the time of application, so that three years prior to their application they would be at least 16 years old (and thus potentially observed in court records, as described below).

We link Excel Center applicants to administrative court records from the Indiana Office of Court Services (IOCS) to measure contact with the criminal justice system. These records aggregate caseload information from all courts and probation offices in the state of Indiana, and include cases with severity ranging from felonies to infractions. We use an extract of records for all cases filed between January 1, 2010 and December 31, 2022. As court records, they are limited to instances in which charges are filed. Our primary outcome is an indicator of having any court filing record in Indiana, though we also examine sub-categories depending on offense type (e.g., violent, property, etc.) and severity (e.g., felony versus misdemeanor). We classify the type of crime using the Text-based Offense Classification (TOC) tool from the Criminal Justice Administrative Records System (CJARS) (Choi et al., 2023), specifically the CJARS-created Uniform Crime Classification Standard (UCCS).[6] We first classify at the charge level, and then use these classifications to label broader

[4]Some records in this period (those from October 2016 to June 2017) are not available due to the Excel Center switching student information systems.

[5]Sex, race, and ethnicity are not directly observed for all students depending on the age of their records (the Excel Center collected this information in 2017, but not prior). For those without these variables directly observed, we impute them using name, date of birth, and address. See Appendix section A.3 for more details.

[6]We rely on the CJARS classification for most filing types, but also manually reviewed the classifications and

filings.[7] The timing of all outcomes is based on the distance between the filing date on a court case record and a student's application date. All events within one year of an application date would contribute to "relative year 0", while all events at least one year but less than two years after an application date would contribute to "relative year 1" and so forth.

Importantly, these court data measure contact with the criminal justice system, rather than directly measuring criminal behavior. The criminal justice system will arrest and charge people who have not committed crimes and fail to apprehend others who have committed a crime. Our measures of criminal justice system contact will precisely align with outcomes of policy interest when considering, for example, the fiscal cost of incarceration but will only approximate the object of policy interest in other instances, such as calculating the cost of crime to victims.

We match our study sample to outcomes using identifiers in both data sets. First, we match records exactly on name, date of birth, sex, and race. If we only used this exact match, we could be falsely excluding true matches because of data entry errors in either data set (e.g., misspelled names). To account for this possibility, we conduct a "fuzzy match" on the same variables, which entails generating a score based on how well the variables match, and manually reviewing potential matches in each score group up to a cutoff. For more details, see Appendix section A.5.

Our sample of TEC applicants is more than three times as likely to come into contact with the criminal justice system than the general population. Our primary outcome of interest is the presence of a matching record within a given time period. Table 1 compares court filing rates for our study sample to average filing rates for the broader population in Indiana in 2010. As shown in the first row, there are 0.034 court filings in 2010 for every Indiana resident at least 15 years

made changes when necessary to better reflect statutes within Indiana and the general prevalence of different charges in Indiana. For more details, please see Appendix section A.1.

[7] Case filings can include multiple charges, each charge of which can have a different degree (e.g., felony, misdemeanor, infraction), a different disposition (e.g., a "guilty" or "not guilty" disposition), a different broader "type" (e.g., traffic, drug, property), and a different more specific "category" (e.g., minor traffic offense, larceny, simple assault) attached to it. For each of these potential variations, we flag a broader case filing as having a particular label if the case includes at least one charge that has that label. For example, a case with at least one felony charge will be labeled more broadly as a "felony filing." Filing types and categories are thus not necessarily mutually exclusive. The exception is "guilty" versus "not guilty" filings, which are defined as having at least one guilty charge and no guilty charges, respectively. For more details about the degree types labeled as misdemeanors or felonies, or the dispositions categorized as "guilty" vs "not guilty", see Appendix Tables A-1 and A-2.

old.[8] Our analysis sample (limited to individuals observed in 2010) had more than twice as many filings, 0.107 per person. This result is expected given we study a sample of people who previously dropped out of high school and have higher than average risk of court contact. Patterns across groups are similar for both the study sample and the broader population (e.g., males have higher rates of court filings than females, younger age groups generally have higher rates of court filings than older age groups). Rates of court involvement are also consistently higher in our study sample for each of these subgroups. Altogether, the alignment of these descriptive facts with known facts about the criminal justice system suggest that our data matching process accurately depicts risk of contact with the criminal justice system, which is high in this study sample.

3.2 Regression Framework

To measure the causal effect of graduating with from TEC with a high school diploma, we compare outcomes across non-enrollees, exiters, and graduates over time.[9] Figure 2a displays the basic identification strategy. Each line shows the fraction of each group with a court filing in a given year, with time measured relative to the year of applying to The Excel Center. As shown in the left side of the figure, students who eventually graduate from TEC have lower court filing rates in the three years prior to applying to TEC, but they have similar upward trends in filing rates that likely reflect age trends for the young sample aging into adult court records. After applying to TEC, both non-enrollees and exiters continue to have smooth time profiles, but contact with courts drops sharply for graduates. Similar trends but different levels in pre-period outcomes suggest that empirical strategies that use differences-in-differences or weights based on pre-application filing rates will successfully account for selection into graduating from TEC. The sharp break in trends for graduates but not other groups preview our main result, that graduating with a diploma leads to an immediate and sustained reduction in contact with the criminal justice system.

[8]The population of Indiana residents who are at least 15 years old is obtained from the 2010 decennial census population counts. The number of court filings in Indiana for those that are at least 15 years old comes from the IOCS data, which has state-wide coverage. See the notes below Table 1 for more details.

[9]We leverage a similar identification strategy as Brough et al. (2024) who measure the labor market impacts of TEC graduation. As such, some language in this section closely follows the description in that paper.

We can make comparisons across these groups using the following regression framework:

$$Y_{it} = \sum_{k \in T} \beta_{1k} I(t = k) * D_i + \tau_{1t} + \epsilon_{1it} \tag{1}$$

In this equation Y_{it} is an outcome, such as an indicator for having a court filing, for person i measured at time relative to application t. D_i is an indicator of treatment status. Time fixed effects τ_{1t} make all comparisons within time period. We estimate this regression for all two-way comparisons across the three groups of applicants. For example, when we estimate this regression with a sample composed of graduates and non-enrollees, the coefficient β_{1k} measures the difference in average outcomes in period k between those who graduate and those who apply but do not enroll. If given a causal interpretation, this coefficient would measure the total return to enrolling at TEC and finishing with a high school diploma.

Simple comparisons across groups of applicants likely do not measure the causal return to enrolling and graduating from TEC. Columns (1) through (3) of Table 2 display mean baseline characteristics of the three groups. For example, graduates are on average 27 years old when they apply to TEC, which makes them younger than exiters (28.4) and applicants (29.3). Similarly, graduates are more likely to be female and have less extensive court records prior to applying. As Figure 2a shows, these pre-existing differences lead to different pre-period levels of criminal justice system contact but not different trends. As a result, we do not estimate simple differences across groups of applicants. Instead, we focus on specifications that control for these level differences using individual fixed effects and/or weights based on pre-application outcomes.

First, we estimate a difference-in-differences model:

$$Y_{it} = \sum_{k \in T} \beta_{2k} I(t = k) * D_i + \tau_{2t} + \psi_{2i} + X_{it}\Gamma_2 + \epsilon_{2it}. \tag{2}$$

In this specification, we include individual fixed effects (ψ_{2i}), which control for time-invariant differences between TEC graduates and other TEC applicants, in addition to relative time fixed effects (τ_{2t}). At times, we also include relative time effects that vary with applicant age ventile and calendar quarter fixed effects, X_{it}, to non-parametrically adjust for observed differences in trends by age between TEC graduates and other TEC applicants and to control for differences in the external environment over time. In this model, β_{2k} compares the change in outcome over time for one group of applicants to that of another groups of applicants. To compare all three groups of applicants, we use the following specification:

$$Y_{it} = \sum_{k \in T} [\delta_k I(t = k) * Enrolled_i + \gamma_k I(t = k) * Graduated_i] + \tau_{3t} + \psi_{3i} + X_{it}\Gamma_3 + \epsilon_{3it} \tag{3}$$

In this case, $Enrolled_i$ is a dummy for those who enroll, both graduates and those who exit, and $Graduated_i$ is a dummy for graduates. Thus, δ_k measures the effect of enrolling while γ_k measures the added effect of graduating, above and beyond enrolling. If we omit $Graduated_i$ from this model, then the coefficient on $Enrolled_i$ measures the overall average effect of enrolling, taking into account the effect of graduation and the probability of graduating.

However, the difference-in-differences estimates rely on functional form assumptions that may fail in our context, so we also control for pre-application differences directly and flexibly by re-weighting the data. As shown in Table 2, the graduates have lower levels of criminal justice system contact in the pre-period so that a similar percentage point decline results in a larger proportional decline. Also, while Figure 2a shows similar pre-application trends, graduates may have a slightly steeper upward trend than applicants. Together, these facts imply that a difference-in-differences approach may underestimate treatment effects. To account for these features, we re-weight the data. In particular, we use inverse propensity weights as in Bitler et al. (2006), adjusted to account for the fact that we have three groups. Predicted probabilities come from a logit model of enrollment or graduation (conditional on enrollment) that employs a LASSO operator. Variables supplied to this model are indicators for age ventile, sex, race, and whether a person had a case filing, a misdemeanor filing, or a felony filing in each of the three years before application.[10] As

[10]Formally, we separately model both the likelihood of enrolling and the likelihood of graduating, conditional on enrolling. For the latter, we restrict the sample to enrollees only. In this scenario, the weight is given by $\hat{w}_i = \frac{(1-Enrolled_i)}{(1-\hat{p}_{Enroll})} + \frac{Enrolled_i(1-Graduated_i)}{\hat{p}_{Enroll}(1-\hat{p}_{Grad|Enroll})} + \frac{Enrolled_i(Graduated_i)}{\hat{p}_{Enroll}(\hat{p}_{Grad|Enroll})}$, which is then standardized across observations to sum to 1. When comparing subgroups of the sample, we re-estimate the predicted probabilities specific to the

shown in Figure 3a, this method essentially re-weights the data to ensure that pre-period average outcomes more closely match for the three groups. Similarly, columns (4)–(6) of Table 2 shows that this re-weighting procedure generates balance in demographic characteristics that correlate highly with criminal justice contact, e.g. age. While individual fixed effects eliminate the pre-period difference in outcome levels, inverse propensity weights instead match the full time profile rather than subtracting away a simple level shift. As such, the two approaches address a common problem of pre-period level differences in outcomes but based on different identifying assumptions. In our preferred specification, we implement doubly robust specifications that include both individual fixed effects and inverse propensity weights and estimates will be unbiased if either the parallel trends or the conditional mean independence assumptions are satisfied. Moreover, we directly test the similarity of pre-trends below following Borusyak et al. (2024).

4 Results

4.1 Education and Employment

While this paper focuses on criminal justice outcomes, attending the Excel Center has more immediate implications for education and labor market outcome, which were studied in detail in Brough et al. (2024) and we summarize here.

TEC helps its students acquire additional education and credentials. First and foremost, TEC helps some students successfully complete a traditional high school diploma. In our sample, about 22 percent of students eventually graduate. While this rate is lower than traditional students (Murnane, 2013), it compares favorably to e.g. GED pass rates (Heckman et al., 2011). The majority of TEC students graduated or exited within 2 years (Appendix Figure A-2, Brough et al., 2024). Important for our identification strategy, students have few other similar options for obtaining a high school credential. Among people who apply to TEC but do not enroll, only nine percent eventually pass the state high school graduation exam (typically required for a diploma),

comparison and sample being used.

and only seven percent pass the GED exam within five years (Appendix Figures A-3 and A-4b, respectively, Brough et al., 2024). Finally, TEC also connects its graduates to technical and higher education. Compared to applicants who do not enroll, they are 48 percentage points more likely to receive professional and technical certifications and 19 percentage points more likely to earn any college credits (Appendix Tables A-13 and A-14, respectively, Brough et al., 2024).

These increased skills led to better labor market outcomes for TEC graduates. Compared to applicants who do not enroll, graduates experienced a 39 percent increase in earnings within five years of applying (Table 3, Brough et al., 2024). This improvement represents about half of the typical gap between people with a high school diploma and those with no such credential. Increased earnings result from both a five percentage point increase in employment and greater earnings conditional on employment (Figure 3, Brough et al., 2024). A diploma from TEC particularly opens up employment in higher-paying industries with stable employment: graduates shift from hospitality to healthcare jobs and have longer continuous spells of employment (Figure 5, Brough et al., 2024). Together, these results indicate that the skills that TEC students gain through a diploma lead to better jobs with greater earnings.

4.2 Main Effects on Criminal Justice System Contact

Graduating from The Excel Center reduces the likelihood that a student comes into contact with the courts. The simple time trends in Figure 2 show a drop in the rate of court filings, which appears for "any filing" as well as for misdemeanor, felony, guilty, and not guilty filings. Figure 3 shows that the drop in court contact sharpens when we use inverse propensity weights to reweight the data to generate similar pre-application levels of criminal justice contact across the groups. More formally, we estimate treatment effects according to Equation (2). Figure 4 plots these dynamic treatment effects. The teal triangles show within-year differences between graduates and non-enrolled applicants. Graduates are relatively less likely to have a court filing in the year immediately following their application to TEC (i.e., Year 0). This effect decays somewhat over time but persists for at least another five years afterward. The figure decomposes this overall effect into the effect of graduation conditional on enrollment (navy circles) and the effect of enrolling (gold diamonds); graduation rather than enrollment drives the effects.

Table 3 quantifies these results and tests their robustness across different specifications. Column (1) shows the overall effect of enrolling at TEC, using the simplest specification. It implements Equation (1) comparing post-application outcomes of all enrollees (both exiters and graduates) to non-enrolled applicants, weighting by inverse propensities. Under this specification, TEC students are 2.1 percentage points less likely to appear in court in the year following application, a 16 percent reduction relative to the non-enrollee rate of 12.9 percent. Column (2) decomposes the overall average effect of enrolling at TEC into the separate effects from only enrolling and the additional effect from graduating. With this specification, graduating leads to a large and statistically significant reduction in court filings (6.0 percentage points) compared to a smaller estimated reduction in court filings for enrolling (0.8 percentage points), which now cannot reject the null of no change.[11] Column (3) reports the doubly-robust model that combines the use of propensity weights with a differences-in-differences approach that uses pre-application data as in Equation (3). This specification strengthens the measured treatment effects somewhat but does not change their statistical significance or qualitative pattern relative to Column (2). Column (4) further includes calendar year and age-relative year fixed effects, which do not change the results much. Finally, we rely on column (5) as our primary specification. This model uses the doubly-robust specification from column (4) but estimates effects in Year 0 and the average effect across Years 1–5. Thus, our preferred estimate suggests that only enrolling at TEC does not reduce court contact by much, if at all. However, graduating decreases court contact by 6.3 percentage points in the application year. About 73 percent, or 4.6 percentage points, of this reduction persists in subsequent years.

Across all specifications, we fail to reject the null hypothesis that pre-trends are the same across all three groups. To implement the test of Borusyak et al. (2024), we test joint hypotheses that pre-period coefficients are equal to zero. For example, in column (1) we regress the outcome on indicators that interact enrollment status with each "relative year" in the pre-period, along with

[11] Note that the effect in column (1) is a weighted sum of the enrollment and graduation effects that accounts for the 22 percent graduation rate observed in our sample: $-0.008 - (0.22 * 0.060) = -0.021$.

relative year fixed effects. Then, we use a Wald test for the joint hypothesis that the coefficients for these indicators are equal to zero, and report the p-value (i.e., $\Pr(\hat{\beta}^{Pre1-3}_{Enroll} = 0)$). For column (2), the procedure is similar but also includes indicators that interact graduation status with each relative year. Then, we separately test that coefficients are 0 for the enrollment-related indicators and the graduation-related indicators (i.e., $\Pr(\hat{\beta}^{Pre1-3}_{Enroll} = 0)$ and $\Pr(\hat{\beta}^{Pre1-3}_{Grad} = 0)$). For column (3), we add person fixed effects, use relative year -3 as the reference period, and jointly test that coefficients related to periods -2 and -1 are zero, and report $\Pr(\hat{\beta}^{Pre1-2}_{Enroll} = 0)$ and $\Pr(\hat{\beta}^{Pre1-2}_{Grad} = 0)$. The procedure for columns (4) and (5) is similar to column (3), but also includes calendar year fixed effects and age-by-relative-year fixed effects. For all of the columns, these regressions restrict to observations within the pre-period, and are weighted according to the same inverse propensity weights that are used in their respective main analysis specifications. We report p-values of these tests in the rows of the table titled "P-val. Pre-trend: Enrolled" and "P-val. Pre-trend: Graduated."

