# ESSAYS ON THE ECONOMICS OF HEALTH AND EDUCATION

by

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# DISSERTATION ABSTRACT

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I present empirical research considering the response of health and educational outcomes to alcohol consumption, drug abuse, and collegiate athletics. Chapter II considers the effect of legal access to alcohol on student achievement. The empirical approach identifies the effect through changes in students' performance after gaining legal access to alcohol, controlling flexibly for the expected evolution of grades as students make progress towards their degrees. The estimates indicate that students' grades fall below their expected levels upon being able to drink legally but by less than previously documented.

Chapter III considers the relationship between collegiate-football success and non-athlete student performance. The findings indicate that the team's success significantly reduces male grades relative to female grades and only in fall quarters, which coincides with the football season. Survey data suggest that males are more likely than females to increase alcohol consumption, decrease studying, and increase partying in response to the success of the team. Yet, females also report that their behavior is affected by athletic success, suggesting that their performance is likely impaired but that this effect is masked by the practice of grade curving.

Finally, chapter IV considers the effect of substance-abuse treatment on drug-overdose deaths. Though the provision of substance-abuse treatment may be an effective way to reduce drug abuse, whether it has a causal effect on drug-related mortality has not been documented. I analyze the effect of substance-abuse treatment on mortality by exploiting county-level variation in treatment facilities driven by facility openings and closings. The estimates indicate that a 10-percent increase in facilities lowers a county's drug-induced mortality rate by 2 percent. The results also suggest that spillovers of treatment reduce other causes of death related to drug abuse.

As a whole, this body of research offers insight into the economic impact of behaviors involving drinking and other substance use.

This dissertation includes previously published co-authored material.

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# CHAPTER I

# INTRODUCTION

In the subsequent chapters, I present empirical research considering questions related to the economics of health and education. While each chapter addresses a distinct research question, together the findings contribute to a broad discussion on the economic impact of behaviors involving drinking and other substance use. Chapters II and III represent co-authored work and also appear as publications in peer-reviewed academic journals. Below I summarize each chapter and discuss the primary findings.

In Chapter II, we consider the effect of legal access to alcohol on student achievement. Our preferred approach identifies the effect through changes in one's performance after gaining legal access to alcohol, controlling flexibly for the expected evolution of grades as one makes progress towards their degree. We also report RD-based estimates but argue that an RD design is not well suited to the research question in our setting. We find that students' grades fall below their expected levels upon being able to drink legally, but by less than previously documented. We also show that there are effects on women and that the effects are persistent. Using the 1997 National Longitudinal Survey of Youth, we show that students drink more often after legal access but do not consume more drinks on days on which they drink.

Chapter III considers the relationship between collegiate-football success and non-athlete student performance. We find that the team's success significantly reduces male grades relative to female grades, and only in fall quarters, which coincides with the football season. Using survey data, we find that males are more likely than females to increase alcohol consumption, decrease studying, and increase partying in response to the success of the team. Yet, females also report that their behavior is affected by athletic success, suggesting that their performance is likely impaired but that this effect is masked by the practice of grade curving.

Finally, In Chapter IV I examine the effect of substance-abuse treatment on drug-related mortality. Drug-overdose deaths, which have more than doubled over the past decade, represent a growing public-health concern. Though the provision of substance-abuse treatment may be an effective way to reduce drug abuse, whether it has a causal effect on drug-related mortality has not been documented. Given the stigma associated with treatment, low completion rates, high risk of relapse, and that many patients are referrals from the criminal justice system, the effect of treatment is not obvious a priori. In this chapter, I analyze the effect of substance-abuse treatment on mortality by exploiting county-level variation in treatment facilities driven by facility openings and closings. The estimates indicate that a 10-percent increase in facilities lowers a county's drug-induced mortality rate by 2 percent. Moreover, the estimates suggest significant benefits of treatment facilities across a range of individual and county characteristics, though the effect is especially large among racial minorities, in urban counties, and in counties with low per-capita incomes. The results also suggest that spillovers of treatment reduce other causes of death related to drug abuse.

## CHAPTER II

## ALCOHOL AND STUDENT PERFORMANCE: ESTIMATING THE EFFECT OF LEGAL ACCESS

This work was published in Volume 32, Issue 1 of the *Journal of Health Economics* in January 2013. Jason Lindo, Isaac Swensen, and Glen Waddell were the principle investigators for this work.

### Introduction

A large literature links alcohol consumption to adverse health and social outcomes.[1] Given long-standing and persistent efforts to restrict access to alcohol, it is no surprise that this topic has received considerable attention from researchers. However, relatively little is known about the effect of legal access to alcohol on the academic performance of students in college, where binge drinking is often cited as a serious and growing problem (DeSimone 2007). That alcohol is associated with acute outcomes such as crime, mortality, and sexual activity gives cause for concern that the effect on student performance may be quite large.

In this paper, we assess the effect of legal access to alcohol on academic performance using two identification strategies. The first has been used to address this research question in a different setting and the second has not, but both approaches exploit the exogenous change in legality induced by the federally mandated minimum

[1] In particular, quasi-experimental methods have been used to consider effects on mortality (Dee 1999; Carpenter 2004; Carpenter and Dobkin 2009), crime (Markowitz and Grossman 1998; Carpenter 2005a; Carpenter 2007; Carpenter and Dobkin 2010), sexual activity (Chesson, Harrison, and Kassler 2000; Rees and Argys 2001; Sen 2002; Rashad 2004; Carpenter 2005b; Waddell 2012), employment (Mullahy and Sindelar 1996; Terza 2002; Dave and Kaestner 2002; MacDonald 2004, Renna 2008), and teenagers' educational outcomes (Cook and Moore 1993; Dee and Evans 2003; Chatterji and DeSimone 2006), among others.

legal drinking age (MLDA). Our first identification strategy follows Carrell, Hoekstra, and West (2011) who exploit the sharp change in legality that occurs at age 21 in a regression discontinuity (RD) framework to estimate the effect of legal access on student performance. While it is relatively straightforward to use an RD design to estimate effects of turning 21 on crime or traffic accidents, as in Carpenter and Dobkin's works, it is less straightforward as an approach to estimating effects on academic outcomes since they are not measured frequently. For this reason, the RD approach uses age from 21 *at the end of the academic term* as the running variable. As such, the estimates capture the effect of legal access to alcohol for students who obtain access near the end of the academic term. In the limit, the thought experiment compares the performance of students who turn 21 the day before their final exam to the performance of students who turn 21 on the day of their final exam. The resulting estimates can therefore be characterized as measuring a local average treatment effect, which may have limited external validity.

Our second and preferred identification strategy overcomes this limitation by making use of the longitudinal nature of the data. In particular, we identify the effect of legal access to alcohol by comparing a student's post-21 academic performance to his own pre-21 academic performance with individual-fixed effects models—implicitly arguing that the best counterfactual for a student's post-21 performance is his own performance prior to turning 21. In addition, our regressions include fixed effects for the number of accumulated credits to account for the possibility that students may systematically improve, "slack off," or experience classes of different difficulty as they progress towards degree completion. Although it is not typical for researchers to be able to separately identify effects of experience (or accumulated credits in our case) and the effects of age (or an age-dependent treatment in our case), we can do so in our context by leveraging the variation in college starting ages.[2] As in the first approach, we use a student's course performance relative to their classmates' as our outcome variable, which will also serve to control for selection into courses.[3]

The data and institutional setting that we consider—transcript-level data from undergraduates at the University of Oregon—allow us to make several additional contributions to the literature. One of the unique features of Carrell, Hoekstra, and West's (2011) study using data from the U.S. Air Force Academy is that the prohibition on underage drinking is taken extremely seriously there—much more so than in most other institutional settings, where enforcement is more lax and punishment less severe.[4] As such, assuming Air Force Academy students are representative of the general student population, their RD estimates tell us about the local average treatment effect of prohibition in environments where enforcement and penalties are unusually strict. In contrast, our results are more likely to speak to the effect of minimum drinking age laws *as they are conventionally enforced* and, in turn, the effect of the changes in drinking behavior that are typically associated

[2]As an alternative to our preferred models that control for accumulated-credit fixed effects, we have also estimated models that instead control for the age profile of student grades. Whereas the former relies on the assumption that the effects of progress are similar for those with and those without legal access, this alternative approach relies on the assumption that the age profile of grades is well captured by the chosen parametric specification. The estimates based on both approaches are statistically significant. The estimates based on the accumulated-credit fixed effects models, however, are more conservative.

[3]In related studies, Williams, Powell, Wechsler (2003) and Powell, Williams, and Wechsler (2004) consider the effect of alcohol consumption on college GPAs using data from the Harvard School of Public Health's College Alcohol Study. These studies involve cross-institution comparisons of student GPAs, with measures of alcohol costs serving as instruments for drinking intensity among those who drink. Kremer and Levy (2008) consider a different-but-related question, exploiting the random assignment of roommates at a large state university in order to identify the effect of having a roommate who drinks. Finally, Lindo, Swensen, and Waddell (forthcoming) consider how non-athlete academic performance varies with collegiate football success and provide survey evidence of increased alcohol consumption and partying associated with football success.

[4]Carrell, Hoekstra, and West (2011) highlight this feature, pointing to the fact that two incidents of underage drinking at the Air Force Academy resulted in expulsion and that some related incidents (e.g., driving under the influence) have also resulted in expulsion.

with legal access to alcohol. As we describe in the next section, the University of Oregon is also more representative of U.S. institutions, which leaves us more confident that estimates based on these data will have greater external validity. Further, our data include over four times the number of observations used in this earlier research, and approximately ten times the number of females which allows a more-precise consideration of heterogeneity across gender.

The results from our preferred approach indicate that students' grades fall below their expected levels by approximately 0.03 standard deviations upon being able to drink legally, a modest amount compared to the 0.06 to 0.13 standard-deviation effect estimated in earlier research. The effect is statistically significant, manifests in the term a student turns 21, and persists into later academic terms. In addition, we find that the effects on academic performance are especially large for females, low-ability males, and males who are most likely from financially disadvantaged backgrounds.

In order to shed light on the mechanisms underlying these effects, we also analyze the effects of legal alcohol access on various drinking-related behaviors among students enrolled in four-year colleges using data from the 1997 National Longitudinal Survey of Youth (NLSY97). This analysis reveals that college students drink more often upon gaining legal access but do not consume more drinks on days in which they drink.[5]

[5]Though the NLSY97 also includes information on GPAs in college, these data have several shortcomings that preclude an analysis similar to that which we perform on our transcript data. Most crucially, the NLSY97 does not offer transcript data on academic performance but instead has retrospective self-reports of GPA by term, leaving much opportunity for measurement error. In particular, in each survey, the students are asked about their GPAs in all academic terms since they were last surveyed. This time period most frequently spans one year but sometimes spans several. Possibly as a result of this survey design, there are also many cases in which it appears as if the performance in a given term may have been reported more than once (in different survey years). Moreover, several features of these data make it difficult to accurately assign whether the student had legal access to alcohol in a given term. Specifically, the month and year in which the term began can be recorded but information on term length is more often missing than not. In our best

The rest of this paper is organized as follows. In Section 3.2 we discuss the data used in our analysis as well as the representative nature of the University of Oregon campus. In Section 2.3 we present an RD strategy, following existing literature, and our preferred longitudinal approach to identifying the influence of legal access to alcohol on academic performance. In Section 2.4 we present and discuss the empirical results, including our analysis of how college students' drinking-related behaviors change upon gaining legal access to alcohol based on the NLSY97. We conclude and discuss the implications of our results in Section 4.8.

## Data

Our primary data are administrative student-course level data from the University of Oregon, spanning fall 1999 to winter 2007, for students entering at 18 or 19 years old. We focus on performance during the fall, winter, and spring terms.[6] Because our identification strategies use variation provided by the federal MLDA law, we also restrict the sample to students observed at least one term in which they are at least 21 years old. The resulting sample consists of 479,342 observations over 13,102 students.

As one contribution of this paper is to provide insight into the effects of MLDA laws in a "typical-college setting," Table 1 (see appendix A for figures and tables) compares characteristics of students at the University of Oregon to those at other U.S. public four-year institutions. While Column 1 provides summary statistics based on our sample, Column 2 considers a more comprehensive set of characteristics based on

attempts to impute the data where necessary, NLSY97 data yield a point estimate that is negative (suggesting a negative effect of legal access on GPAs) but not statistically significant.

[6] One reason for excluding summer terms is that summer enrollment could itself be considered an outcome variable. In addition, summer terms tend to be fundamentally different from other terms in class size, course offerings, student attendance, teacher and student attributes, and term structure.

data from the Integrated Postsecondary Education Data System (IPEDS). Similarly, Column 3 shows statistics on other public four-year institutions, also using data from IPEDS.[7]

Table 1 largely supports that the University of Oregon provides a representative-college setting. While it is twice the size and has higher admission rates than the average public four-year institution, it is similar in terms of enrollment rates and in the ability of enrolled students as measured by SAT scores. It is also similar to the average college in terms of costs and financial aid. Like most other institutions, the University of Oregon is over half female and predominately white, although at seventy-five percent it has a larger share of white students than average.[8]

In contrast, the U.S. Air Force Academy, the only other institution where this research question has been addressed, offers a relatively unique setting. In addition to being highly selective, it is very different from most schools in terms of its students' objectives. In particular, all students at the Air Force Academy are given full scholarships but are expected to serve a five-year commitment as a commissioned officer in the U.S. Air Force following graduation. Moreover, females comprise only eighteen percent of its student body, which stands in stark contrast to the nation-wide average of fifty-five percent. As mentioned in the introduction, it is also important to note that the Air Force Academy is an outlier in strongly enforcing the MLDA law. That students at the Air Force Academy are such a select group from the distribution of all students, in both ability and preferences, and that they are in an

[7] In comparing across institutions we have used variables that provide a snapshot of school admissions and graduation rates, general academic standards, undergraduate student demographics, and student financial costs and aid. The statistics reported in columns 2 and 3 are based primarily on the 2003-2004 academic year, which is close to the median year for our data.