4.3 Robustness of the Identification Strategy

Our identification strategy assumes that, after adjusting for observable selection into enrollment and graduation related to prior criminal justice contact, any remaining selection into TEC graduation among applicants is independent of subsequent court contact had the applicants not graduated. This identification assumption could fail in at least two ways. First, applicants may select into graduation based on post-application shocks, e.g., if a personal crisis leads a person to both stop attending classes and participate in illegal activity. Second, causality could run the opposite direction with criminal justice involvement causing TEC applicants to exit the program. For example, the student becomes incarcerated and can no longer attend class. [12] In either case, the resulting bias would lead our main results to overestimate the effect of TEC graduation on criminal justice contact. These

[12] We are able to observe specific exit reasons recorded by TEC related to criminal justice involvement starting in 2017 (prior to 2017, we can only observe indicators for exits related to a work conflict, disinterest in the program, interpersonal reasons, and "no shows"). For the subset of our main sample that has an application in 2017, exit reasons coded as "court ordered" or "incarcerated" are very infrequent, occurring less than one percent of the time among those who enrolled but exited. This could suggest that leaving strictly because of criminal justice involvement is actaully rare, or, more likely, that this type of exit is often not directly observed or recorded as such by TEC. For this reason, we assume that we cannot directly observe if a student left TEC directly related to a court filing, and conduct the timing and bounding exercises described in this section.

phenomena presumably exist to some extent; but their extent is likely somewhat limited. For example, Appendix Figure A-2 shows that enrollees who have a court filing during their enrollment have similarly-timed exit rates as a matched comparison group of enrollees that did not have a filing during their enrollment. Moreover, the majority of students that have a filing are still enrolled six months after this filing (see Appendix section A.6 for more detail). The key question, then, is whether selection on negative shocks and reverse causality are empirically relevant enough to generate large bias in our main results.

We bound such bias by focusing on a sample of TEC students who exit due to known, positive shocks. TEC records include withdrawal reasons for all exiting students. One common reason is a work conflict, and students who exit due to work are positively selected, having greater earnings levels before applying and steeper earnings trends after applying (Brough et al., 2024, p. 53). Because the exit reason is known for this group, reverse causality is not a concern, and because this group is positively selected, we will obtain a conservative estimate of treatment effects. Figure 5 repeats the main weighted specification as in Figure 4, but restricts the exited group to individuals that left TEC because of work conflicts. The navy circles show the comparison between graduates and exiters with a work conflict. The pattern of effects—an immediate large decline that attenuates partially over time—is qualitatively similar to the main results. The magnitude of the effects is somewhat smaller. Appendix Table A-4 reports results using comparisons from various exit reasons. The treatment effect measured by comparing graduates only to those who exit for work conflicts is roughly 65 percent as large in year zero (4.1 percentage point decline) and about 70 percent as large in following years (3.2 percentage point decline), relative to our main specification.

Our results are also generally robust to various specifications of the inverse propensity weights that consider concerns related to omitted variable bias, overfitting, and selecting the procedure used to generate weights (Table A-5). In our primary specification reproduced in column (1), we construct weights based on logit models of enrollment and graduation that use LASSO-selected baseline characteristics. For this specification, the candidate list includes age ventiles, indicators for sex, indicators for race groups, and annual indicators for any filings, felony filings, and misdemeanor filings. However, this is just one potential choice of procedure for generating weights with a candidate variable list. For transparency, we report versions that rely on one round of LASSO selection (the default method in this paper), as well as versions that (1) estimate a logit model without the LASSO selection, and (2) use LASSO to select variables correlated with both the outcome or treatment status, not just treatment ("double selection"). These different procedures are noted in Table A-5 as "logit" and "2x selection", respectively.

Across these three procedures, the direction and statistical significance of estimated effects for graduates remains constant. There are, however, moderate differences that attenuate the magnitude for these coefficients. For example, the largest difference among the different procedures (using the original candidate variable list) can be seen comparing column (1) to column (3), in which the long-run effect of graduation decreases from 4.6 percentage points to 2.8 percentage points. In general, these estimates can serve as a lower bound for our estimated treatment effects.

Across all of these processes, bias could enter if the variables provided as candidates to construct the weights omit factors that are correlated with both subsequent court contact and selection into enrollment and graduation. Thus columns (4) through (6) of Table A-5 test whether including a richer set of variables in the candidate list used to construct weights affects the analysis. To the original list, we add "risk" variables guided by the Indiana Risk Assessment System's (IRAS) Pretrial Assessment Tool and Community Supervision Tool, as well as home Census tract characteristics.[13] Adding these candidate variables does not change the direction or statistical significance of the main results, and slightly increases point estimate magnitudes.

An alternative concern related to the weights used in our primary specification could be that the weights are constructed using too many covariates; particularly, including lagged outcomes could

[13] "Risk" variables include: an indicator for whether an individual was younger than 33 at application and had a filing in the pre-period before application, an indicator for having three or more filings in the pre-period, a variable summarizing the number of drug-related filings the individual had in the pre-period (=0 if none, =1 if either one or two, =2 if greater than two), a variable summarizing the individual's most serious pre-period filing (=0 if none, =1 if most serious is a misdemeanor, =2 if most serious is a felony), and a variable summarizing the number of felony filings in the pre-period (=0 if none, =1 if one or two, =2 if greater than two). "Home Census tract characteristics" include total population, median income, percentage of the population that is White, the percentage of individuals over 25 with a college degree or higher, and the civilian employment rate. Please see the Appendix section A.2 for more details about the construction of Census variables.

lead to over-fitting and bias in the presence of mean reversion. To address this concern, columns (7) through (9) exclude annual indicators for any filings, felony filings, and misdemeanor filings from the candidate variable lists used in columns (1) through (3). The results change little when comparing across estimates, holding fixed the procedure used to select controls.

As final robustness checks, we (1) check if the pattern of results seen for our main sample and specification hold when changing the event window considered, and (2) design a placebo test to assess whether our results are spurious. Our results hold across different pre-period and post-period lengths – Figures A-5, A-6, and A-7 and Table A-9 show that the estimated treatment effects of enrollment consistently cannot reject 0 for both Year 0 and Year 1 onward, while the impact of graduating is consistently negative in both periods (in Year 0, magnitudes range from a decrease of 4.4 percentage points to a decrease of 6.3 percentage points, and for Year 1 onward, magnitudes range from a decrease of 3.3 percentage points to a decrease of 4.6 percentage points.)

For the placebo test, the general intuition is to randomly assign a fake application date to each student within their pre-application period, and then carry out the same estimation strategy as for our main results. Specifically, we carry out 500 iterations of the following process. First, we assign a new, random application date within each individual's actual pre-application period. We then restrict to a "placebo sample" of students (1) who are at least 19 years old at the time of their random application date, and (2) whose outcome data from more than one year prior to their actual TEC application date can cover an event window of $-3 \leq t \leq 5$ relative to their random application date. Then, for each placebo sample, we run the same estimation as for our main specification. Figure A-1 shows the empirical distribution of estimates from this procedure. Encouragingly, we cannot reject a null hypothesis of zero effect for each comparison of interest (graduates vs those who exited, those who exited vs those who did not enroll, and graduates vs those who did not enroll). This supports the notion that our main estimation results represent a true treatment effect of TEC.

4.4 Types of Charges

Court filings fall for charges of various levels of severity for TEC graduates. Table 4 shows treatment effects of graduating using different subsets of court filings as outcomes.[14] Column (1) replicates our prior results, while columns (2) and (3) show that the overall decrease in filings reflects sizable decreases for both filings with misdemeanor charges and filings with felony charges. Longer-term rates of filings with misdemeanor charges drop by 4.7 percentage points (about 54 percent of the mean rates for applicants in years 1–5), while rates for filings with felony charges decrease by 2.5 percentage points (about 58 percent relative to the comparison mean).

Furthermore, the overall decline in filings encompasses both those that end in guilty dispositions and those that do not. Columns (4) and (5) of Table 4 show a roughly balanced split in treatment effects in Year 0 for filings with guilty dispositions and those without (a 3.1 vs. 2.7 percentage point decrease, respectively), which tilts towards guilty filings in years 1–5 (a 3.4 vs 1.8 percentage point decrease). As with most court cases, guilty dispositions are largely plea agreements, and not-guilty dispositions mostly reflect situations where charges were dismissed.[15] While the treatment effects on guilty versus not guilty dispositions both reflect the effect of TEC on a student's contact with the criminal justice system, only the former provides any evidence of a decline in crime. We find effects on both.

Graduating with a high school diploma also broadly affects charges for various types of criminal charges. Table 5 displays treatment effects for broad categories of criminal charges.[16] Column (1) again replicates our main results and subsequent columns report the estimated effects on filing rates by the crime type (i.e., filings with at least one criminal traffic charge, non-traffic charge, DUI charge, drug charge, property charge, public order, or violent charge). Compared to mean filing rates in Year 0, we see a slightly larger impact for filings with criminal traffic charges as compared to those with non-traffic charges. Specifically, we observe a 66 percent decrease in filings with traffic

[14] Figures 4b through 4e plot the dynamics of treatment effects for these subsets of charges.

[15] See Appendix Table A-6 for more detail, which reports treatment effects for the five most common guilty dispositions and the five most common not-guilty dispositions.

[16] See Appendix Table A-7 for similar results but for more detailed charge categories.

charges, the most common of which are minor offenses like driving without a license or driving with a suspended license. For filings with non-traffic charges we see a 48 percent decrease in Year 0, which largely persists through Years 1–5 to make decreases in traffic versus non-traffic filings roughly balanced (a 50 percent decrease for traffic filings, and a 45 percent decrease for non-traffic filings). In percentage point terms, the largest decreases in Years 1–5 among non-traffic filings are those with drug-related charges, followed by public order-related charges and violent charges. We do not observe a meaningful change in filings with DUI-related charges. Finally, the estimated effects on violent crime (a decline of 1.3 percentage points in Years 1–5, or 56 percent of the comparison mean) will be particularly important for cost-benefit analysis because of its high social cost.

4.5 Sub-Group Effects

A high school diploma reduces justice contact across different demographic sub-groups, but treatment effects tend to be larger for groups of students who are at greater risk of contact with the criminal justice system. Figure 6 summarizes treatment effects for different subgroups of the main sample, and Table 6 details these results. For example, relative to applicants, graduates who had a court record in the three years prior to applying to TEC experience larger declines in court contact compared to their counterpart graduates who did not have a filing in their pre-application period (an 8.3 percentage point decline versus a 3.1 percentage point decline in Years 1–5). These differences largely reflect differences in baseline risk: 26.9 percent of non-matriculating TEC applicants with a history of court filings have a new filing in the first year, compared to 8.0 percent of those with no court records. Other subgroup comparisons follow this pattern—namely, men and younger applicants experience both much larger treatment effects and greater baseline risk. Characteristics that are relatively less predictive of baseline risk in our sample, like race and residence within versus outside Indianapolis, also do not as strongly predict treatment effects.

5 Discussion

5.1 Mechanisms

Pursuit of a high school diploma as an adult may reduce contact with the criminal justice system through incapacitation or by improving job prospects that change the opportunity cost of crime.

Incapacitation may be either direct or dynamic. Direct incapacitation relates to how participation in schooling reduces contemporaneous criminal justice contact, e.g. because it is difficult to commit a crime while in class. Dynamic incapacitation effects can result from path dependence, for example when being in school prevents someone from accumulating a criminal record which then affects that person's future legal and non-legal options. Evidence on the empirical magnitude and even direction of the incapacitation effects of schooling varies. Bell et al. (2022) find that nearly all effects of recent increases in compulsory schooling ages on crime result from either direct or dynamic incapacitation. But evidence for incapacitation more generally is mixed; for example, violent crime incidents actually fall when schools close for teacher in-service days (Jacob and Lefgren, 2003; Billings and Phillips, 2017).

Incapacitation effects only account for a small portion of the effects we observe. Two facts demonstrate this. First, Table 3 finds treatment effects on court filing rates for at least six years, which is well beyond the period of enrollment. Figure A-3 shows that graduates are enrolled for a median of 604 days (roughly 1.7 years), and Figure A-4 shows that the median graduate has left TEC by Year 2 post-application. That we see significant decreases in filing rates throughout the five years post-application suggests that a mechanism beyond incapacitation is contributing to the decreases in court contact. Second, even if we assume all measured effects for exiters result from incapacitation, incapacitation plays a minor role overall. As shown in columns (2) to (5) of Table 3, most of the impact of TEC goes through graduation rather than enrollment, though there are negative enrollment point estimates of -0.8 pp and -0.7 pp in the first two years, summing to -1.5 pp if ignoring subsequent positive point estimates. Suppose this entire effect is due to incapacitation. The median exiter is enrolled for 128 days in the year of their application and 270 days total, compared to 214 and 604 for the median graduate. If the incapacitation effect scales proportionally with time enrolled, it then accounts for a 3.4 pp decline over the entire six years. That is, it accounts for less than half of the decline in the first year and less than one-tenth of the total effect of graduating over the course the six observed years.

Obtaining a high school diploma could also affect criminal justice contact by encouraging legal employment. If the opportunity cost of committing crime is a person's wage (Becker, 1968), then the availability of high-quality jobs and better wages can affect criminal justice contact (Grogger, 1998; Schnepel, 2018). As shown in Brough et al. (2024), graduating from TEC improves employment opportunities, increasing total earnings by 38 percent. This increase is split between increased employment and substantial increases in earnings conditional on working. The latter comes largely from the accumulation of skilled certifications and sectoral shifts from low- to high-paying industries, e.g. from hospitality to healthcare.

The employment effects generated by receiving a diploma from TEC can account for some of the criminal justice effects that we observe but are too small to be the primary driver. For example, Agan and Makowsky (2023) find an elasticity of -0.28 between the minimum wage and recidivism, which is one of the larger effects of an income-related program on crime (Ludwig and Schnepel, 2025). If we take that value as a general relationship between earnings and criminal justice contact, the 38 percent increase in earnings for TEC graduates would imply an 11 percent decline in court charges. That proportion is substantial but much smaller than the nearly 45 percent persistent decline we observe in Years 1–5.

Thus, an adult high school diploma's effects on criminal justice contact likely result from a diffuse set of mechanisms. Direct incapacitation and increased employment likely contribute significantly, but cannot explain the majority of the magnitude of the effects that we observe. Other more difficult-to-quantify mechanisms likely also drive lower criminal justice contact. Additional mechanisms proposed by the literature include dynamic incapacitation, a higher opportunity cost of punishment, and the ways schooling affects preferences regarding risk, patience, and legal behavior. However, we are unable to test the exact contribution of each of these potential mechanisms with the available data.

5.2 Cost-Benefit

We summarize the economic impact of reducing crime through adult high school education using the marginal value of public funds (MVPF) framework (Hendren and Sprung-Keyser, 2020). This section augments the MVPF estimates reported in Brough et al. (2024), which relied solely on earnings impacts, by including new estimates of the effects on both the social and fiscal costs of crime. The MVPF is the ratio of the willingness to pay (WTP) for TEC and the cost of providing the program net any fiscal savings. As such, we add any effects on the social cost of crime that we observe to the already estimated impacts on after-tax earnings gains experienced by TEC students. Additionally, we incorporate the fiscal cost savings from reducing crime into the net cost of the program that already incorporated increases in taxes collected from future earnings gains.

We follow Bhatt et al. (2024) in constructing estimates of the effect of TEC on the costs associated with crime. We start with the detailed crime categories as shown in Appendix Table A-7. We assign costs based on crime type to each guilty disposition in the court data using the costs from (Table A.XVIII, Bhatt et al., 2024). These valuations include reductions in both fiscal costs to the criminal justice system, which we subtract from the fiscal cost of the program, and social costs to the public, which we add to the willingness to pay. Social costs are particularly hard to value: putting dollar values on how victims' costs vary with the type of crime is subjective, and many crimes are not observed as guilty court filings because of low reporting and arrest/charging rates. We use the same estimates as Bhatt et al. (2024), who rely on willingness to pay measures from the criminology literature and include both lower bounds that do not scale up costs by reporting and arrest rates and upper bounds that do. Appendix Table A-8 reports effect estimates on the cost of crime using court cases in our data that ended with a guilty disposition.

Previously, Brough et al. (2024) show that TEC generates reasonably large benefits for students relative to its net fiscal cost. Table 7 replicates their MVPF estimates across various assumptions about the durability of earnings gains. For example, column (1) shows that the program provides

$2,559 of present value earnings per enrollee during a five-year sample period (averaging across exiters and graduates) at a net fiscal cost of $6,809, yielding an MVPF of 0.4. If earnings effects continue for 40 years when the typical student turns 65, the MVPF increases to 20.7 because higher mid-career earnings yields greater earnings gains and fiscal savings. This scenario is likely given that TEC appears to produce durable human capital.

Incorporating our estimates of crime reductions with the earlier estimated earnings increases further enhances the program's cost-effectiveness, even within short time horizons. In the most conservative case, we estimate that enrolling a student at TEC reduces the social cost of crime and thus increases willingness to pay by $11,152. It also generates $901 in additional fiscal savings, which reduces the net fiscal cost by 13%. Together, accounting for crime reduction benefits increases the MVPF from 0.4 to 2.3, with most of this coming from willingness to pay. Allowing for a greater social cost of crime yields a dramatically higher MVPF of 17.5.