[8] Also like most other large public institutions, a majority freshman at the University of Oregon live on campus (87 percent) while sophomores, juniors, and seniors do not often live on campus (7, 3, and 2 percent, respectively).

environment that is unusually strict with respect to underage drinking, gives cause for concern about the external validity of earlier estimates and highlights the importance of considering the research question in different contexts.

Our secondary data source is a subsample of respondents from the 1997 National Longitudinal Survey of Youth (NLSY97), which allows us to speak to the sensitivity of alcohol consumption to legality among college goers. To the extent possible, our sample restrictions on these data reflect those above. In particular, we restrict the NLSY97 sample to individuals who report being currently enrolled in a four-year college, who were first observed in college at age 18 or 19, and also observed in college after turning 21.[9] We also balance the sample by removing observations when any of the outcome variables we consider (described below) are missing. Ultimately, the sample spans 1998 to 2009, includes 2,298 person observations and 9,023 person-year observations. Summary statistics for the NLSY97 data are provided in the last column of Table 1. Like the University of Oregon, the NLSY97 offers a sample that is similar in ability to the broader group of students attending four-year public institutions and its gender composition, but differs in some important ways in its racial composition. In particular, like the University of Oregon, it provides a sample that is disproportionately white; however, whereas the University of Oregon has very relatively few black students, the NLSY97 has relatively few Asian students.

[9] An individual's age at the time of the interview is based on the interview date along with the individual's month and year of birth. We impute the fifteenth as the day of birth. Though this will lead some person-year observations to be misclassified in terms of legal access, few observations are on the margin for this to make a difference. 3,790 observations are classified as having legal access when the fifteenth of the month is imputed as the day of birth; this count becomes 3,827 and 3,719, respectively, if the first or twenty-eighth of the month are used instead.

## Empirical Strategy

### *RD-Based Approach*

Following the prior literature, we begin by estimating the effect of having had one's 21st birthday before the end of the academic term on academic performance using the following regression equation:

$$G_{ijt} = \alpha_0 + \alpha_1 1\{\text{AGE}_{it} \geq 0\} + f(\text{AGE}_{it}) + \epsilon_{ijt}, \tag{2.1}$$

where $G_{ijt}$ is the normalized grade for student $i$ in class $j$ in term $t$. Normalized grades are calculated as the deviation in a student's grade from the class mean divided by the class standard deviation. $\text{AGE}_{it}$ is the student's age at the end of the term in days, centered on 21 years. For example, in the comparison of means as estimates approach the treatment threshold from each side, a bandwidth of 90 days would put weight on all students who had their 21st birthday in the range 90 days prior to the end of the term (i.e., AGE $= 90$) through 90 days after the end of the term (i.e., AGE $= -90$). Last, $f(\text{AGE}_{it})$ controls for a student's age at the end of the term in a flexible manner. In practice, we estimate a variety of models, including those that do not control for age at the end of the term, those that control for age at the end of the term with a linear specification flexible on each side of the cutoff, and those that control for age at the end of the term with a quadratic specification flexible on each side of the cutoff. Further, we consider a variety of bandwidths between 20 days and 240 days.

It is important to note that this identification strategy departs from the usual RD exercise. Typically, we observe—or know as a result of institutional detail—the extent to which the treatment of interest jumps on the "treatment side" of the threshold. For example, in DiNardo and Lee's (2004) unionization study, all elections with union support greater than fifty percent lead to unionization while elections with less support do not. Similarly, in Angrist and Lavy's (1999) class-size study, we observe class-size reductions above multiples of forty enrolled students. Our example is similar insofar as all students on the "treatment side" of the threshold have had the opportunity to drink alcohol legally prior to the conclusion of the academic term. However, because the underlying effects on drinking behavior is unobserved, the magnitude of any estimated effect will be somewhat difficult to interpret. Even though drinking tends to increase when a student turns 21, as we show in Section 2.4, we do not know to what extent this holds true for students who turn 21 near the end of an academic term, which this identification strategy pre-supposes. As such, the comparison involved with this RD approach is informative about the effect of drinking on college performance but its "local" nature (close to 21 *and* close to the end of the term) introduces additional interpretive challenges.[10]

In the absence of estimated effects on drinking behavior for such a sample, the results are appropriately characterized as intent-to-treat effects, measuring the reduced-form effect of the minimum-drinking-age law. While this is certainly of interest in itself, the RD design only provides an estimate of a very local intent-to-treat effect, corresponding to students gaining legal access to alcohol at the end of the academic term. This will remain a disadvantage of the RD approach in this setting, something that we improve on with the identification strategy presented in the next section where we exploit the longitudinal nature of the data.

[10]We note that all RD-based studies that consider the effect of being able to drink legally are local in the first (close to 21) sense but that the second sense is specific to this application, driven by the fact that outcomes are not measured daily.

*Individual Fixed Effects Approach*

Our preferred approach to estimating the effects of legal access to alcohol focuses on within-student variation over time. Although we first present estimates from more-parsimonious models, we ultimately arrive at estimates derived from the following regression:

$$G_{ijt} = \theta \text{AGE21}_{it} + \beta X_{ijt} + \alpha_i + u_{ijt}, \tag{2.2}$$

where $G_{ijt}$ is the normalized grade for student $i$ in class $j$ in term $t$, $\text{AGE21}_{it}$ is an indicator variable that takes a value of one if the student could drink legally at any time during term $t$ and zero otherwise, $X_{ijt}$ can include term- or class-varying individual characteristics, $\alpha_i$ are a set of individual fixed effects, and $u_{ijt}$ is a random error term. In our analysis of transcript data from the University of Oregon, we always include "experience controls" in $X_{ijt}$, i.e., fixed effects for the number of accumulated credits (in four-credit intervals) to control for grade changes that are expected as a student progresses toward degree completion.[11] For example, these variables are intended to control for phenomena such as "senioritis." As such, the estimation strategy compares a student's grades after turning 21 to what would be expected based on his average prior performance and accumulated experience.[12] This approach relies on the assumption that accumulated experience has a similar effect on grades before and after a student turns 21. As an alternative to fixed effects for accumulated credits, one might anticipate that we would model the age profile

[11]While it would be attractive to also include fixed effects for the number of terms a student has been at the university, doing so is likely to introduce problems of multicollinearity in conjunction with the individual fixed effects and accumulated-credits fixed effects since there is little variation in credits attempted each term. For example, such a model would be impossible to estimate if all students earned 12 credits each term. We have explored models that include fixed effects for the number of terms a student has been at the university *instead* of the accumulated-credits fixed effects and the results are quite similar.