Over longer time horizons, interactions between employment and crime reduction benefits dramatically increase cost-effectiveness. Column (4) shows a plausible scenario in which, because of the very different age profiles of earnings and crime, earnings effects persist through retirement but crime effects only happen within the observed sample period. The fiscal benefits of crime reduction are salient in this scenario: if greater taxes and less benefit spending for mid-life TEC graduates pay for much of the cost of TEC already, a roughly $900 in savings on criminal justice spending pays for about half of what remains. Overall, in this preferred specification, the MVPF of 20.7 based solely on earnings increases to 45.6 when accounting for criminal justice fiscal savings and 60.6 when conservatively accounting for all crime reduction benefits. In both cases, the additional fiscal savings makes it so the 95-percent confidence interval includes the scenario in which TEC fully pays for itself (i.e., the MVPF is ∞).

6 Conclusion

For adults who have previously exited traditional high school, graduating with a full high school diploma reduces their contact with the criminal justice system. We compare graduates of the Excel Center (TEC), a network of adult charter high schools in Indiana, to people who applied but did not enroll. Using a difference-in-differences specification that weights the data to match levels and trends in pre-application court filings, we find that graduates are 49 percent less likely to be charged in criminal court in their first year after applying for TEC and that this effect largely persists for at least five years. We do not observe declines in court filings for students who enroll in TEC but exit before graduating, which suggests that the observed effect for graduates results from the diploma and the education required to get it, rather than direct incapacitation.

The empirical specification assumes that applicants provide a valid counterfactual for graduates, conditional on past criminal justice involvement. This assumption is reasonable. Applicants and graduates have similarly selected into interest in education, and past court filings provide a rich measure of criminal justice risk. Though, it will estimate treatment effects with bias if students select out of TEC based on negative shocks. To adress this concern, we vary the information used to construct the weights, evaluate the timing of students' exits post-filing compared to a placebo group, and bound bias by examining a comparison group composed of people selecting out based on a positive employment situation.

Our results are necessarily particular to a specific set of schools. While TEC students pursue a very common and traditional high school diploma, the school itself has several unique components built to support adults, such as shortened terms, individually-tailored course schedules, and on-site childcare. Our results may not extrapolate to other adult high schools in other environments.

Still, this study indicates that it is possible to reduce criminal justice involvement by giving adult students intensive support. Most adults in the criminal justice system do not have high school diplomas. The most common educational intervention for them, the GED, has a poor track record for producing labor market benefits (Heckman et al., 2011), let alone criminal justice benefits. Educational and labor market interventions that do effectively reduce criminal justice involvement are typically built for younger participants (Belfield et al., 2006; Heller, 2014). In this context, TEC provides an example showing that programs and policy can help high-risk adults who previously dropped out of high school complete a traditional diploma and avoid the criminal justice system. And if facilitating diplomas both encourages employment (Brough et al., 2024) and reduces crime, then it becomes highly cost-effective. In a marginal value of public funds framework (Hendren and Sprung-Keyser, 2020), every net dollar spent on TEC yields roughly $60 in public benefits from participant earnings and the reduced social cost of crime, compared to about $20 when only considering earnings.

More generally, our results suggest a high social return to a high school diploma for marginal but motivated students. While education tends to generate social benefits (Lochner and Moretti, 2004; Bell et al., 2022), it is not obvious that this is true for marginal students. In a world with negative interactions with school discipline (Bacher-Hicks et al., 2024) or peer effects (Jacob and Lefgren, 2003; Carrell and Hoekstra, 2010), bringing a high-risk group of adults together in a school could even increase crime. Instead, our results indicate that the underlying return to education for such students is very high, but frictions, like lack of childcare and mismatch between students' learning gaps and curriculum, prevent them from accessing those returns. Unlocking the private and public returns to education for marginal, motivated students requires intensive support, but seems worth the investment.

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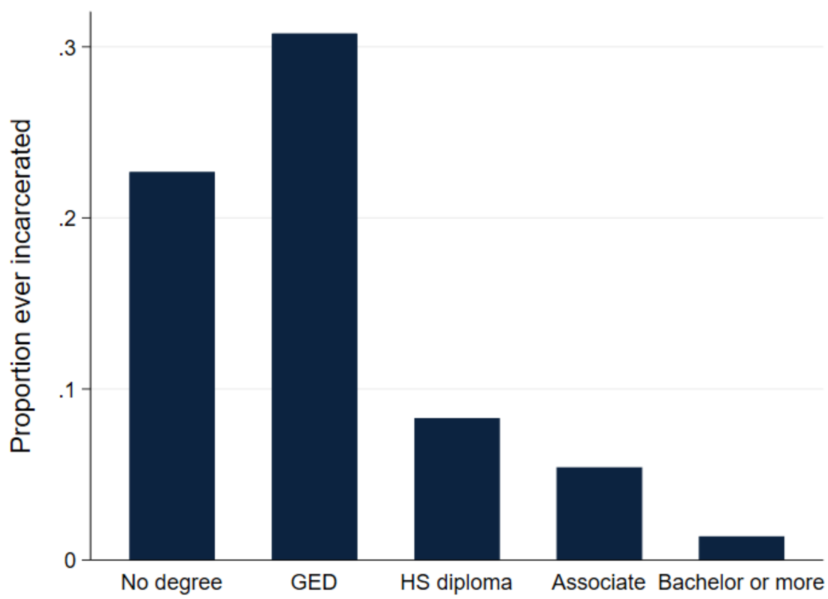
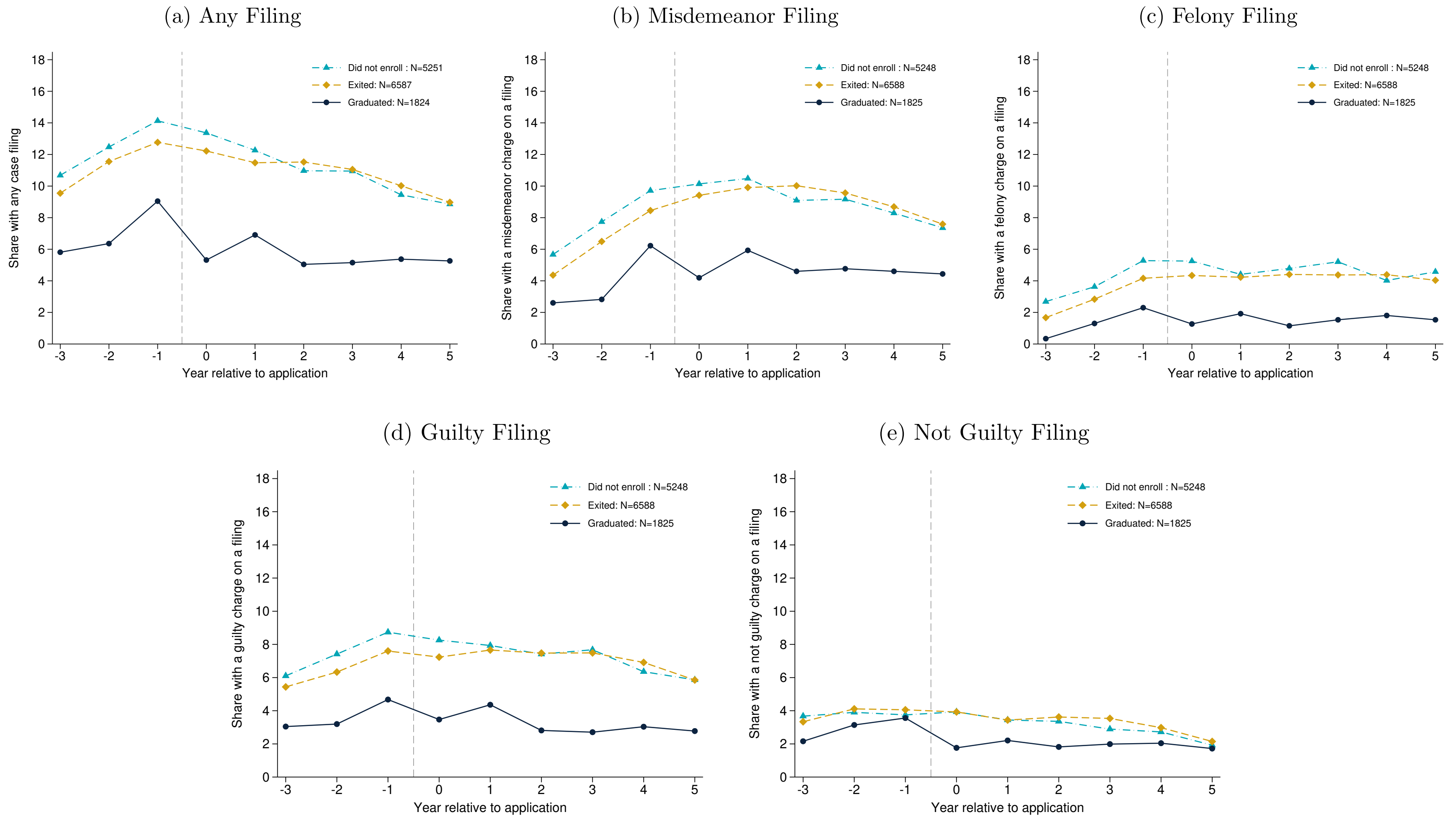
Figure 1: Incarceration Rates by Degree Attainment



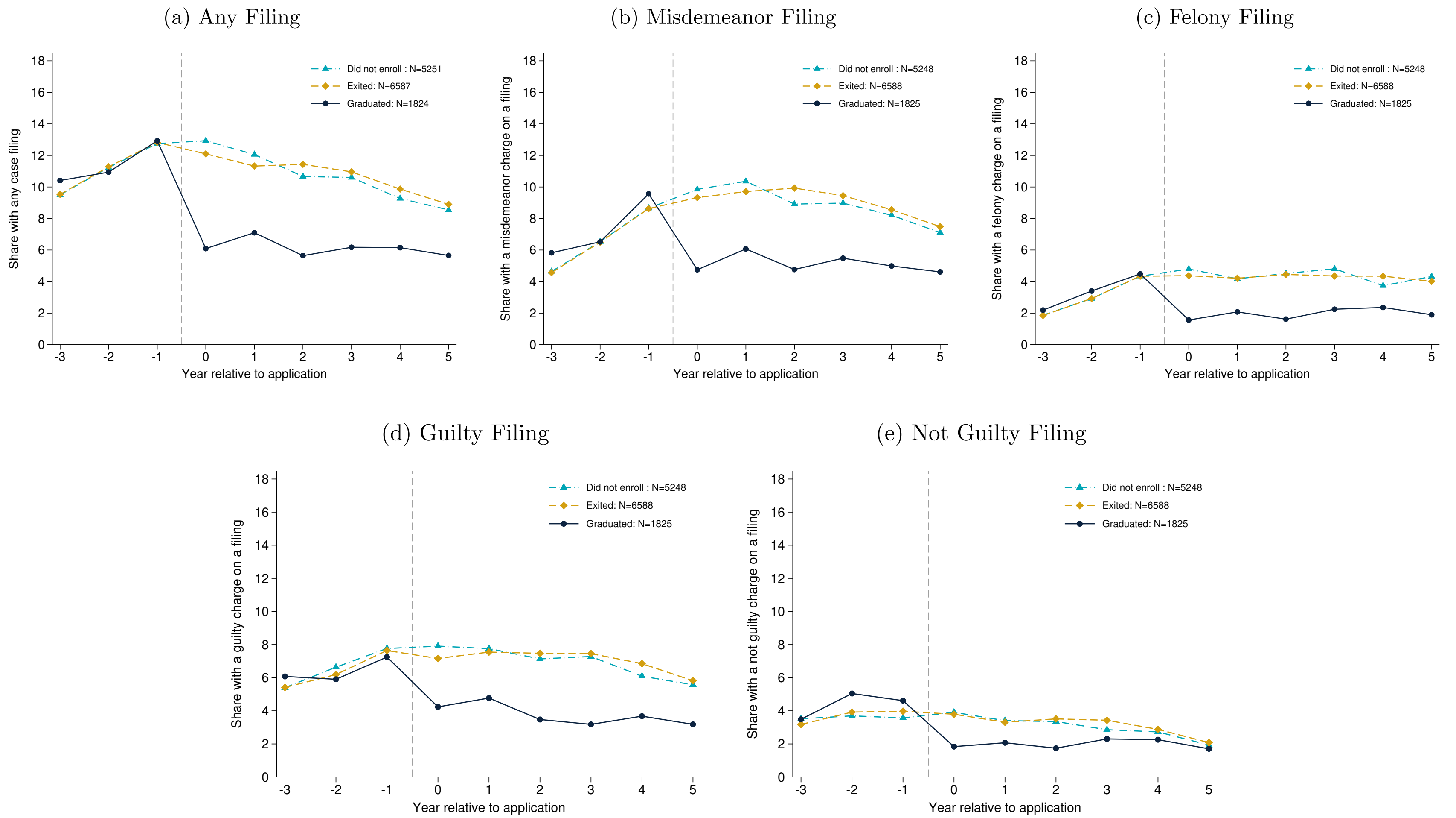
Notes: Based on authors' calculations using the National Longitudinal Survey of Youth, 1997 cohort.

Figure 2: Trends in Case Filings, by Degree and Disposition



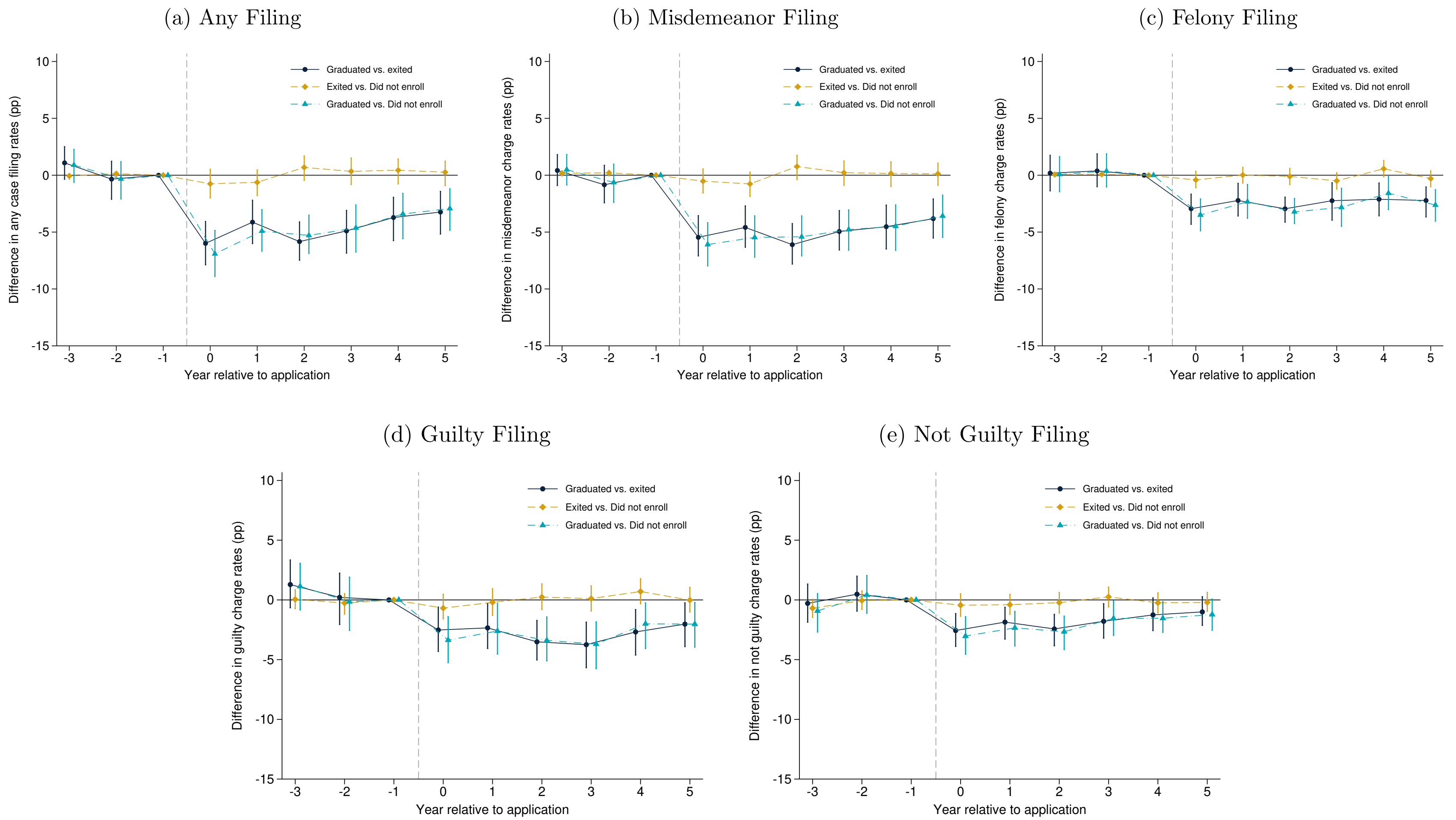
Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). This group is divided into three subsets: TEC graduates (navy circles), TEC students who enrolled but did not graduate ("exited", gold diamonds), and TEC applicants who did not enroll (teal triangles). The horizontal axis indicates year relative to initial TEC application date, where year 0 represents the first year following when an individual applied to TEC. Each panel plots the share of the group with the filing-type given in the panel title.

Figure 3: Weighted Trends in Case Filings, by Degree



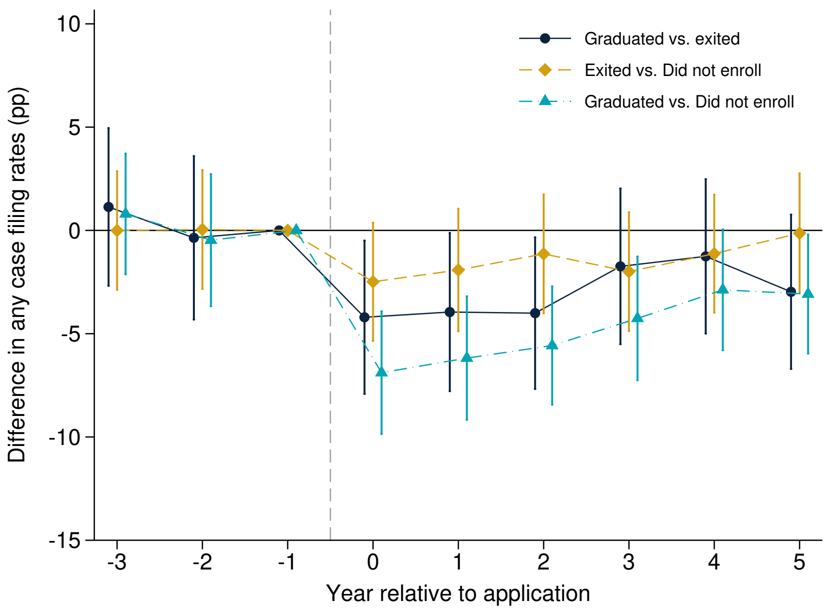
Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). This group is divided into three subsets: TEC graduates (navy circles), TEC students who did not graduate (gold diamonds), and TEC applicants who did not enroll (teal triangles). Each group is re-weighted using inverse propensity weights. See text for details. The horizontal axis indicates year relative to initial TEC application date, where year 0 represents the first year following when an individual applied to TEC. Each panel plots the weighted share of the group with the filing-type given in the panel title.

Figure 4: Weighted Event Study of Case Filings, by Degree



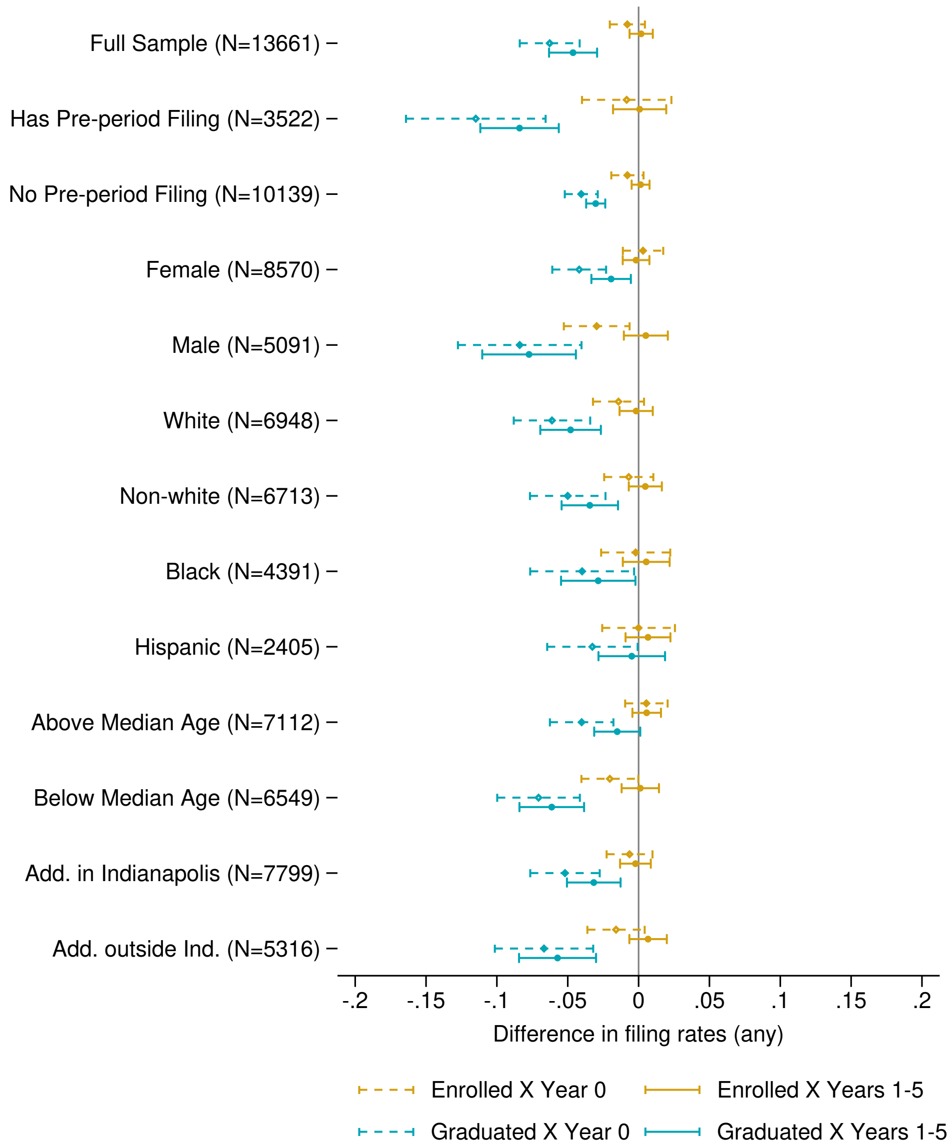
Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). This group is divided into three subsets: TEC graduates (navy circles), TEC students who did not graduate (gold diamonds), and TEC applicants who did not enroll (teal triangles). Each group is re-weighted using inverse propensity weights. The horizontal axis indicates year relative to initial TEC application date, where year 0 represents the first year following when an individual applied to TEC. Each panel plots the coefficients from a weighted event study regression that compares case rates between two groups controlling for age-bin-specific relative time fixed effects, individual fixed effects, and calendar year fixed effects. Year -1 is the reference year. Vertical bars represent 95 percent confidence intervals from a bootstrap distribution (500 iterations). Notably, these confidence intervals are less conservative than the analytical standard errors that are reported by default with the weighted event study coefficients (clustered at the individual level). All other results tables and figures report the more conservative analytical standard errors.

Figure 5: Weighted Event Study of Any Court Filing, Exit Reason is a Work Conflict



Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). This group is divided into three subsets: TEC graduates (navy circles), TEC students who enrolled but did not graduate specifically because of work conflicts (gold diamonds), and TEC applicants who did not enroll (teal triangles). Each group is re-weighted using inverse propensity weights. See text for details. The horizontal axis indicates year relative to initial TEC application date, where year 0 represents the first year following when an individual applied to TEC. Each panel plots the coefficients from a weighted event study regression that compares case rates between two groups controlling for age-bin-specific relative time fixed effects, individual fixed effects, and calendar year fixed effects. Year -1 is the reference year. Vertical bars represent 95 percent confidence intervals calculate from standard errors clustered by individual.

Figure 6: Effect of Enrollment and Graduation from The Excel Center on Case Filings, by Subgroup and Years



Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. For different subgroups of the main sample, we report the same four coefficients (see text for details). The top row replicates results reported elsewhere for the main sample. A "pre-period filing" refers to a filing occurring for a student in the three years prior to their application to TEC. "Add. in Indianapolis" refers to the subgroup whose address at the time of application was within Indianapolis and vice versa for "Add. outside" of Indianapolis. The plotted results are the same as those reported in Table 6. The coefficients are generated from a weighted event study regression that compares case rates between two groups controlling for age-bin-specific relative time fixed effects, individual fixed effects, and calendar year fixed effects. Horizontal bars represent 95 percent confidence intervals, calculated from standard errors clustered by individual.

Table 1: Court Filing Rates by Group: Indiana versus TEC Sample, 2010

	Indiana, 2010		TEC Sample, 2010	
	(1)	N	(2)	N
Total pop $\geq$ 15 yrs old	0.034	5152735	0.107	3255
Males $\geq$15 yrs old	0.045	2509222	0.156	1237
Females $\geq$15 yrs old	0.016	2643513	0.077	2018
15-17 Years Old	0.006	277231	0.040	593
18-19 Years Old	0.089	198284	0.152	462
20-24 Years Old	0.084	452026	0.130	821
25-39 Years Old	0.057	1244356	0.117	1056
40-59 Years Old	0.021	1789102	0.077	312

Notes: Data for column (1) come from 2010 decennial Census population counts at the state level (downloaded from the National Historical Geographic Information System) and 2010 filings in administrative court records provided by the Indiana Office of Court Services (IOCS). Data for column (2) come from TEC application records linked to IOCS records. These records are then restricted to the main analysis sample, and further limited to observations occurring in 2010. Filing rates for a group are defined as the number of case filing records for that group divided by the total persons for that group. For example, in the first row ("total population") of column (1), the filing rate of 0.034 is the number of filings we see in the IOCS data for people at least 15 years old in 2010 divided by the total number of people at least 15 years old in Indiana in 2010.

Table 2: Descriptive Differences Between Applicants to the Excel Center

	Unweighted			Weighted				
	Did not enroll	Exited	Graduated	Did not enroll	Exited	Graduated	Differences	
							(5) − (4)	(6) − (4)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full Sample (N = 13,662)								
Age	29.3	28.4	27.0	28.6	28.6	28.3	0.0	-0.3
Male	0.41	0.36	0.29	0.37	0.37	0.37	0.00	-0.00
Female	0.59	0.64	0.71	0.63	0.63	0.63	-0.00	0.00
White (Non-Hispanic)	0.51	0.49	0.51	0.50	0.50	0.50	0.00	0.00
Black (Non-Hispanic)	0.29	0.33	0.29	0.31	0.31	0.31	-0.00	0.00
Hispanic	0.17	0.15	0.15	0.16	0.16	0.15	0.00	-0.00
	0.49	0.51	0.49	0.50	0.50	0.50	-0.00	-0.00
In the three years pre-application:								
Any case filing	0.28	0.26	0.17	0.26	0.26	0.25	0.00	-0.01
Any misdemeanor filing	0.18	0.16	0.10	0.16	0.16	0.16	-0.00	0.00
Any felony filing	0.10	0.08	0.04	0.08	0.08	0.08	-0.00	-0.00
Any guilty filing	0.18	0.16	0.10	0.16	0.15	0.15	-0.00	-0.01
Any not guilty filing	0.10	0.10	0.08	0.09	0.10	0.11	0.00	0.01

Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). This group is divided into three subsets: TEC applicants who did not enroll (e.g., column (1)), TEC students who applied but did not graduate (e.g., column (2)), and TEC graduates (e.g., column (3)). The first three columns report raw, unweighted means for different variables, denoted by row titles. The next three columns report means after applying inverse propensity weights. Columns (7) and (8) report differences in means between columns (5) and (4), and columns (6) and (4), respectively. Statistical significance for the differences in columns (7) and (8) are reported at the 10, 5, and 1 percent levels and are denoted respectively by *, **, and ***. In this case, no difference had an associated test statistic that crossed these thresholds.

Table 3: Effect of Enrollment and Graduation from The Excel Center on Case Filings

	Weighted SD (1)	Weighted SD (2)	Weighted (3)	Weighted (4)	Weighted (5)
Enrolled X Year 0	-0.021***	-0.008	-0.009	-0.008	-0.008
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Enrolled X Year 1	-0.016***	-0.007	-0.008	-0.007	
	(0.006)	(0.006)	(0.006)	(0.006)	
Enrolled X Year 2	-0.005	0.008	0.007	0.007	
	(0.005)	(0.006)	(0.006)	(0.006)	
Enrolled X Year 3	-0.007	0.004	0.003	0.003	
	(0.005)	(0.006)	(0.006)	(0.006)	
Enrolled X Year 4	-0.002	0.006	0.006	0.005	
	(0.005)	(0.005)	(0.006)	(0.006)	
Enrolled X Year 5	-0.004	0.004	0.003	0.003	
	(0.005)	(0.005)	(0.006)	(0.006)	
Enrolled X Years 1-5					0.002
					(0.004)
Graduated X Year 0		-0.060***	-0.062***	-0.063***	-0.063***
		(0.008)	(0.011)	(0.011)	(0.011)
Graduated X Year 1		-0.042***	-0.044***	-0.045***	
		(0.008)	(0.011)	(0.011)	
Graduated X Year 2		-0.058***	-0.060***	-0.061***	
		(0.008)	(0.011)	(0.011)	
Graduated X Year 3		-0.048***	-0.050***	-0.051***	
		(0.009)	(0.011)	(0.011)	
Graduated X Year 4		-0.037***	-0.039***	-0.040***	
		(0.008)	(0.011)	(0.011)	
Graduated X Year 5		-0.032***	-0.034***	-0.035***	
		(0.007)	(0.011)	(0.011)	
Graduated X Years 1-5					-0.046***
					(0.009)
Relative Year FE	X	X	X	X	X
Person FE			X	X	X
Calendar Year FE				X	X
Age Bin X Relative Year FE				X	X
Comp. Mean-Year 0	0.129	0.129	0.129	0.129	0.129
Comp. Mean-Years 1-5	0.102	0.102	0.102	0.102	0.102
R^2	0.002	0.004	0.267	0.271	0.271
Observations	81,972	81,972	122,958	122,958	122,958
Individuals	13,662	13,662	13,662	13,662	13,662
P-val. Pre-trend: Enrolled	0.982	0.999	0.997	0.932	0.932
P-val. Pre-trend: Graduated		0.832	0.650	0.548	0.548

Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). Time is measured in years relative to application date, and the data are a balanced panel from year -3 to year 5. The outcome is whether an individual had any case filing in the given relative year. All columns are re-weighted using inverse propensity score weights. Columns (1) and (2) are single-difference specifications that only include the five post-period years. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***. Comparison means are calculated in the noted years for the "applicant" group within the full sample. See text for details about pre-trend tests conducted and the associated p-values reported.

Table 4: Effect of Enrollment and Graduation from The Excel Center, by Degree and Disposition

	Any Filing	Misdemeanor Filing	Felony Filing	Guilty Filing	Not Guilty Filing
	(1)	(2)	(3)	(4)	(5)
Enrolled X Year 0	-0.008	-0.007	-0.004	-0.007	-0.002
	(0.006)	(0.006)	(0.004)	(0.005)	(0.004)
Enrolled X Years 1-5	0.002	0.000	-0.001	0.002	0.001
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)
Graduated X Year 0	-0.063***	-0.054***	-0.031***	-0.031***	-0.027***
	(0.011)	(0.011)	(0.008)	(0.009)	(0.006)
Graduated X Years 1-5	-0.046***	-0.047***	-0.025***	-0.034***	-0.018***
	(0.009)	(0.009)	(0.006)	(0.007)	(0.005)
Relative Year FE	X	X	X	X	X
Person FE	X	X	X	X	X
Calendar Year FE	X	X	X	X	X
Age Bin X Relative Year FE	X	X	X	X	X
Comp. Mean-Year 0	0.129	0.098	0.048	0.080	0.039
Comp. Mean-Years 1-5	0.102	0.087	0.043	0.068	0.029
R^2	0.271	0.251	0.233	0.257	0.187
Observations	122,958	121,295	121,234	120,846	120,846
Individuals	13,662	13,662	13,662	13,662	13,662
$\Pr(\hat{\beta}\_Enroll^{Pre1-2} = 0)$	0.932	0.899	0.995	0.731	0.222
$\Pr(\hat{\beta}\_Grad^{Pre1-2} = 0)$	0.548	0.584	0.901	0.586	0.661

Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). Time is measured in years relative to application date, and the data are a balanced panel from year -3 to year 5. The outcome is denoted by the column header. All specifications are re-weighted using inverse propensity score weights. The specifications for each column in this table are the same as for column (5) in Table 3. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***. Comparison means are calculated in the noted years for the "applicant" group within the full sample. See text for details about pre-trend tests conducted and the associated p-values reported.

Table 5: Effect of Enrollment and Graduation from The Excel Center, by Crime Type

	Any Filing	Criminal Traffic	Any Non-Traffic	DUI	Drug	Property	Public Order	Violent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enrolled X Year 0	-0.008	-0.004	-0.007	-0.003*	0.002	-0.004	-0.007**	-0.005
	(0.006)	(0.003)	(0.006)	(0.002)	(0.004)	(0.004)	(0.004)	(0.003)
Enrolled X Years 1-5	0.002	-0.002	0.002	-0.001	0.000	0.001	0.002	0.001
	(0.004)	(0.002)	(0.004)	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)
Graduated X Year 0	-0.063***	-0.018***	-0.057***	0.003	-0.030***	-0.018***	-0.014**	-0.015***
	(0.011)	(0.006)	(0.010)	(0.003)	(0.007)	(0.005)	(0.006)	(0.005)
Graduated X Years 1-5	-0.046***	-0.014***	-0.041***	-0.002	-0.024***	-0.007**	-0.014***	-0.013***
	(0.009)	(0.005)	(0.009)	(0.002)	(0.006)	(0.004)	(0.004)	(0.004)
Relative Year FE	X	X	X	X	X	X	X	X
Person FE	X	X	X	X	X	X	X	X
Calendar Year FE	X	X	X	X	X	X	X	X
Age Bin X Relative Year FE	X	X	X	X	X	X	X	X
Comp. Mean-Year 0	0.129	0.027	0.118	0.007	0.043	0.046	0.036	0.029
Comp. Mean-Years 1-5	0.102	0.028	0.090	0.008	0.038	0.029	0.027	0.023
R^2	0.271	0.199	0.258	0.146	0.224	0.193	0.212	0.182
Observations	122,958	122,958	122,958	122,958	122,958	122,958	122,958	122,958
Individuals	13,662	13,662	13,662	13,662	13,662	13,662	13,662	13,662
$\Pr(\hat{\beta}\_Enroll^{Pre1-2} = 0)$	0.932	0.664	0.964	0.788	0.955	0.954	0.321	0.360
$\Pr(\hat{\beta}\_Grad^{Pre1-2} = 0)$	0.548	0.806	0.614	0.278	0.337	0.861	0.068	0.333

Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). Time is measured in years relative to application date, and the data are a balanced panel from year -3 to year 5. The outcome is denoted by the column header. All specifications are re-weighted using inverse propensity score weights. The specifications for each column in this table are the same as for column (5) in Table 3. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***. Comparison means are calculated in the noted years for the "applicant" group within the full sample. See text for details about pre-trend tests conducted and the associated p-values reported.