[12]We also estimate models that control for course characteristics.

of academic performance, and consider deviations from the predicted grade upon turning 21 as a reflection of the causal effect of legal access to alcohol. Each of these two approaches offers conceptual advantages and both support our conclusions. We limit the presentation and discussion of the results to the former approach, which yields the more-conservative estimates.

Our approach to estimating the effect of legal alcohol access on drinking behavior in the NLSY97 is quite similar with the exception of the controls we include in the model. In particular, since the data are at the individual-by-survey-year level (asking about drinking behavior "in the past 30 days") as opposed to the term-level, we estimate the effects using the model that controls for age at the time of the interview with a quadratic as opposed to the number of credits that the student has acquired in college.[13]

## Results

In this section, we first present RD-based estimates of the effects of legal access on academic grades using Oregon transcript data. We then we report the results from our preferred approach—fixed-effect based estimates—of the same. Finally, we report on the NLSY97 analyses, where we estimate the response of alcohol consumption and drinking behavior to legalization.

[13]One could conceivably control for accumulated credits using the NLSY97, but the the number of credits a student has taken in each term is often missing. Moreover, lacking the ability to track transfers from school to school and to determine how many classes have been passed or failed, we would not expect such a measure to reflect true progress towards degree completion. Thus, we rely on the assumption that the age profile of drinking behavior is well captured by the chosen parametric specification.

*RD-Based Estimates using Transcript Data*

Panel A of Table 2 presents RD-based estimates of the effect of legal access to alcohol at the end of a term on academic performance. Across the fourteen columns, the table shows estimates based on a wide range of bandwidths and functional-form choices. While the upper portion of this panel reports unadjusted estimates, the lower portion reports estimates that controls for course-by-quarter-by-year fixed effects, birth-year fixed effects, accumulated-credits fixed effects, gender, math and verbal SAT scores, high-school GPA, and indicator variables for university athletes, private high school attendance, race, and ethnicity.[14]

Overall, the set of results in Panel A of Table 2 suggests that turning 21 before the end of a term has a negative impact on a student's grades. While the point estimates vary somewhat from specification to specification and are sensitive to control variables, they are routinely negative and suggest that students who turn 21 prior to the end of the quarter score roughly 0.03 to 0.05 standard deviations lower than those who turn 21 after the quarter ends. However, the sensitivity of RD estimates to the inclusion of controls—primarily the inclusion of individual characteristics and accumulated credits—casts doubt on the validity of this strategy in our setting.

As a further robustness check, panels B and C of Table 2 report the results from a similar exercise that instead considers the effect of turning 20 and 22, respectively, before a quarter ends. In particular, these results test for a more-general "birthday effect" which would raise the concern that the estimates in Panel A might reflect a "21st birthday effect" that cannot be separated from the effect of gaining legal access

[14]Race and ethnicity controls consists of a set of indicator variables for being black, Hispanic, or Asian.

to alcohol. Although the estimates are usually not significant, the fact that 51 of the 56 point estimates are negative casts further doubt on the validity of this strategy in our setting. If we believe that the estimated "20th birthday effect" or "22nd birthday effect" provides a good estimate for the "21st birthday effect independent of legal access," then the difference between Panel A and either Panel B or Panel C can be interpreted as the effect of legal access at the end of the term. Given the marginal significance of most of the estimates in Panel A and the consistently negative estimates in panels B and C, this approach would not produce convincing evidence that legal access at the end of the academic term significantly reduces grades.[15]

*Fixed-Effects Estimates using Transcript Data*

In this section, we present our main results followed by a consideration of the possible dynamic response to being able to drink legally (Section 4.2.2), and the heterogeneity of the established results by gender, ability, and financial need (Section 4.2.3).

Main Results

In Table 3 we present our main results, making use of the longitudinal nature of the data. In Column 1, we show the estimated effect based on a regression of a student's normalized grade on an indicator for whether a student could drink legally at any time during the term. Because we anticipate that relatively low ability students

[15] Carrell, Hoekstra, and West (2011) conduct a similar analysis using alternative age cutoffs and find no evidence of 20th or 22nd birthday effects at the U.S. Air Force Academy. In an alternative attempt to separate the birthday effect from that of a potentially-persistent effect of legal access to alcohol we have also explored the use of a donut-RD approach (Carpenter and Dobkin, 2009; Barreca, Guldi, Lindo, and Waddell, 2011; Barreca, Lindo and Waddell, 2011). In particular, we have conducted a similar analysis after dropping observations 1, 2, 3, 10, and 15 days to either side of the cutoff. This analysis continued to show similar estimates when considering the effect of turning 20 and 21.

will be observed more often at older ages (as they take longer to complete their degrees), we anticipate that this approach will overstate the negative effect of legal access to alcohol. After we control for ability and other unobservable characteristics with the inclusion of individual fixed effects, the estimate is indeed much smaller (falling from -0.146 to -0.097 from Column 1 to Column 2). However, estimates in Column 2 may still suffer from bias due to the potential for grades to fall as students progress towards their degrees while they become increasingly likely to be 21 years old. As anticipated, the magnitude of the estimate is even smaller when we remove this source of bias by controlling for a student's accumulated credits with fixed effects. That said, the point estimate (shown in Column 3) remains statistically significant at the one-percent level, indicating that a student's course-normalized grades fall by 0.033 standard deviations after they gain legal access to alcohol relative to what we would expect based on their prior performance and accumulated experience. The estimated effect is identical when we add controls for subject-by-level fixed effects and term fixed effects in Column 4, which is not surprising since our outcome variable is normalized at the class level.[16]

In order to better understand our main results in Table 3, in Table 4 we explore the effects of legal access on additional academic outcomes. To begin, we consider the distributional effects of legal alcohol access on grades. In particular, we use linear probability models (with the same controls used in the richest specification of Table 3) to separately estimate the effect of of legal alcohol access on the probability that a student earns an A grade, a B grade, a C grade, and a D or F grade, respectively. These estimates suggest that the negative effect of legal alcohol access on grades

[16]For these fixed effects, subjects correspond to economics, english, and mathematics. Levels correspond to either 100-, 200-, 300-, or 400-level classes. As summer terms are not considered as part of our analysis, terms are fall, winter, and spring.

overall is driven by its negative effect on the probability that a student earns an A grade and its positive effect on the probability that a student earns a C grade. The estimated effects on B grades and failing grades are negligible.[17]

Although the estimates in Table 3 address omitted variable bias that might be induced by effects on course-taking behavior by normalizing students' grades relative to their classmates and by controlling for course characteristics, any effect on course selection is of interest itself. We explore this issue columns 5 and 6 of Table 4, considering the effect of legal alcohol access on course difficulty and course loads. This analysis is identical to the preceding analysis, except that it is conducted at the student-by-quarter level rather than the student-by-quarter-by-course level and, as such, omits course-level controls.