Table 6: Effect of Enrollment and Graduation from The Excel Center on Case Filings, by Subgroups

	Enrolled Year 0 (1)	Enrolled Years 1-5 (2)	Graduated Year 0 (3)	Graduated Years 1-5 (4)	Comp. Mean Year 0	Comp. Mean Years 1-5	R^2	Obs.	Indvdls.	$\Pr(\hat{\beta}\_Enroll_{Pre1-2} = 0)$	$\Pr(\hat{\beta}\_Grad_{Pre1-2} = 0)$
Full Sample	-0.008	0.002	-0.063***	-0.046***	0.129	0.102	0.271	122958	13662	0.932	0.548
	(0.006)	(0.004)	(0.011)	(0.009)							
Males	-0.029**	0.006	-0.084***	-0.077***	0.193	0.144	0.278	45828	5092	0.922	0.407
	(0.012)	(0.008)	(0.022)	(0.017)							
Females	0.003	-0.001	-0.043***	-0.021***	0.091	0.077	0.252	77130	8570	0.986	0.959
	(0.007)	(0.005)	(0.010)	(0.007)							
Pre-app Filing	-0.010	0.000	-0.114***	-0.083***	0.269	0.205	0.233	31698	3522	0.743	0.438
	(0.016)	(0.010)	(0.025)	(0.014)							
No Pre-app Flng.	-0.008	0.002	-0.041***	-0.031***	0.080	0.067	0.231	91260	10140		
	(0.006)	(0.003)	(0.006)	(0.003)							
White (non-Hisp)	-0.012	-0.001	-0.063***	-0.051***	0.138	0.109	0.272	61524	6836	0.939	0.661
	(0.009)	(0.006)	(0.014)	(0.011)							
Non-white	-0.003	0.006	-0.049***	-0.031***	0.120	0.095	0.272	61434	6826	0.998	0.816
	(0.009)	(0.006)	(0.014)	(0.010)							
> Median Age	0.004	0.005	-0.039***	-0.014*	0.094	0.073	0.263	64116	7124	0.964	0.462
	(0.008)	(0.005)	(0.011)	(0.008)							
< Median Age	-0.019*	0.002	-0.071***	-0.061***	0.169	0.134	0.267	58842	6538	0.961	0.810
	(0.010)	(0.007)	(0.015)	(0.012)							
Add. In Indy	-0.007	-0.002	-0.051***	-0.031***	0.120	0.097	0.269	70128	7792	0.987	0.847
	(0.008)	(0.006)	(0.012)	(0.010)							
Add. Out. Indy	-0.015	0.007	-0.068***	-0.060***	0.142	0.107	0.282	47925	5325	0.950	0.499
	(0.010)	(0.007)	(0.018)	(0.015)							

Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The "Full Sample" includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). Rows denote subsections of this full sample. Time is measured in years relative to application date, and the data are a balanced panel from year -3 to year 5. The outcome is whether a student had a case filing in a given relative year. All specifications are re-weighted using inverse propensity score weights that are generated for each particular subgroup. Otherwise the specifications for each column in this table are the same as for column (5) in Table 3. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***. Comparison means are calculated in the noted years for the "applicant" group within the full sample. See text for details about pre-trend tests conducted and the associated p-values reported. Filing rates for the "no pre-application filing" subgroup are mechanically zero, and as such p-values from a pre-trend test are not reported for this group.

Table 7: Marginal Value of Public Funds Calculations

Outcomes:	5-Year Earnings Impacts (1)	7-Year Earnings Impacts (2)	10-Year Earnings Impacts (3)	40-Year Earnings Impacts (4)	40-Year Earnings Impacts (5)	40-Year Earnings Impacts (6)
Willingness to Pay						
Earnings effects only	2,559	4,907	8,415	34,063	24,073	26,680
	[1,277, 3,866]	[2,895, 6,931]	[5,319, 11,497]	[22,120, 45,787]	[15,570, 32,406]	[17,544, 36,014]
With less inclusive social cost of crime effect	13,711	16,059	19,567	45,215	34,906	37,832
	[4,291, 23,296]	[6,381, 25,854]	[9,322, 29,473]	[30,112, 60,441]	[22,473, 47,396]	[24,670, 50,780]
With more inclusive social cost of crime effect	103,063	105,411	108,919	134,567	122,420	127,184
	[28,237, 178,215]	[30,991, 180,397]	[32,767, 184,285]	[59,207, 209,312]	[49,507, 197,012]	[51,986, 204,124]
Cost Net of Fiscal Savings						
Earnings effects only	6,809	6,560	6,188	1,648	3,330	4,248
	[6,703, 6,913]	[6,376, 6,743]	[5,888, 6,489]	[64, 3,259]	[2,229, 4,452]	[3,279, 5,197]
With fiscal cost of crime effect	5,908	5,659	5,287	747	2,452	3,347
	[5,239, 6,556]	[4,986, 6,329]	[4,579, 6,025]	[-984, 2,468]	[1,177, 3,737]	[2,200, 4,514]
MVPF						
Earnings effects only	0.38	0.75	1.36	20.67	7.23	6.28
	[0.18, 0.58]	[0.43, 1.09]	[0.82, 1.95]	[6.79, 694.00]	[3.50, 14.53]	[3.38, 10.98]
With fiscal cost of crime effect	0.43	0.87	1.59	45.63	9.82	7.97
	[0.21, 0.68]	[0.49, 1.29]	[0.93, 2.35]	[9.20, ∞]	[4.28, 26.94]	[4.05, 15.92]
With fiscal and less inclusive social cost of crime effects	2.32	2.84	3.70	60.56	14.23	11.30
	[0.66, 4.44]	[1.01, 5.16]	[1.55, 6.44]	[12.46, ∞]	[6.07, 39.63]	[5.51, 22.97]
With fiscal and more inclusive social cost of crime effects	17.45	18.63	20.60	180.25	49.92	38.00
	[4.32, 33.61]	[4.92, 35.83]	[5.53, 39.38]	[31.74, ∞]	[14.97, 146.81]	[12.58, 84.16]
Discount Rate	3%	3%	3%	3%	5%	3%
Earnings Extrapolation Method	Actual	Lifecycle	Lifecycle	Lifecycle	Lifecycle	Constant

Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services and MVPF results reported in Brough et al. (2024). The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). The table reports estimates of the willingness to pay of the Excel Center, the cost of serving the typical enrolled student net of fiscal savings, and the marginal value of public funds (MVPF). We combine estimates for enrolled TEC students reported in Appendix Table A-11 of Brough et al. (2024) with five-year cost of crime impact estimates from Table A-8. Results across columns vary the assumed duration of earnings impacts (given by the column header), the discount rate, and the method for extrapolating earnings gains into the future. The lifecycle extrapolation method (columns 2 through 5) assumes Year-5 earnings gains are held constant in proportion to the earnings of non-enrolled applicants, where non-enrolled applicant earnings follow the population age-earnings profile of high school non-completers observed in the 2015 ACS (Ruggles et al., 2020) The constant extrapolation method assumes the Year-5 earnings gains are held constant in dollar terms throughout the remainder of the time horizon. 95% confidence intervals are reported in brackets and come from 10,000 bootstrap samples of the joint estimate distribution. See text and Hendren and Sprung-Keyser (2020) for additional details.

A Appendix

A.1 Review of CJARS classifications

We rely on the CJARS classification tool for most filing types in the IOCS data, but also reviewed the classifications and made two changes to better reflect statutes within Indiana and the general prevalence of different charges in Indiana. For review, we used data from the National Incident Based Reporting System (NIBRS) to generate a ranked list of offense types by category in Indiana and compared this list to the relative prevalence of different offense types in the IOCS data, as categorized by the CJARS tool. Comparing these lists, we noted that the CJARS classifications led to an unusually high proportion of charges being labeled as "aggravated assault" as opposed to "simple assault", despite the fact that the latter are generally much more common in Indiana. Through the same process, we also noted that CJARS classifications alone led to an unusually low prevalence of "larceny/theft" and an unusually high prevalence of "fraud/forgery."

To adjust the classifications regarding assault, we reviewed the charges labeled by CJARS as "aggravated assault" and reassigned their type to "simple assault" unless the charge description or attached statutes referred to any bodily harm, use of a weapon, or a degree higher than a "level 6" felony. To adjust the classifications regarding theft and fraud, we reassigned charge labels from "fraud/forgery" to "larceny/theft" when the charge had an attached statute mentioning "conversion", which is listed under "theft" and "receiving stolen goods" in Indiana Code.

A.2 Tract-level Census Information

Data at the tract level is downloaded for ACS years 2006-2010, 2007-2011, 2008-2012, 2009-2013, 2010-2014, 2011-2015, 2012-2016, 2013-2017, 2014-2018, and 2015-2019, and data at the block group level is downloaded for years 2016-2020, 2017-2021 and 2018-2022 from the National Historical Geographic Information System (NHGIS).[17] This information is used when imputing sex and race as well as in the "expanded" list of variables used to generate inverse propensity weights (see text or Table A-5 for details). For an individual who applied to TEC in 2010, their relevant tract-level information is matched to ACS estimates for 2006-2010, and so on. The specific tract associated with each student is based on their address at the time of application, geolocated to 2010 census tract boundaries. ACS estimates from 2016-2020 onwards are standardized to 2010 census tract boundaries following NHGIS guidance for geographic crosswalks. Namely, this involves downloading estimates at the block group level, connecting these block group estimates to a crosswalk between 2020 and 2010 block group boundaries, and then aggregating to the 2010 tract level.

A.3 Applicant Sex and Race

Individuals who applied to TEC prior to 2017 (about 76% of the analysis sample) do not have sex or race recorded in TEC base files. For these applicants, we generate continuous variables representing the probability that a student belongs to a particular sex or race group. These variables are created

[17]Jonathan Schroeder, David Van Riper, Steven Manson, Katherine Knowles, Tracy Kugler, Finn Roberts, and Steven Ruggles. IPUMS National Historical Geographic Information System: Version 20.0 [dataset]. Minneapolis, MN: IPUMS. 2025. http://doi.org/10.18128/D050.V20.0

by following procedures similar to those detailed by the Consumer Financial Protection Bureau (2014).

For example, to generate the probability that an applicant is female, we first use records from the Social Security Administration (SSA) to gather counts of first names by sex in a given birth year. This generates the probability that a student is female given their first name and birth year. For about 96% of names in this file, sex is certain (i.e., $p\{female|\text{first name, birth year}\} = 0$ or 1). For others, like the name "Carey" in 1971, we know that about 64% of individuals were female (and thus $p\{female|\text{first name, birth year}\} = 0.64$). Next, we gather ACS 5-year estimates of counts of population by sex at the tract level (downloaded from the National Historical Geographic Information System). These are linked to TEC records using the individual's application year and address, which is geolocated to 2010 census tract definitions. These counts generate the probability that a student lives in a particular census tract given their application year and that they are female. With this information in hand, the final probability that a student is female (given their first name n, birth year y, census tract t, and application year a) is calculated using Bayes' Theorem as:

$$pr\{female|n, y, t, a\} = \frac{p\{female|n, y\} * q\{t|female, a\}}{\sum_{female,male} p * q}$$

The same process is followed to generate the probability that a student belongs to one of six race groups, but uses surnames instead of first names (with data from the Census Bureau) and counts of population by race group. The race groups used are Hispanic, White, Black, Asian/Pacific Islander, American Indian/Alaska Native, and Multiracial.

Ultimately, about 75% of the TEC records that didn't have sex recorded were able to have pr{female | n, y, t, a} generated, versus 25% who remain without this variable labeled because of either (1) their name not matching to SSA records, or (2) their address not being able to be geolocated. About 92% of the TEC records that didn't have race recorded were able to have pr{race = r | n, y, t, a} generated. In subgroup analysis that divided the sample by sex or race, the individual is assigned to the group for which they have the greatest probability assigned.

A.4 Risk Score Variables

To the extent that data availability allows, we follow the Indiana Risk Assessment System's (IRAS) Pretrial Assessment Tool to construct "risk score" variables for each student. For example, we create indicators for whether a student had a filing before age 33, whether a student has three or more more prior filings before their application TEC, and the number of drug-related court filings the student had before their application to TEC. These are meant to substitute for "younger than 33 at first arrest", "three or more prior jail incarcerations", and "illicit drug use in the past six months", which we cannot perfectly replicate with our court records. Following the example of other risk assessment questionnaires in Indiana (e.g., the "community supervision tool") we also construct variables denoting the most serious type of filing the student has had prior to their application, and the number of felony filings the student had prior to their application.

A.5 Matching Procedure between Excel Center and Court Records

To link data about TEC applicants to data about criminal justice system contact from IOCS records, we first match exactly by name and date of birth. Among the applicants who have a match with court records in our data, about 92% are matched exactly. However, only using the results from an exact match between TEC and IOCS files could be ignoring cases where there were data entry issues (e.g., misspellings of names) in either file.

For those applicants who are yet to have a match, we use the *dtalink* package in Stata to consider all possible combinations of records from the TEC and court files, and assign a "quality score" to each potential match based on a list of characteristics (Kranker, 2018). Specifically, we conduct *dtalink* using first name (5 points for an exact match, -3 for no match), last name (7 points for an exact match, -3 for no match), birth date (8 points for an exact match, -2 for no match), *soundex* code for first name (4 for an exact match, -3 for no match), *soundex* code for last name (6 for an exact match, -3 for no match), month and year of birth (7.5 for an exact match, -3 for no match), sex (2 for an exact match, 0 for no match), and race (0.5 for an exact match, 0 for no match).

We use the scores from *dtalink* to partition all potential matches into files that can each be more easily manually screened. Based on this manual review, we kept no "fuzzy" matches with scores less than or equal to 10. For potential matches with scores greater than 10, we further exclude pairs that do not at least have the first or last three letters of their first or last names matching. Among all of the matches to court records, just under eight percent are from fuzzy matches.

A.6 Exit Timing Relative to Filing Events

As discussed in Section 4.3, we may be concerned about reverse causality – that criminal justice involvement directly causes TEC applicants to exit the program. While we cannot directly test this mechanism, we can examine the timing of student exits relative to court filings to assess its plausibility. Specifically, we compare exit rates of students with filings to those of a matched comparison group with randomly assigned "placebo" filing dates. If enrollees systematically exited more quickly after their filings, this would suggest reverse causality plays a significant role.

We first identify exiters who had at least one filing during TEC enrollment, using TEC's entry and exit date records. These students form our "treated group," with their first filing during enrollment serving as the "event date" in Figure A-2.

We then construct a comparison group using propensity score matching. First, we run a logit specification estimating the probability of having a filing during enrollment using student demographics (age at application, indicators for "female" and different race categories), Census tract characteristics (total population, median income, percentage of the population that is White, the percentage of individuals over 25 with a college degree or higher, and the civilian employment rate), the total number of filings a student had in the three years prior to their application, and the length of their enrollment at TEC. We then match each treated exiter to the closest propensity score match from exiters who (1) had no filings during enrollment and (2) were enrolled for at least six months. We allow for a comparison student to serve as a control multiple times as needed (as opposed to selecting without replacement, and degrading match quality). About 67% of the comparison matches are unique students. "Event dates" for the comparison group are chosen as a random day within their enrollment period.

Figure A-2 examines final exit timing for both groups over a 6-month post-period. Panel (a) shows raw exit rates by relative week, while Panel (b) presents simple difference-in-differences coefficients using week -1 as the reference. We omit earlier pre-periods because exit rates are mechanically zero before the event date for both groups. This is because we are specifically focusing on filings that occur after enrollment but *before* a student's final exit (and we are randomly selecting the comparison group's event date to be before their exit as well) – so, there are no cases where a final exit occurs before the event date.

In Figure A-2, we see that students with filings during enrollment have similar exit timing compared to the matched comparison group with placebo filing dates. Moreover, we note that exit rates are generally low for exiters that have a filing during their enrollment – over 75% of these students remain enrolled after six months. While these results cannot definitively rule out reverse causation, they suggest its influence is limited. For further robustness checks, please see Section 4.3.

Table A-1: Disposition List, with Assigned Labels for "Guilty" vs. "Not Guilty"

Disposition description (from IOCS data)	Labeled as a "guilty" charge?
Adjudicated CHINS	No
Adjudicated Delinquent: Lesser Included	Yes
Admission	Yes
Charge Added in Error	No
Conversion Unknown	Unknown (label left missing)
Conviction Merged	Yes
Default Judgment	Yes
Dismissed	No
Dismissed Without Prejudice	No
Dismissed with Prejudice	No
Disposition Removed Due to Clerical Error	Unknown (label left missing)
Finding of Aggravated Circumstances	Yes
Finding of Dangerous	Yes
Finding of Guilty	Yes
Finding of Guilty Lesser Included	Yes
Finding of Guilty but Mentally Ill	Yes
Finding of Not Dangerous	No
Finding of Not Guilty	No
Guilty Verdict Added, Counts Merged	Yes
Judgment Against Defendant	Yes
Judgment Set Aside	No
Judgment for Defendant	No
Null	Unknown (label left missing)
Nolo Contendere	Yes
Not Responsible By Reason Of Insanity	No
Plea By Agreement but Mentally Ill	Yes
Plea Guilty	Yes
Plea Guilty Lesser Included	Yes
Plea Guilty Lesser Included but Mentally Ill	Yes
Plea Guilty but Mentally Ill	Yes
Plea by Agreement	Yes
Remanded	No
Reversed	No
Reversed and Remanded	No
Vacated	No
Vacated (Trial De Novo)	No

Notes: List from disposition descriptions contained in IOCS administrative court records.

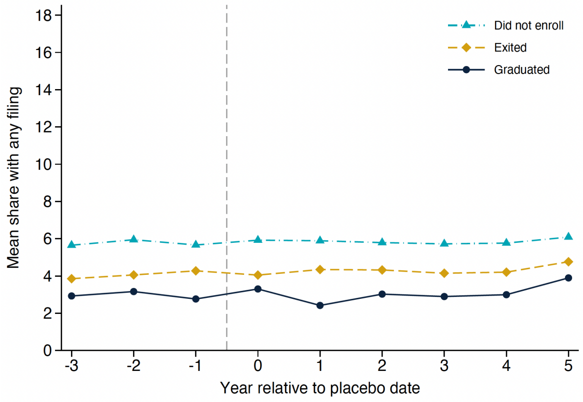
Table A-2: Degree List, with Assigned Labels for Misdemeanor, Felony, Infraction

Degree description (from IOCS data)	Labeled as a ”felony” charge?	Labeled as a ”misdemeanor” charge?	Labeled as an ”infraction” charge?
Felony	Yes	No	No
Felony 1	Yes	No	No
Felony 2	Yes	No	No
Felony 3	Yes	No	No
Felony 4	Yes	No	No
Felony 5	Yes	No	No
Felony 6	Yes	No	No
Felony A	Yes	No	No
Felony B	Yes	No	No
Felony C	Yes	No	No
Felony D	Yes	No	No
Murder	Yes	No	No
Misdemeanor	No	Yes	No
Misdemeanor Class A	No	Yes	No
Misdemeanor Class B	No	Yes	No
Misdemeanor Class C	No	Yes	No
Misdemeanor Class D	No	Yes	No
Infraction Class A	No	No	Yes
Infraction Class B	No	No	Yes
Infraction Class C	No	No	Yes
Infraction Class D	No	No	Yes
No State Code	Unknown (label left missing)	Unknown (label left missing)	Unknown (label left missing)
“Degree Code”	Unknown (label left missing)	Unknown (label left missing)	Unknown (label left missing)

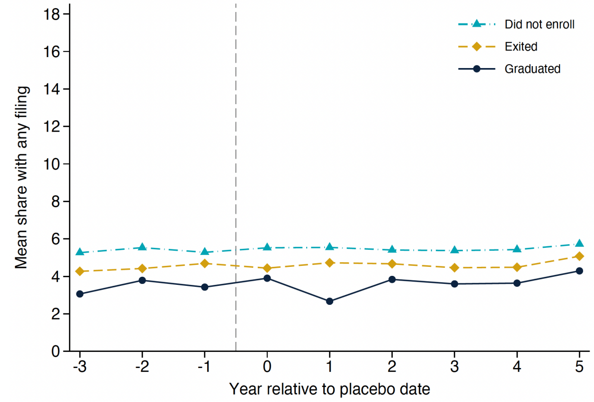
Notes: List from degree descriptions contained in IOCS administrative court records.

Figure A-1: Trends, Weighted Trends, and Weighted Event Study, Placebo Samples

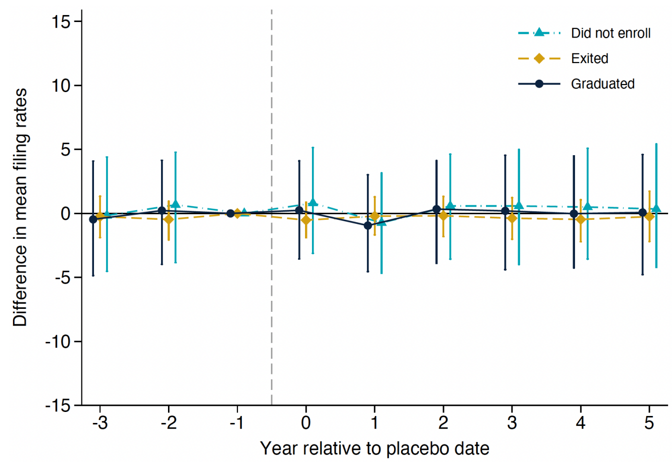
(a) Trends in Case Filings



(b) Weighted Trends in Case Filings



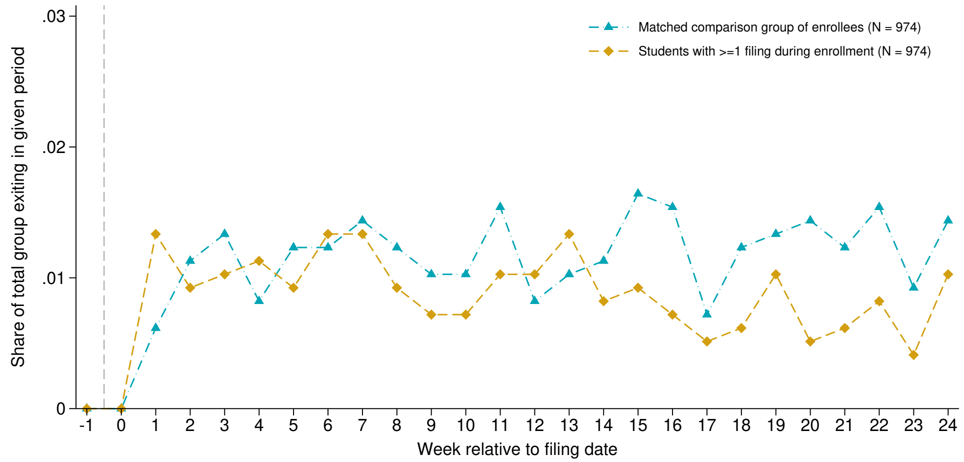
(c) Weighted Event Study



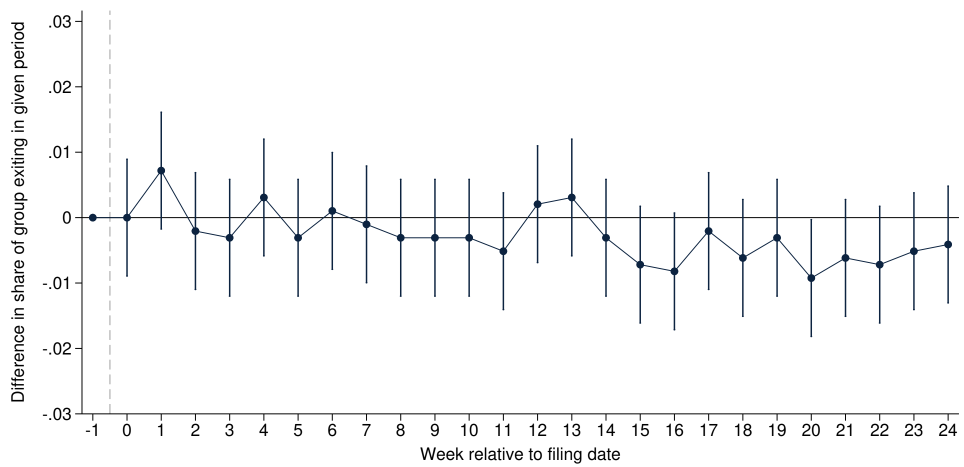
Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2019 through June 2024 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). That group is divided into three groups: TEC graduates (navy circles), TEC students who did not graduate (gold diamonds), and TEC applicants who did not enroll (teal triangles). Each group is re-weighted using inverse propensity weights. The horizontal axis indicates year relative to a randomly assigned TEC application date in the student's actual pre-period, where year 0 represents the first year following the randomly assigned application date. Each panel plots the coefficients from a weighted event study regression that compares case rates between two groups controlling for age-bin-specific relative time fixed effects, individual fixed effects, and calendar year fixed effects. Year -1 is the reference year. Vertical bars represent the 95 percent empirical distribution from randomly assigning application dates and generating regression estimates over 500 iterations. See Section 4.3 for more detail about the construction and procedure of these results.

Figure A-2: Exit Rates Relative to Filing Event: Matched Comparison Group

(a) Trends in Exit Rates

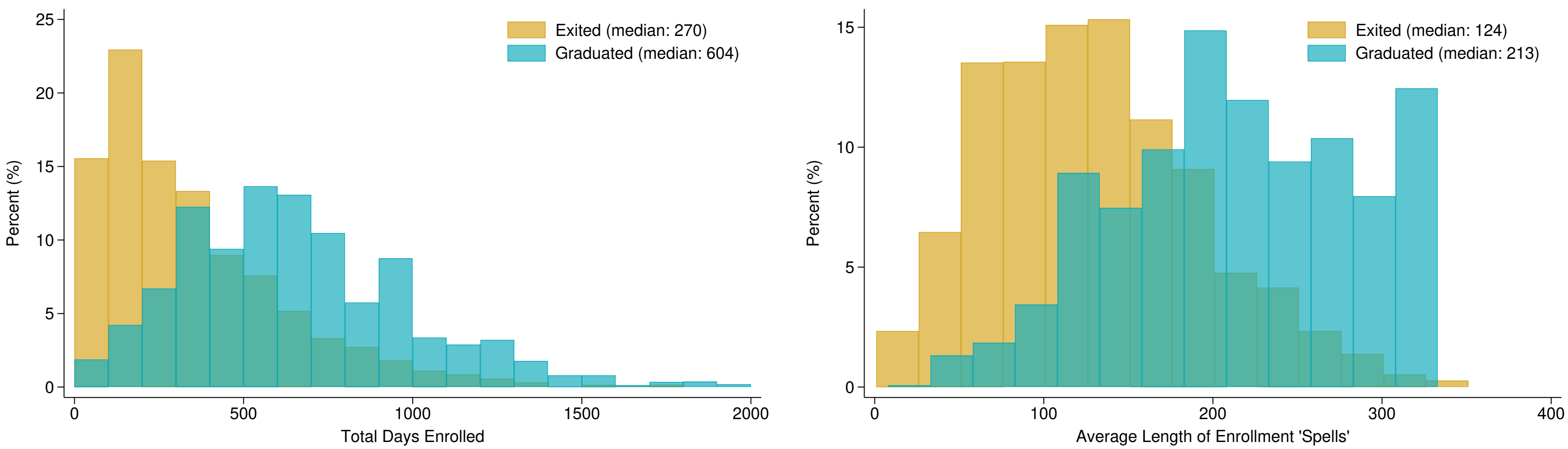


(b) Differences in Exit Rates



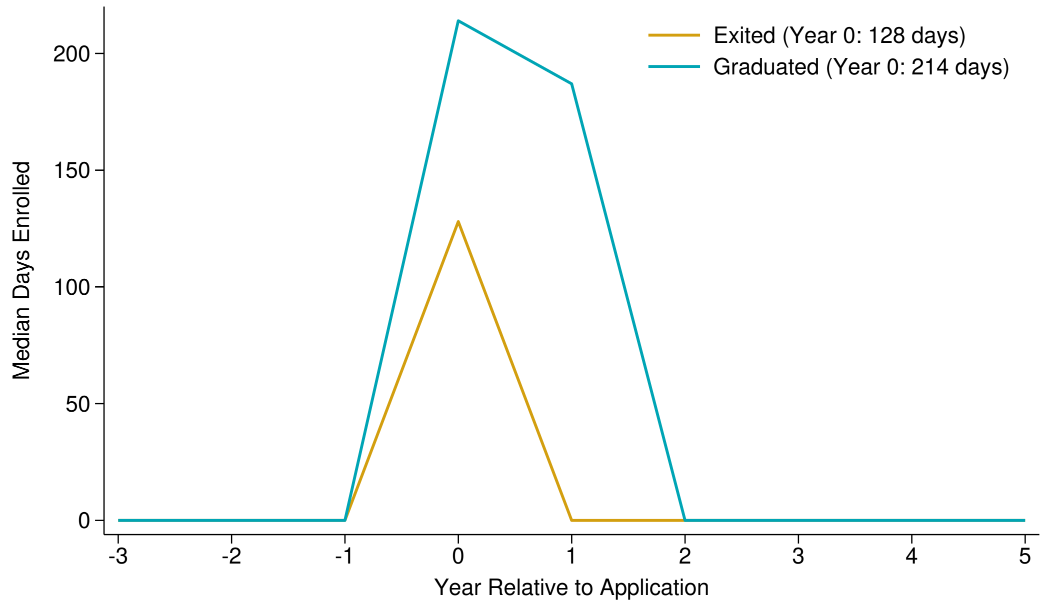
Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services and supplementary TEC records about entry and exit from TEC. From the main full analysis sample, we identify a "treated" group of enrollees (exiters) with $\geq$ 1 filing during their enrollment (gold) as well as a comparison group (turquoise) of enrollees (exiters) who (1) did not have a filing during their enrollment, (2) had at least six months of enrolled days total, and (3) were chosen through propensity score matching re: the treated group. The key event date is the date of the first filing during enrollment, and the horizontal axis in both panels displays the week relative to the event date. For the comparison group, the event date is a randomly chosen date within a student's enrollment period. The key outcome for both groups is whether their final exit from TEC occurs in the given relative week. Panel (a) reports raw trends in exits relative to filing/event dates, while Panel (b) reports coefficients from a simple difference-in-differences that uses relative week -1 as the reference period. In Panel (b), vertical bars represent 95 percent confidence intervals. All pre-periods feature mechanically zero exit rates in this exercise. See Section A.6 for more detail.

Figure A-3: Distribution of Enrollment Lengths for Exiters and Graduates



Notes: Data come from TEC application records, and are restricted to students who appear in the main analysis sample, i.e., all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017, and for whom we are able to generate an inverse propensity weight (see text for details). The left figure plots the distribution of total days enrolled, and the right figure plots the distribution of an average "enrollment spell" length across "exited" (enrolled but did not graduate) and "graduated" groups. A student can have multiple enrollment spells over the course of their total enrollment at TEC (e.g., because of leaving for a summer break).

Figure A-4: Median Days Enrolled by Year Relative to Application, Exiters and Graduates



Notes: Data come from TEC application records, and are restricted to students who appear in the main analysis sample, i.e., all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017, and for whom we are able to generate an inverse propensity weight (see text for details). The figure plots the median number of days enrolled in a given year relative to application for both exiters (those who enrolled but did not graduate) and graduates. For instance, in Year 0 we see that the median graduate was enrolled at TEC for 214 days, while in Year 0 the median exiter was enrolled for 128 days.

Table A-3: Differences in unknown information across applicants to the Excel Center

	Unweighted				
	Did not enroll	Exited	Graduated	Differences	
				(2) − (1)	(3) − (1)
	(1)	(2)	(3)	(4)	(5)
All observed students (N = 15,853)					
Missing Male/Female Flag	0.13	0.16	0.14	0.03	0.02
Missing White/Non-white Flag	0.14	0.17	0.16	0.03	0.02

Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is divided into three groups: TEC applicants (column (1)), TEC enrollees that did not graduate (column (2)), and TEC graduates (column (3)). The first three columns report mean values for the variables noted in the row titles. Columns (4) and (5) report differences in means, and as applicable significance levels from a test of this difference. Significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and *** – but in this case none of the test statistics for these differences exceeded any of these thresholds.