As our measure of course difficulty, we focus on a student's unconditional "expected" GPA. This is calculated based on the average grades in the most-recent offerings of the courses a student has enrolled in for their current quarter. The estimated effect on this outcome is 0.009, which suggests that students take slightly easier classes upon turning 21—a small and statistically significant effect on course-taking behavior. However, as we show in the final column of Table 4, there is no evidence that students take more or fewer credits upon gaining legal access to alcohol.

[17]We have also estimated the effect on the probability that a student earns a quarterly GPA weakly above 2.0 (i.e., the GPA that would imply probation) and the probability that a student earns a quarterly GPA above 3.75 (i.e., the GPA that would imply Dean's List membership). These results indicate that legal access does not affect the probability that a student earns a 3.75 but increases the probability that a student is placed on academic probation, with point estimates (and standard errors) of -0.003 (0.002) and 0.019 (0.002), respectively. In addition, we have separately estimated the effects on grades in classes taken within a student's major and outside of a student's major. These results reveal larger effects for courses taken outside a student's major, with point estimates (and standard errors) of -0.058 (0.007) and -0.023 (0.009), respectively. Finally, we do not find any evidence that legal access affects the probability that students repeat classes.

Treatment-Effect Dynamics

In order to consider the dynamic effect of being able to drink legally, we return to our preferred outcome variable, students' normalized grades, and our preferred specification but replace the post-21 indicator variable with a set of indicator variables corresponding to the number of terms following the term in which a student gains legal access to alcohol. In particular, we include separate indicator variables for the term in which the individual turns 21, one term after a student turns 21, ..., five terms after a student turns 21, and six-or-more terms after a student turns 21. The omitted category, essential for identifying individual fixed effects and trends, is being in a term prior to turning 21.[18]

Although it is possible to include indicator variables for terms prior to turning 21 to verify that grades do not fall below their expected levels in anticipation of gaining legal access—which we do in a series of falsification tests—our preferred estimates of the dynamic effects do not take this approach. We make this choice out of consideration for the general tradeoff involved with including pre-treatment indicator variables when using panel data approaches to estimation. Specifically, as one includes more indicator variables for pre-treatment periods, the counterfactual for the post-treatment periods becomes worse and worse as fewer observations contribute to the estimate of the individual fixed effects. For example, if we were to include indicators for one, two, three, and four terms prior to turning 21, our model would be projecting a student's future performance using observations from when he was under the age of 20. As such, our estimates of interest corresponding to post-21 terms

[18]Note that although summer terms do not contribute to out analysis, such terms are considered in defining the term-based proximity to the term in which a student turns 21. As such, when the "turned 21 four terms ago" indicator variable is equal to one we are considering an individual in the term he turns 22.

would be noisier and less reliable than estimates that do not include these indicator variables and instead use all pre-21 terms to form counterfactuals.

Our preferred estimates of the treatment effect dynamics, shown in Column 1 of Table 5, indicate that grades fall significantly below their expected levels—by 0.036 standard deviations—in the term a student turns 21. This suggests an immediate negative effect of legal access to alcohol on academic performance. Further, the estimated coefficients corresponding to subsequent terms are usually significant and of similar magnitude, which indicates that the effect persists. We do note, however, that the coefficient on having turned 21 four terms ago (-0.055) is somewhat higher than the rest, which may reflect a 22nd-birthday effect.[19]

In Figure 1, we present a graphical analogue to this analysis. In particular, we plot average adjusted normalized GPAs by students' ages in quarters. The normalized GPAs have been adjusted by taking the residuals from a regression on individual fixed effects, accumulated-credits fixed effects, and the course-specific fixed effects described above. Like the estimates in Column 1 of Table 5, this figure shows clearly that student GPAs fall below their expected levels when students turn 21 and, further, they stay below their below their expected levels for several subsequent quarters.

In columns 2 through 5 of Table 5, we subject our estimation strategy to a series of specification tests. In particular, we add to our model indicator variables for terms preceding the term in which a student turns 21. Simply put, it would be a threat to the validity of the research design if similar effect evident in terms before a student turns 21. In order to maximize power, we take an incremental approach to adding

[19]We have also examined whether the timing of a student's 21st birthday during the quarter is related to its impact on grades. These results, discussed in detail in Lindo, Swensen, and Waddell (2011), demonstrate that there are effects of gaining legal access to alcohol at any time during a given quarter. Further, we cannot reject that the effect is the same for students gaining legal access to alcohol at different times during the quarter.

indicator variables for terms preceding an individual's 21st birthday. In Column 2 we add an indicator for being one term prior to turning 21, in Column 3 we add an indicator for being two terms prior to turning 21, in Column 4 we add an indicator for being three terms prior to turning 21, and in Column 5 we add an indicator for being four terms prior to 21. Ultimately, we have ten "placebo tests" across these four columns where we do not anticipate any effects. Of these ten estimates, none are significant, which provides support for our preferred identification strategy. We also note that the estimates shown in Column 5 are what one would get if they were estimating the effect of turning 20 on student performance. Unlike the RD-approach above, where a 20th birthday effect is evident, we find no evidence that performance declines when a student turns 20 using our preferred approach.

Treatment-Effect Heterogeneity

In Table 6 we explore the extent to which there are heterogeneous effects of legal alcohol access on student achievement. Motivated by prior research documenting gender differences in educational performance and in tendencies to engage in risky behaviors, these tables report separate estimates for males and females. We also consider heterogeneity by ability and financial-aid eligibility to determine whether our main results are driven by individuals more likely to struggle with coursework or those from particular economic backgrounds.

In Panel A we stratify the sample by student gender and ability, with "high ability" students defined as those with SAT scores above the sample median of 1120 and "low ability" students defined as those at or below the sample median. The results in columns 1 and 2 suggest that the effect of being able to drink legally is larger for females on average than it is for males. The point estimates remain small, however, with legal access reducing female grades by 0.045 and male grades by 0.024 standard deviations. In columns 3 and 4, point estimates also suggest that the effect on low-ability students may be slightly greater than the effect on high-ability students.

In columns 5 through 8, we separately consider the effects for low-ability males, high-ability males, low-ability females, and high-ability females. These estimates reveal substantial heterogeneity among males. Although there is a significant effect on low-ability males whose grades fall 0.047 standard deviations below their expected level after they gain legal access to alcohol, there appears to be no effect on high-ability males. On the other hand, our point estimates suggest that there are negative effects for both high- and low-ability females, although the estimated effects are greatest for low-ability females.

In Panel B we stratify the estimates by financial-aid eligibility and gender for the seventy percent of students who submitted a Free Application for Federal Student Aid (FAFSA). In so doing, we define a student as "high-eligibility" if Pell Grant eligibility is above the sample median and "low-eligibility" if Pell Grant eligibility is below the sample median. In Column 1, we show that the estimated effect for this sample of students (-0.042) is somewhat larger than the estimated effect based on the full sample (-0.033). However, the set of estimates suggests that, among males, the effect is concentrated among those who are likely to be from disadvantaged backgrounds. In contrast, the estimated effect is similar across differing levels of financial-aid eligibility among females.

*Fixed-Effects Estimates using the NLSY97*

The statistics shown in Section 3.2 support the notion that the University of Oregon provides a fairly representative college setting. However, we lack data on drinking behavior at the University of Oregon. Thus, in this section we use data from the National Longitudinal Survey of Youth (NLSY97) to analyze how college students' drinking-related behaviors change upon gaining legal access to alcohol. While several prior studies have analyzed the effects of legal alcohol access on drinking-related behaviors (see Carpenter and Dobkin 2011 for a review), to our knowledge we are the first to focus on the effects among college students.

Again utilizing the longitudinal nature of the data, we estimate the effect of legal alcohol access (being at least 21 years old at the time of the interview) on drinking-related behaviors during the past 30 days with an individual fixed effects model that controls for a quadratic in age. As such, the estimated effect is identified based on the discrete changes in behaviors that occur after an individual turns 21, adjusted for the gradual changes that are expected as individuals grow older. All regressions use NLSY97 sampling weights and cluster the standard-error estimates on the individual.

In Column 1 of Table 7 we summarize the results of our analysis for the full sample. Across the panels A through D, we report the estimated effects on the probability that an individual has drank alcohol during the past 30 days, the number of days in which an individual has drank alcohol in the past 30 days, the number of alcoholic drinks an individual has had on average when drinking during the past 30 days, and the number of days an individual has drank five or more drinks during the past 30 days.[20] These results demonstrate that legal access causes college students to drink more often, as it increases the probability that an individual reports drinking by 6.5 percentage points, the number days drinking by 1.4, and the number of days drinking five or more drinks by 0.4. However, there is no evidence that legal access causes college students to drink more intensely on occasions during which they drink.

[20]We impute 15 drinks per day for the 0.9 percent of individuals who report that they drink more than 15 drinks per drinking occasion.

This may reflect the likelihood that college students are more likely to be drinking in public after they turn 21 where social pressures (and bartenders) may limit the amount that an individual consumes.[21]

In columns 2 through 9 we consider heterogeneity across gender, ability, and their interaction. High and low ability groups are defined based on the same SAT cutoff used to define the two groups in our analysis of University of Oregon transcript data. However, because SAT scores are missing for approximately two-thirds of the sample where Armed Services Vocational Aptitude Battery (ASVAB) test scores are not, we use ASVAB scores to impute predicted SAT scores where actual SAT scores are not available.[22]

These estimates illustrate that the link between legal access and drinking behaviors is complex. In particular, even putting imprecision aside, we usually cannot say that the effects are greater for one group than another. For example, the point estimates suggest that access has a bigger effect on the probability of drinking for females but has a bigger effect on the number of days drinking for males. This mixed pattern also appears when making comparisons across gender within ability groups. Comparing the effects across ability groups, it appears as if the effect of legal access is greater for high-ability females than low-ability females. We do not, however, see the same pattern among males.

Given this mixed set of results, we are inclined to view these estimates as evidence of "proof of concept." Specifically, that we find effects of legal alcohol access on grades

[21] These results are consistent with Carpenter and Dobkin (2009) who report that legal access leads to a 21 percent increase in drinking days (versus our estimate of is 33 percent), an insignificant 20-percent increase in heavy drinking days (versus our significant estimate of 19 percent), and find little evidence of any effect on the number of drinks consumed when drinking.

[22] Specifically, predicted SAT scores are based on the conditional expectation function implied by a regression of SAT scores on a quartic in ASVAB scores for the sample where both scores are available.

among students at the University of Oregon is only compelling if legal access affects the behaviors of college students, which we demonstrate using the NLSY97. However, we are not inclined to extend our interpretation of the results to estimate the effect any particular measure of "drinking" on college performance given the variety of ways in which one can measure drinking. As we alluded to in the introduction, legal access may affect achievement through its impact on whether a student drinks, the frequency with which a student drinks, and the intensity with which a student drinks, in addition to the wide array of social activities that are associated with drinking. Moreover, there are generally not any specific groups in the NLSY97 that we can point to as being particularly responsive to legal access in their drinking activity, except perhaps high-ability females.

That said, we do find it interesting that we find no significant effect on the grades of high-ability males at the University of Oregon despite finding evidence in the NLSY97 that they drink more often with legality. This may suggest that high-ability students are particularly adept at changing their drinking-related behaviors without compromising their grades but, of course, we cannot rule out the possibility that high-ability males at the University of Oregon do not alter their drinking behaviors upon gaining legal access. That said, the results for females also support the notion that high-ability students are better able to maintain their grades when they change their drinking activity. In particular, the NLSY97 data suggest that legal access has a greater impact on the drinking behaviors of high-ability females than on low-ability females; yet transcript data from the University of Oregon suggests that legal access has a greater impact on the grades of low-ability females (though the difference is not statistically significant).

## Conclusion

As a whole, our analysis suggests that legal access to alcohol does affect the drinking behavior of college students and, in turn, affects student performance. At the University of Oregon, legal access to alcohol reduces grades by 0.03 standard deviations, or the equivalent of causing a student to perform as if his or her SAT score were 20 points lower. As such, the effect we identify is smaller than Carrell, Hoekstra, and West (2011) who find that gaining legal access at the end of the academic term reduces grades by approximately 0.10 standard deviations. Given the more conventional enforcement of MLDA at large public universities, this difference might exist because legal access has a different effect on alcohol-related behavior across the two settings. We also find substantial heterogeneity across gender and ability, in ways that diverge meaningfully from the prior research. In particular, given that the U.S. Air Force Academy is more selective and has a much larger fraction of men than the University of Oregon, it is perhaps surprising that we find no evidence of an effect among high-ability males. In addition, we identify a significant effect on the performance of females.

## CHAPTER III

## ARE BIG-TIME SPORTS A THREAT TO STUDENT ACHIEVEMENT?

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### Introduction

> "Tailgaiting rituals, painted faces, and screaming fans are part of American higher education as surely as physics labs and seminars on Milton. . . Big-time athletics is too important to be relegated entirely to the sports pages. . . At issue is whether the university entertainment enterprise is a threat to American higher education or instead is one of its reasons for success."
>
> – Charles T. Clotfelter, *Big-Time Sports in American Universities*

In the midst of record-setting revenues, escalating costs, and the launching of conference-specific television networks, collegiate sports have never been bigger. Moreover, they have grown to be quite important from a public-finance perspective. In 2010, 211 out of 218 Division I athletics departments at universities subject to open records laws received a subsidy from their student body or general fund.[1] These subsidies are substantial and rapidly growing. From 2006 to 2010, the average subsidy increased 25 percent, to nine-million dollars. Given the large amount of tax-payer and tuition dollars that are being channeled toward college sports programs, and concerns

[1]There are a total of 346 Division I schools, 128 of which are not subject to open records laws. Division I is the highest level of intercollegiate athletics. Statistics are based on the analysis of documents gathered by USA Today and Indiana University's National Sports Journalism Center. These data were available on 10 October 2011 at `www.usatoday.com/sports/college/2011-06-23-2011-athletic-department-subsidy-table_n.htm`.

that these programs might be detrimental to the academic missions of universities, the merits of this spending has been the subject of intense debate.