Table A-4: Effect of Enrollment and Graduation from the Excel Center on Case Filings, by Exit Reason

	Full Sample (1)	Work Conflict (2)	Disinterest (3)	Interpersonal Reasons (4)	No Show (5)
Enrolled X Year 0	-0.008	-0.032***	-0.001	-0.001	-0.023**
	(0.006)	(0.012)	(0.018)	(0.007)	(0.010)
Enrolled X Years 1-5	0.002	-0.014*	-0.011	0.011**	-0.003
	(0.004)	(0.008)	(0.013)	(0.005)	(0.006)
Graduated X Year 0	-0.063***	-0.041***	-0.070***	-0.071***	-0.049***
	(0.011)	(0.015)	(0.020)	(0.011)	(0.014)
Graduated X Years 1-5	-0.046***	-0.032***	-0.034**	-0.055***	-0.043***
	(0.009)	(0.011)	(0.015)	(0.009)	(0.011)
Relative Year FE	X	X	X	X	X
Person FE	X	X	X	X	X
Calendar Year FE	X	X	X	X	X
Age Bin X Relative Year FE	X	X	X	X	X
Comp. Mean-Year 0	0.129	0.128	0.127	0.130	0.128
Comp. Mean-Years 1-5	0.102	0.101	0.100	0.103	0.102
R^2	0.271	0.269	0.276	0.270	0.271
Observations	122,958	71,991	68,481	98,235	78,732
Individuals	13,662	7,999	7,609	10,915	8,748
$\Pr(\hat{\beta}\_Enroll^{Pre1-2} = 0)$	0.932	0.953	0.796	0.723	0.994
$\Pr(\hat{\beta}\_Grad^{Pre1-2} = 0)$	0.548	0.770	0.580	0.561	0.894

Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The "Full Sample" includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). Subsequent columns report subsections of this full sample, which keep the same "applicant" and "graduate" groups, but restrict the "exiter" group (those who enrolled but did not graduate) to those who have left for a particular reason, noted in the column titles. Time is measured in years relative to application date, and the data are a balanced panel from year -3 to year 5. The outcome is whether an individual had any case filing in a given relative year. All specifications are re-weighted using inverse propensity score weights that are generated for each particular subgroup. Otherwise the specifications for each column in this table are the same as for column (5) in Table 3. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***. Comparison means are calculated in the noted years for the "applicant" group within the paricular subgroup. See text for details about pre-trend tests conducted and the associated p-values reported.

Table A-5: Effect of Enrollment and Graduation from The Excel Center on Case Filings, by Weight Type

	Original vars, LASSO (default) (1)	Orig. vars, logit (2)	Orig. vars, 2x selection (3)	Expanded vars, LASSO (4)	Exp. vars, logit (5)	Exp. vars, 2x selection (6)	Revised vars, LASSO (7)	Rev. vars, logit (8)	Rev. vars, 2x selection (9)
Enrolled X Year 0	-0.008	-0.009	-0.009	-0.007	-0.010	-0.009	-0.008	-0.012*	-0.012*
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Enrolled X Years 1-5	0.002	0.003	0.003	0.003	0.003	0.003	0.002	0.002	0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Graduated X Year 0	-0.063***	-0.050***	-0.046***	-0.065***	-0.058***	-0.053***	-0.065***	-0.059***	-0.055***
	(0.011)	(0.009)	(0.009)	(0.011)	(0.010)	(0.009)	(0.011)	(0.010)	(0.010)
Graduated X Years 1-5	-0.046***	-0.031***	-0.028***	-0.049***	-0.039***	-0.036***	-0.048***	-0.040***	-0.036***
	(0.009)	(0.006)	(0.006)	(0.009)	(0.007)	(0.007)	(0.009)	(0.008)	(0.007)
Relative Year FE	X	X	X	X	X	X	X	X	X
Person FE	X	X	X	X	X	X	X	X	X
Calendar Year FE	X	X	X	X	X	X	X	X	X
Age Bin X Relative Year FE	X	X	X	X	X	X	X	X	X
Comp. Mean-Year 0	0.129	0.129	0.129	0.129	0.129	0.129	0.129	0.132	0.132
Comp. Mean-Years 1-5	0.102	0.101	0.101	0.102	0.101	0.101	0.102	0.101	0.101
R^2	0.271	0.272	0.272	0.271	0.272	0.272	0.271	0.273	0.273
Observations	122,958	118,053	118,053	122,787	117,873	117,891	122,787	114,147	114,147
Individuals	13,662	13,117	13,117	13,643	13,097	13,099	13,643	12,683	12,683
$\Pr(\hat{\beta}\_Enroll^{Pre1-2} = 0)$	0.932	0.801	0.994	0.868	0.906	0.918	0.916	0.938	0.982
$\Pr(\hat{\beta}\_Grad^{Pre1-2} = 0)$	0.548	0.705	0.138	0.416	0.499	0.124	0.583	0.597	0.183

Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017. Each group is re-weighted using inverse propensity weights that vary by column. "LASSO" refers to weights created by running a logit model with LASSO once. "Logit" refers to weights created by running a logit model without imposing LASSO selection. "2x selection" refers to (1) running a logit model with lasso twice (once with "enrolled" or "graduated" as the outcome of interest, and once with an indicator for having a filing in the post period as the outcome of interest), and then (2) using only variables selected by both iterations in a plain logit model (without LASSO) to create the final weights. The "original" list of variables includes indicators for age ventiles, indicators for sex and race groups, and lagged indicators for filings, felony filings, and misdemeanor filings in each of the three pre-period application years. The "expanded" list includes all of the variables from the original list, plus variables about a student's Census tract at the time of application, and "risk score" variables (see text, as well as Appendix Section A.4). The "revised" list removes lagged indicators about filings from the "expanded" list. Time is measured in years relative to application date, and the data are a balanced panel from year -3 to year 5. The outcome is whether an individual had any case filing in a given year. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***. Comparison means are calculated in the noted years for the "applicant" group within the full sample. See text for details about pre-trend tests conducted and the associated p-values reported.

Table A-6: Effect of Enrollment and Graduation from the Excel Center on Filings, by Disposition Type

	Enrolled Year 0 (1)	Enrolled Years 1-5 (2)	Graduated Year 0 (3)	Graduated Years 1-5 (4)	Comp. Mean Year 0	Comp. Mean Years 1-5	R^2	Obs.	Indvdls.	$\Pr(\hat{\beta}\_Enroll_{Pre1-2} = 0)$	$\Pr(\hat{\beta}\_Grad_{Pre1-2} = 0)$
Any Filing	-0.008	0.002	-0.063***	-0.046***	0.129	0.102	0.271	122958	13662	0.932	0.548
	(0.006)	(0.004)	(0.011)	(0.009)							
Any Guilty Filing	-0.007	0.002	-0.031***	-0.034***	0.080	0.068	0.257	120846	13662	0.731	0.586
	(0.005)	(0.003)	(0.009)	(0.007)							
Plea: Agreement	-0.005	-0.003	-0.022***	-0.023***	0.049	0.049	0.223	122958	13662	0.969	0.908
	(0.004)	(0.003)	(0.007)	(0.006)							
Finding: Guilty	-0.003	0.002	0.002	0.002	0.015	0.005	0.171	122958	13662	0.922	0.477
	(0.003)	(0.002)	(0.004)	(0.003)							
Plea: Guilty	0.002	0.002	-0.010**	-0.016***	0.014	0.014	0.187	122958	13662	0.692	0.245
	(0.002)	(0.002)	(0.005)	(0.004)							
Admission	0.000	0.001	-0.006**	-0.004**	0.004	0.002	0.123	122958	13662	0.414	0.319
	(0.001)	(0.001)	(0.002)	(0.002)							
Plea Glty: Lesser	-0.001	0.000	0.000	-0.001	0.002	0.002	0.115	122958	13662	0.427	0.435
	(0.001)	(0.000)	(0.001)	(0.000)							
Any Not-Glty. Flng.	-0.002	0.001	-0.027***	-0.018***	0.039	0.029	0.187	120846	13662	0.222	0.661
	(0.004)	(0.002)	(0.006)	(0.005)							
Dismissed	-0.004	0.000	-0.042***	-0.040***	0.066	0.056	0.227	122958	13662	0.443	0.790
	(0.005)	(0.003)	(0.009)	(0.008)							
Dism. w/ Prejudice.	0.000	0.000	-0.001	-0.001	0.001	0.001	0.168	122958	13662	0.691	0.545
	(0.001)	(0.000)	(0.001)	(0.001)							
Dism. w/o Prjdc.	-0.001	0.000	-0.002	-0.002*	0.003	0.002	0.134	122958	13662	0.574	0.151
	(0.001)	(0.001)	(0.001)	(0.001)							
Finding: Not Glty.	0.000	0.000	-0.000	-0.000	0.001	0.001	0.113	122958	13662	0.196	0.594
	(0.001)	(0.000)	(0.001)	(0.001)							
Vacated	-0.001*	0.000	0.000	-0.000*	0.001	0.000	0.112	122958	13662	0.607	0.607
	(0.000)	(0.000)	(0.000)	(0.000)							

Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). Time is measured in years relative to application date, and the data are a balanced panel from year -3 to year 5. The outcomes vary by row, each denoting whether an individual had a particular type of case filing in a given year. A "guilty filing" occurs when an individual's case filing has at least one charge tagged with a "guilty" disposition, and vice versa for "not guilty" classifications. Rows (3) through (7) report results specifically for filings with the top five guilty dispositions (noted in the row title), while rows (9) through (13) report results specifically for filings with the top five not-guilty dispositions. All specifications are re-weighted using inverse propensity score weights that are the same as for column (5) in Table 3. See text for details. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***. Comparison means are calculated in the noted years for the "applicant" group within the full sample. See text for details about pre-trend tests conducted and the associated p-values reported

Table A-7: Effect of Enrollment and Graduation from the Excel Center on Filings, by Charge Category

	Year 0					Years 1–5				
	Comparison	Enrolled		Graduated		Comparison	Enrolled		Graduated	
	Mean	Coef.	SE	Coef.	SE	Mean	Coef.	SE	Coef.	SE
Outcomes:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Any filing	0.1293	-0.0079	(0.0063)	-0.0635***	(0.0110)	0.1025	0.0019	(0.0042)	-0.0470***	(0.0088)
Minor traffic offense	0.0427	0.0027	(0.0041)	-0.0291***	(0.0072)	0.0381	-0.0004	(0.0027)	-0.0242***	(0.0057)
Larceny, unknown value	0.0224	-0.0035	(0.0029)	-0.0103***	(0.0031)	0.0131	0.0001	(0.0017)	-0.0026	(0.0024)
Simple assault	0.0167	-0.0007	(0.0026)	-0.0110**	(0.0049)	0.0165	-0.0004	(0.0017)	-0.0126***	(0.0044)
Obstruction - law enforcement	0.0144	-0.0030	(0.0023)	-0.0097**	(0.0041)	0.0124	-0.0003	(0.0015)	-0.0092***	(0.0034)
Other drug offense or paraphernalia	0.0127	-0.0025	(0.0021)	-0.0093**	(0.0037)	0.0128	0.0003	(0.0014)	-0.0106***	(0.0035)
Possession or use, marijuana	0.0121	-0.0012	(0.0022)	-0.0167***	(0.0053)	0.0127	-0.0012	(0.0015)	-0.0123**	(0.0049)
Drunkenness, vagrancy, or disorderly conduct	0.0108	-0.0009	(0.0020)	-0.0049*	(0.0029)	0.0074	0.0007	(0.0012)	-0.0041*	(0.0022)
Possession or use, drug unspecified	0.0089	-0.0003	(0.0018)	-0.0026	(0.0018)	0.0083	0.0012	(0.0011)	-0.0026**	(0.0012)
Trespassing	0.0072	-0.0001	(0.0017)	-0.0035**	(0.0017)	0.0047	-0.0001	(0.0010)	-0.0008	(0.0012)
Driving under the influence	0.0070	-0.0031*	(0.0016)	0.0038	(0.0033)	0.0072	-0.0006	(0.0011)	-0.0023	(0.0022)
Other (aggregated)	0.0065	0.0001	(0.0017)	-0.0042***	(0.0015)	0.0060	-0.0003	(0.0011)	0.0001	(0.0014)
Burglary	0.0065	-0.0021	(0.0014)	-0.0015	(0.0016)	0.0035	0.0008	(0.0008)	-0.0010	(0.0013)
Weapon offense	0.0057	-0.0027*	(0.0014)	-0.0001	(0.0035)	0.0051	-0.0009	(0.0008)	-0.0033*	(0.0020)
Aggravated assault	0.0055	-0.0022*	(0.0013)	-0.0035***	(0.0013)	0.0033	0.0001	(0.0007)	-0.0024**	(0.0011)
Forgery or fraud	0.0052	0.0000	(0.0015)	-0.0010	(0.0031)	0.0037	0.0004	(0.0009)	-0.0004	(0.0014)
Blackmail, extortion, or intimidation	0.0047	-0.0000	(0.0013)	-0.0021*	(0.0012)	0.0034	0.0011	(0.0007)	-0.0003	(0.0012)
Armed robbery	0.0044	-0.0032***	(0.0011)	-0.0005	(0.0006)	0.0014	-0.0003	(0.0005)	-0.0004	(0.0005)
Contempt of court	0.0040	-0.0022**	(0.0011)	0.0009	(0.0015)	0.0032	0.0005	(0.0007)	-0.0017*	(0.0009)
Grand larceny, theft over 500	0.0038	-0.0016	(0.0011)	-0.0031**	(0.0014)	0.0034	-0.0007	(0.0006)	-0.0037***	(0.0012)
Destruction of property	0.0036	0.0007	(0.0013)	-0.0033*	(0.0018)	0.0043	-0.0003	(0.0009)	-0.0026*	(0.0015)
Liquor law violations	0.0033	0.0002	(0.0014)	-0.0024	(0.0020)	0.0003	0.0006	(0.0010)	-0.0005	(0.0018)
Possession or use, cocaine or crack	0.0033	-0.0013	(0.0010)	-0.0003	(0.0007)	0.0022	0.0007	(0.0007)	0.0007	(0.0009)
Kidnapping	0.0032	-0.0003	(0.0011)	-0.0036	(0.0023)	0.0023	0.0002	(0.0006)	-0.0032	(0.0023)
Driving while intoxicated	0.0030	-0.0021**	(0.0010)	0.0018	(0.0019)	0.0024	-0.0009	(0.0006)	-0.0004	(0.0010)
Violent offenses, other	0.0028	-0.0004	(0.0011)	-0.0016*	(0.0009)	0.0014	0.0011*	(0.0006)	-0.0002	(0.0009)
Possession of amphetamines	0.0026	0.0014	(0.0011)	-0.0018	(0.0015)	0.0066	-0.0011	(0.0008)	-0.0026***	(0.0010)
Hit and run driving, property damage	0.0026	-0.0001	(0.0010)	0.0000	(0.0016)	0.0019	0.0004	(0.0005)	-0.0002	(0.0008)
Auto theft	0.0019	0.0001	(0.0009)	0.0005	(0.0014)	0.0019	-0.0006	(0.0005)	-0.0001	(0.0005)
Distribution, drug unspecified	0.0019	-0.0007	(0.0009)	0.0003	(0.0007)	0.0013	-0.0002	(0.0006)	0.0007	(0.0006)
Stolen property, receiving	0.0019	0.0024**	(0.0011)	-0.0004	(0.0013)	0.0000	0.0022***	(0.0008)	0.0013	(0.0010)
Invasion of privacy	0.0018	-0.0014*	(0.0007)	0.0005	(0.0014)	0.0006	0.0000	(0.0004)	-0.0006	(0.0009)
Escape from custody	0.0015	-0.0002	(0.0007)	-0.0005	(0.0008)	0.0014	0.0002	(0.0005)	0.0000	(0.0011)
Assaulting public officer	0.0014	-0.0000	(0.0007)	-0.0007	(0.0012)	0.0015	-0.0003	(0.0004)	-0.0007	(0.0006)
Distribution, marijuana	0.0013	0.0001	(0.0008)	-0.0015	(0.0019)	0.0015	-0.0005	(0.0005)	-0.0011	(0.0016)
Public order offenses, other	0.0012	-0.0007	(0.0006)	-0.0001	(0.0004)	0.0003	0.0001	(0.0003)	0.0007	(0.0006)
Driving under influence, drugs	0.0012	-0.0011*	(0.0006)	-0.0001	(0.0008)	0.0012	-0.0010**	(0.0005)	0.0000	(0.0007)
Habitual offender	0.0010	-0.0006	(0.0004)	0.0000	(0.0002)	0.0002	0.0002	(0.0002)	-0.0001	(0.0001)
Distribution, cocaine or crack	0.0010	-0.0006	(0.0006)	-0.0001	(0.0004)	0.0006	-0.0000	(0.0003)	0.0003	(0.0004)

Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). Time is measured in years relative to application date, and the data are a balanced panel from year -3 to year 5. The outcomes vary by row, each denoting whether an individual had a filing with at least one charge of that particular type in a given year. Each row is a different crime category as delineated by the CJARS-created Uniform Crime Classification Standard (UCCS). All specifications are re-weighted using inverse propensity score weights that are the same as for column (5) in Table 3. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***. Comparison means are calculated in the noted years for the "applicant" group within the full sample. See text for details about pre-trend tests conducted and the associated p-values reported.