However, almost nothing is known about its effect on human capital acquisition.[2] The research that has been conducted on big-time college sports has focused primarily on its advertising effects, considering impacts on student applications, student enrollment, and alumni giving.[3] To our knowledge, Clotfelter (2011) is the only prior study to plausibly identify a causal effect of college sports on learning and research, which are clearly the most important objectives of post-secondary institutions. It is important to note that it is not clear *ex ante* what effect to anticipate, as some have argued that college sports are a distraction that diverts time and attention away from academic pursuits whereas others have argued that it could enhance productivity by promoting social capital. To address the question empirically, Clotfelter examines the number of JSTOR articles viewed (as a measure of work done by students and faculty) at 78 research libraries around the time of the NCAA basketball tournament. He finds that having a team in the tournament reduces the number of article views and, further, that unexpected wins have especially large effects.

In this paper, we build on this earlier work by considering how academic performance at a large public university varies with the prominence of university football on campus, as measured by the team's winning percentage in a given year. One of the advantages of our approach that focuses on student GPAs is that it provides a relatively long-run measure of student performance, whereas Clotfelter

[2]In describing the close-to-twenty years he convened the National Bureau of Economic Research working group on higher education, Clotfelter (2011) reports: "In the 30 meetings of that group that occurred over this period, scholars presented 176 papers on topics ranging from financial aid, rising costs, and preferential admissions to faculty retirement, doctoral training, and sponsored research. But only one paper during this entire period had to do with big-time college sports."

[3]See the Knight Commission on Intercollegiate Athletics for further discussion. For recent work, see Pope and Pope (2011).

may be identifying the inter-temporal substitution of study time that might not affect levels of learning. In addition, we can exploit the gender asymmetry in how closely students follow college sports in order to speak to the extent to which the effects might be driven by professor behavior rather than student behavior.

Our paper also contributes to the large literature on gender differences in higher education, where some have argued that there is a pending "boy crisis." This concern is often motivated by the fact that males have fallen further and further behind females in college attendance and completion over the past thirty years (Goldin, Katz, and Kuziemko, 2006). The 2008 American Community Survey shows that 24 to 29 year old females are 17-percent more likely to have attended college and 29-percent more likely to have completed a baccalaureate degree than similarly-aged males.

Of additional concern is the fact that males tend to be less responsive than females to educational interventions, which suggests that we may need to look beyond traditional educational policies to better understand the determinants of male performance.[4] In this area, research focusing on the ability level of peers also tends to find greater effects for females than males whereas studies that explore alcohol consumption and its associated activities find mixed results.[5] Collectively, this

[4]For example, males have been found to be less responsive, if responsive at all, to achievement awards (Angrist and Lavy, 2009; Angrist, Lang, Oreopoulos, 2009), tuition reductions (Dynarski, 2008), and offers of academic advising (Angrist, Lang, Oreopoulos, 2009). Lindo, Sanders, and Oreopoulos (2010) find that being placed on academic probation improves the grades of returning females more than returning males, but that it causes males to drop out and has no such impact on females. Babcock and Marks (2011) document that female students study more than their male counterparts.

[5]In particular, Stinebrickner and Stinebrickner (2006), Han and Li (2009), and Carrell, Hoekstra, and West (2011b) find greater peer effects among females; Foster (2006) finds larger effects for males but concludes that there is "little evidence of robust residential peer effects on undergraduate performance." Kremer and Levy (2008) find that being assigned a heavily-drinking roommate affects males more than females; Carrell, Hoekstra, and West (2011a) find that the effect of legal access to alcohol is similar for males and females; and Lindo, Swensen, and Waddell (2011) find that legal access affects females but not males.

research suggests that one would be hard-pressed to reliably identify an important factor that would have a greater influence on males' academic performance than females' academic performance. While instructor gender might appear to be a likely candidate, even the research in this area is mixed.[6] As such, it is informative to consider a prominent component of college culture that our priors suggest would exhibit a pronounced influence on the male population—the hype and interest associated with the success of the university football team.

The public university we consider, the University of Oregon, being largely representative of other four-year public institutions and having substantial variation in football success, provides an ideal setting to explore the effects of big-time college sports. Highlighting the significance of the football team, Figure 2 summarizes survey evidence on the number of football games students watched during the 2010 season. Only 10 percent of females and an even smaller share of males report watching zero games. Some 40 percent of females watched 10 or more games out of 12, while over 50 percent of males watched 10 or more games.

Our main results are based on student fixed effects models to ensure that the estimates are not driven by systematic changes in the composition of students that may be correlated with the success of the football team. This analysis reveals that GPAs vary systematically with the prominence of university football on campus, as measured by the team's winning percentage in a given year.[7] Our estimates suggest

[6] At the post-secondary level, Hoffmann and Oreopoulos (2009) find small effects overall but report that these effects are "driven more by males performing worse when assigned to a female instructor, with females performing about the same." In contrast, Carrell, Page, and West (2010) report that "professor gender has little impact on male students, [but] has a powerful effect on female students' performance in math and science classes, their likelihood of taking future math and science courses, and their likelihood of graduating with a STEM degree." See Hoffmann and Oreopoulos (2009) for an in-depth review of the larger literature that focuses on the primary and secondary levels.

[7] Other documented behaviors associated with collegiate football include increased crime (Rees and Schnepel, 2009) and heavy alcohol consumption (Neal and Fromme, 2007; Glassman, Werch,

that three fewer wins in a season would be expected to reduce the gender gap by nine percent. In order to speak to the mechanisms at work, we provide evidence that students' time use and study behaviors respond differentially by gender to the football team's performance.[8] Given that females' time use and study behaviors are also affected by the team's performance, it is likely that their performance is affected as well but this is masked by the usual practice of grade curving. We also explore heterogeneity across race and measures of socioeconomic status—we find that the effects are most severe for non-whites and those from disadvantaged backgrounds.

In order to consider an outcome that is more-clearly linked to long-run outcomes, we supplement our analysis of GPAs with an analysis of drop-out behavior. With the caveat that we cannot control for student fixed effects in this analysis, we find that the success of the football team does not significantly affect the probability that male students drop out before their next fall quarter. Given that their grades are impaired, this suggests that male drop-out behavior is not sensitive to academic performance or that any effect of academic performance on drop-out behavior is counterbalanced by other effects of the football team's success. In contrast, we find that the success of the football team decreases the probability that low-ability females drop out, which may be due to improved grades or to other effects of the team's success.

## Data Used in Main Analysis

Our primary source of data is University of Oregon student transcripts, covering all undergraduate classes administered from fall quarter of 1999 through winter

and Bian, 2007; Glassman, *et. al.*, 2010). Card and Dahl (2011) also find increases in male on female violence associated with NFL football games.