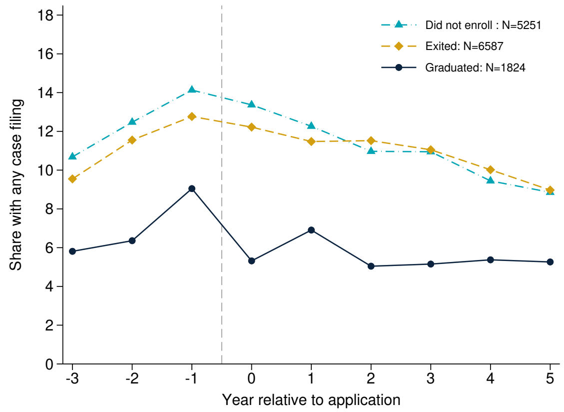
Table A-8: Effect of Enrollment and Graduation from the Excel Center on Crime Costs

	Fiscal costs: legal system (1)	Social costs, lower bound (2)	Social costs, upper bound (3)
Enrolled X Year 0	-470.159***	-5809.304**	-63411.097***
	(163.439)	(2423.849)	(19145.735)
Enrolled X Years 1-5	-58.986	-970.229	-4890.986
	(64.671)	(955.590)	(7440.888)
Graduated X Year 0	-76.941**	96.155	-5666.082
	(37.250)	(322.403)	(4226.677)
Graduated X Years 1-5	-148.602***	-1001.255*	-14083.428***
	(41.925)	(525.128)	(4578.105)
Relative Year FE	X	X	X
Person FE	X	X	X
Calendar Year FE	X	X	X
Age Bin X Relative Year FE	X	X	X
Comp. Mean-Year 0	614.157	5548.732	76133.067
Comp. Mean-Years 1-5	294.264	2066.240	28761.838
R^2	0.120	0.115	0.120
Observations	120,258	120,258	120,258
Individuals	13,662	13,662	13,662
$\Pr(\hat{\beta}\_Enroll^{Pre1-2} = 0)$	0.403	0.076	0.078
$\Pr(\hat{\beta}\_Grad^{Pre1-2} = 0)$	0.396	0.457	0.333

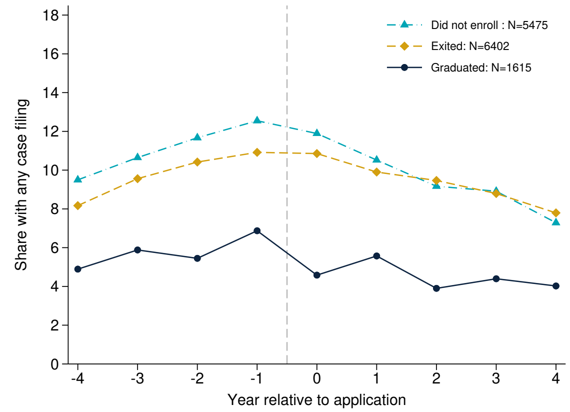
Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. The sample includes all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 and is restricted to those who have enough information to generate inverse propensity weights (see text for details). Time is measured in years relative to application date, and the data are a balanced panel from year -3 to year 5. Each column represents the change in the total cost of crime implied by the charges within case filing records, using Bhatt et al. (2024) as a reference for the lower and upper bounds of costs. "Fiscal costs" refer to costs incurred by the legal system associated with a crime, while "social costs" incorporate victim costs (and, at the upper bound, estimates of willingness to pay to avoid a crime). Cost totals only use charges that end in "guilty" dispositions. All specifications are re-weighted using inverse propensity score weights that are the same as for column (5) in Table 3. See text for details. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***. Comparison means are calculated in the noted years for the "applicant" group within the full sample. See text for details about pre-trend tests conducted and the associated p-values reported.

Figure A-5: Trends in Case Filings, Varying Event Windows

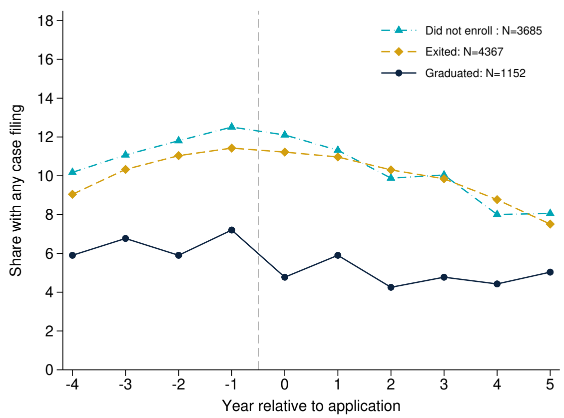
(a) $-3 \leq t \leq 5$ (Original)



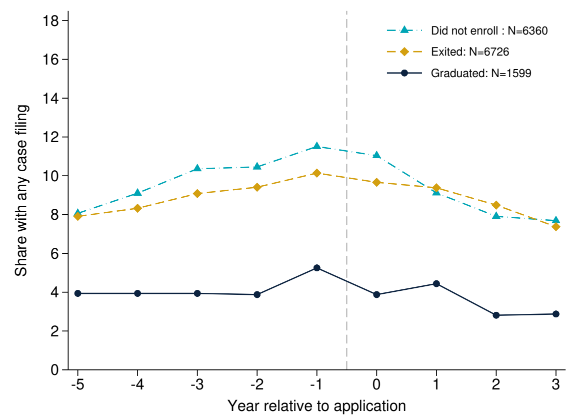
(b) $-4 \leq t \leq 4$



(c) $-4 \leq t \leq 5$



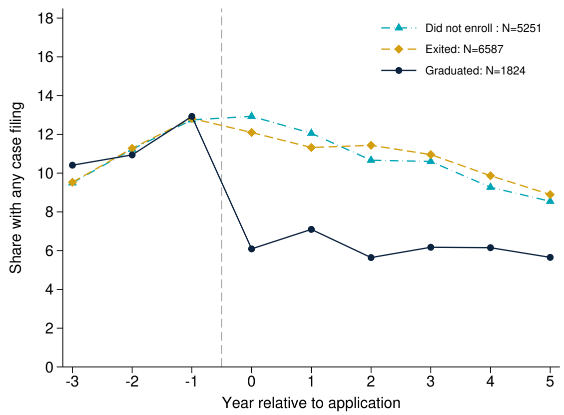
(d) $-5 \leq t \leq 3$



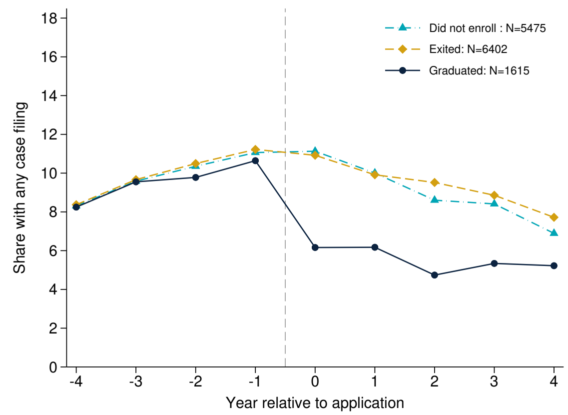
Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. In each panel, the sample includes all TEC applicants aged 16 or older in the earliest time period of the specified event window. For panel (a), this is all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 (the main analysis sample). For panel (b), this is all TEC applicants aged 20 or older at application from January 2014 to December 2018. For panel (c), this is all TEC applicants aged 20 or older at application from January 2014 to December 2017. For panel (d), this is all TEC applicants aged 21 or older at application from January 2015 to December 2019. For all samples, they are further restricted to individuals for whom there is enough information to generate inverse propensity weights (see text for details). These weights are reconstructed for each sample. In each panel the sample is divided into three groups: TEC graduates (navy circles), TEC students who did not graduate (gold diamonds), and TEC applicants who did not enroll (teal triangles). The horizontal axis indicates year relative to application date. The graphs plot raw, unweighted trends in any case filing for the labeled subgroups.

Figure A-6: Weighted Trends in Case Filings, Varying Event Windows

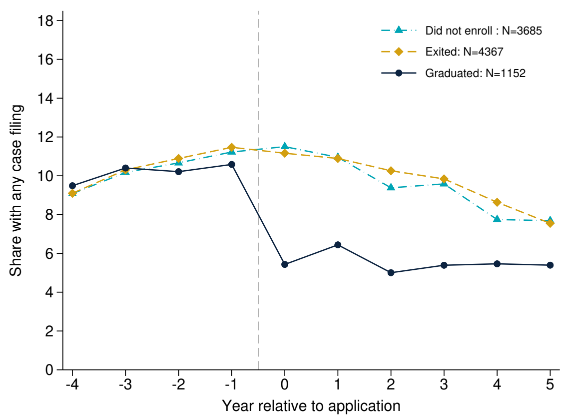
(a) $-3 \leq t \leq 5$ (Original)



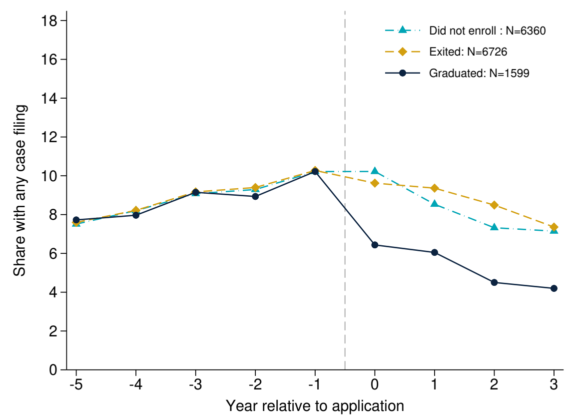
(b) $-4 \leq t \leq 4$



(c) $-4 \leq t \leq 5$



(d) $-5 \leq t \leq 3$



Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. In each panel, the sample includes all TEC applicants aged 16 or older in the earliest time period of the specified event window. For panel (a), this is all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 (the main analysis sample). For panel (b), this is all TEC applicants aged 20 or older at application from January 2014 to December 2018. For panel (c), this is all TEC applicants aged 20 or older at application from January 2014 to December 2017. For panel (d), this is all TEC applicants aged 21 or older at application from January 2015 to December 2019. For all samples, they are further restricted to individuals for whom there is enough information to generate inverse propensity weights (see text for details). These weights are reconstructed for each sample. In each panel the sample is divided into three groups: TEC graduates (navy circles), TEC students who did not graduate (gold diamonds), and TEC applicants who did not enroll (teal triangles). The horizontal axis indicates year relative to application date. The graphs plot weighted trends in any case filing for the labeled subgroups.

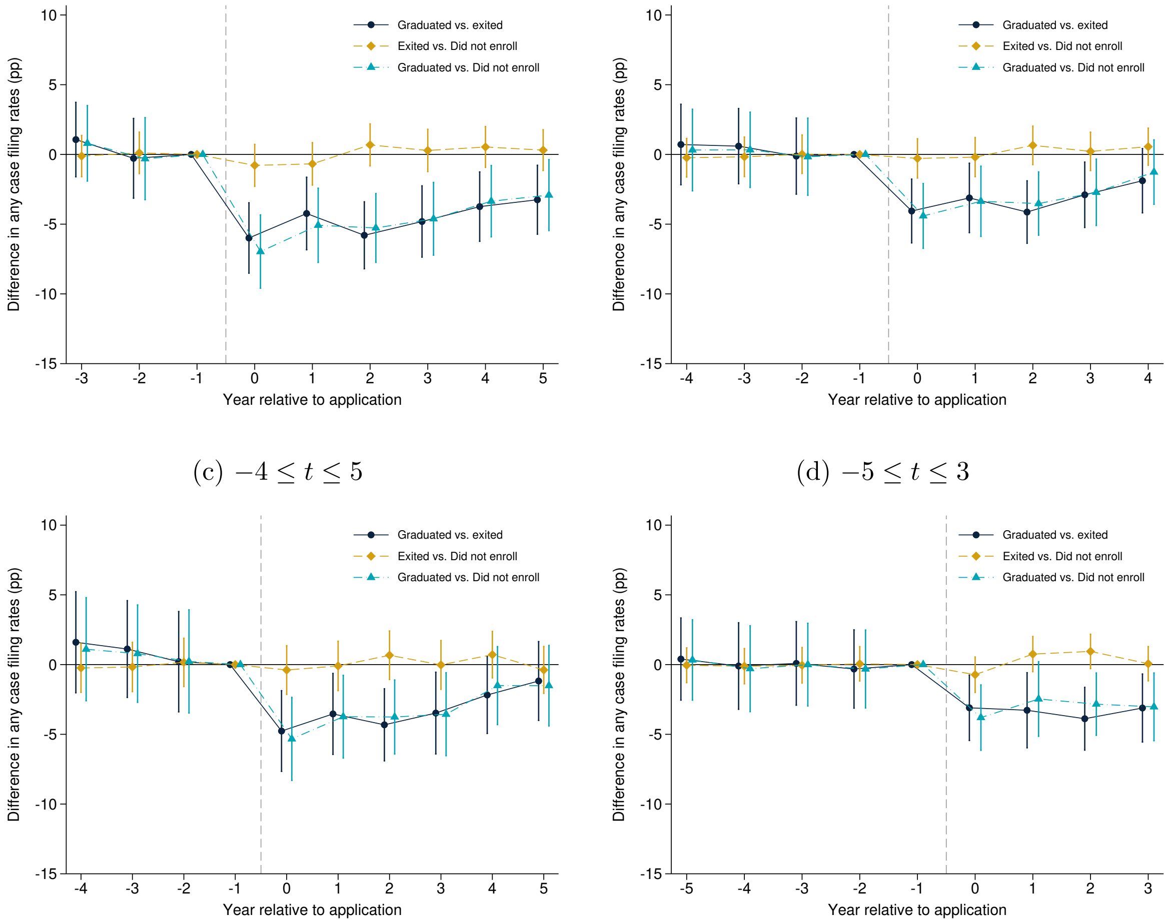
Figure A-7: Weighted Event Study of Case Filings, Varying Event Windows

(a) $-3 \leq t \leq 5$ (Original)

(b) $-4 \leq t \leq 4$

(c) $-4 \leq t \leq 5$

(d) $-5 \leq t \leq 3$



Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. In each panel, the sample includes all TEC applicants aged 16 or older in the earliest time period. For panel (a), this is all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 (the main analysis sample). For panel (b), this is all TEC applicants aged 20 or older at application from January 2014 to December 2018. For panel (c), this is all TEC applicants aged 20 or older at application from January 2014 to December 2017. For panel (d), this is all TEC applicants aged 21 or older at application from January 2015 to December 2019. For all samples, they are further restricted to individuals for whom there is enough information to generate inverse propensity weights (see text for details). These weights are reconstructed for each sample. In each panel the sample is divided into three groups: TEC graduates (navy circles), TEC students who did not graduate (gold diamonds), and TEC applicants who did not enroll (teal triangles). The horizontal axis indicates year relative to application date. The graphs plot coefficients from a weighted event study regression that compares case rates between two groups controlling for age-bin-specific relative time fixed effects, individual fixed effects, and calendar year fixed effects. Year -1 is the reference year. Vertical bars represent 95 percent confidence intervals.

Table A-9: Effect of Enrollment and Graduation from The Excel Center, Varying Event Windows

	-3 $\leq t \leq$ 5 (Original)	-4 $\leq t \leq$ 4	-4 $\leq t \leq$ 5	-5 $\leq t \leq$ 3
	(1)	(2)	(3)	(4)
Enrolled X Year 0	-0.008	-0.002	-0.003	-0.008
	(0.006)	(0.006)	(0.007)	(0.006)
Enrolled X Years $\geq$ 1	0.002	0.004	0.002	0.002
	(0.004)	(0.004)	(0.005)	(0.004)
Graduated X Year 0	-0.063***	-0.044***	-0.055***	-0.063***
	(0.011)	(0.010)	(0.013)	(0.011)
Graduated X Years $\geq$ 1	-0.046***	-0.033***	-0.037***	-0.046***
	(0.009)	(0.008)	(0.011)	(0.009)
Relative Year FE	X	X	X	X
Person FE	X	X	X	X
Calendar Year FE	X	X	X	X
Age Bin X Relative Year FE	X	X	X	X
Comp. Mean-Year 0	0.129	0.111	0.115	0.129
Comp. Mean-Years $\geq$ 1	0.102	0.085	0.091	0.102
R^2	0.271	0.274	0.259	0.271
Observations	122,958	121,428	92,040	122,958
Individuals	13,662	13,492	9,204	13,662
Pr($\hat{\beta}$_$Enroll^{Pre1-2}$ = 0)	0.932	0.957	0.935	0.932
Pr($\hat{\beta}$_$Grad^{Pre1-2}$ = 0)	0.548	0.908	0.766	0.548

Notes: Data come from TEC application records linked to administrative court records provided by the Indiana Office of Court Services. In each panel, the sample includes all TEC applicants aged 16 or older in the earliest time period of that panel. For panel (a), this is all TEC applicants aged 19 or older at the time of application from January 2013 through December 2017 (the main analysis sample). For panel (b), this is all TEC applicants aged 20 or older at application from January 2014 to December 2018. For panel (c), this is all TEC applicants aged 20 or older at application from January 2014 to December 2017. For panel (d), this is all TEC applicants aged 21 or older at application from January 2015 to December 2019. For all samples, they are further restricted to individuals for whom there is enough information to generate inverse propensity weights (see text for details). These weights are reconstructed for each sample. Time is measured in years relative to application date, and the data are balanced panels. The various event windows are denoted by the column header. All specifications are re-weighted using inverse propensity score weights that are generated for each particular subgroup. Otherwise the specifications for each column in this table are the same as for column (5) in Table 3. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted respectively by *, **, and ***.