[8]See Stinebrickner and Stinebrickner (2004) for a discussion on the relationship between time use and educational outcomes.

quarter of 2007. For our main analysis, we limit the sample to fall quarters to coincide with the collegiate-football seasons. We also limit the sample to non-athlete undergraduate students as we anticipate that athletic success, if not endogeneous to athletes' academic performance, may interact differently with student-athlete grades. After making these restrictions, our main sample consists of 29,737 students, or 267,322 student-class observations across nine fall quarters.[9]

We combine these data with readily available reports of the football team's win-loss records which we use to form our term-specific measures of athletic success—the ratio of total games won to total games played.[10] Over our sample period, the winning percentage is 69.7 percent, on average, and varies from 45.5 percent to 90.9 percent.

This large public university is also representative in terms of institutional and student characteristics. While twice the size and having higher admission rates than the average public-four-year institution, it is similar in terms of enrollment rates and SAT scores of incoming students. It is also similar to the average college in costs of attendance and in financial aid opportunities. Like most other institutions, the University of Oregon is over half female and predominately white, although at 75 percent it has a larger share of white students than is typical of the universe of U.S. post-secondary institutions.[11] We report summary characteristics of our data in Table 8. Consistent with the discussion in the introduction, males have systematically lower

[9]The implied average number of classes per student is low as we do not observe all students' complete tenure at the institution. Normal patterns of attrition from the university also act to lower this ratio.

[10]For five of the nine seasons spanned by our data, the team played 11 regular season games; for the other four seasons, the team played 12 regular season games. Post-season games, which take place after the fall quarter ends, are not used to construct the winning percentage used for our analysis. Although pre- or post-season rankings could conceivably be used to proxy for the prominence of university football on campus in a similar fashion, the University of Oregon was unranked in most of the years spanned by our data.

[11]See Lindo, Swensen, Waddell (2011) for additional comparisons to other four-year public U.S. institutions.

GPAs than females. On average, they earn GPAs of 2.94 whereas the average among females is 3.12. This gap is present for first-year students and students who have been at the university for several years. In unreported analysis, we have verified that the gap cannot be explained by ability upon entry, as measured by high-school GPAs and SAT scores.

## Estimated Effects on GPAs

### *Main Results*

In Panel A of Table 9, we report estimates of the effect of athletic success on male GPAs. To begin, in Column (1) we estimate

$$G_{ijt} = \alpha + \theta WinningPercentage_t + \epsilon_{ijt}, \tag{3.1}$$

where $G_{ijt}$ is the grade of student $i$ in class $j$ in the fall term of year $t$ and $WinningPercentage_t$ is the ratio of wins to total games played in year $t$; standard errors estimates are clustered on the student. This simple model leads to an estimate of $\theta$ of -0.142.

Of course, the extent to which the university experienced grade inflation in the years spanned by the data and the football team's performance got somewhat worse, this estimate may overstate the negative impact of the football team's success. Indeed, in Column (2) where we control for a quadratic in time, the point estimate is substantially smaller (-0.064) although it remains statistically significant. This estimate is largely unchanged by the inclusion of a rich set of controls for observable student characteristics in Column (3).[12]

Our preferred estimates are identified off of within-student longitudinal variation in grades as the football team's winning percentage varies, corresponding to the regression equation

$$G_{ijt} = \alpha_i + \psi WinningPercentage_t + \beta X_{ijt} + e_{ijt}, \tag{3.2}$$

where $\alpha_i$ are student fixed effects. As this approach isolates the effect of athletic success on individual-student performance in fall classes across terms, we are implicitly assuming that the counterfactual for a student's performance in "high-win terms" is the student's own performance in "low-win terms" and vice versa. We prefer this approach because it controls flexibly for the changing composition of the student body from year to year which may not be well captured by the battery of observable characteristics available in our data (or by a smooth time trend). Column (4) shows the estimated effect based on this model, still controlling for the overall time trend in order to address grade inflation; it is smaller (-0.039) but remains statistically significant.

In Column (5), we address the fact courses offered during fall quarters may differ from year to year which could lead to a spurious relationship between the performance of the football team and student grades. We do so by controlling for subject-by-level

[12] In particular, in this column, we add controls for math and verbal SAT scores, high-school GPA, age at entry, and indicator variables for Black, Hispanic, and Asian, and for having graduated from a private high-school.

fixed effects.[13] With the addition of these controls, the estimated effect is unchanged but is more precisely estimated.

In Column (6), we take an alternative approach to controlling for time-varying factors, including fixed effects for the number of credits a student has accumulated before the quarter begins instead of the overall time trend.[14] This approach is motivated by our desire to control flexibly for the tendency for grades to increase as students make progress toward their degrees. Of course, it would be desirable to control for this tendency in addition to the long-run trend but, with student fixed effects in the model, this would lead to near-perfect multicolinearity. With this tradeoff acknowledged, we note that the estimated effect based on this model (-0.069) is larger than the estimate based on the model that instead controls for a smooth time trend.

It is important to note that the practice of grading student performance on a curve implies that the estimates in Panel A are likely to understate the true effect on male performance. For example, if the success of the football team impairs all students' performance equally, there would be no effect on any student's GPA under strict curving. In contrast, if the success of the football team has an especially large impact on the performance of a particular group of students (males), we would clearly expect to see their GPAs fall relative to others (females). Further, one would anticipate that the one group's "response" to athletic success would appear to offset the other's, consistent with the zero-sum nature of strict grading curves.

With this in mind, in Panel B we perform the same analysis for the female-student population. The estimates based on our model with student fixed effects are

[13] For example, subjects correspond to economics, english, and mathematics, while levels correspond to either 100-, 200-, 300-, or 400-level classes.

[14] The fixed effects are a series of indicator variables for credits in intervals of four.

positive though they vary in their statistical significance. However, for the reasons described above, even if the estimates were strongly significant, it would not imply that athletic success improves female performance—in Section 3.5 we present evidence suggesting that female performance is likely impaired—but instead would likely reflect that the relatively-large impact on males improves females' *relative* performance which translates into higher grades when grades are based on a curve. The fact that the magnitude of the estimated effects on females tends to be smaller than the magnitude of the estimated effect on males suggests that grade curves are not *perfectly* strict.

Additionally, it is important to note that the estimated effects on females' GPAs suggest that it is unlikely that the effects on males are driven by professor behavior. If athletic success led professors to be more generous or more harsh in assigning grades, we would anticipate observing similar effects on both male and female students.[15]

As a measure of relative performance, the gender gap in grades is not subject to the interpretative challenges discussed above. Inasmuch as grading curves are uniformly applied to male and female students, changes in the gender-gap in GPAs that are systematic with football performance are clearly indicative of changes in gender-specific performance. Pooling male and female observations and adding the interaction of winning percentage and an indicator for being male to the models described above, in Panel C we estimate the effect of athletic success on the gender gap in grades. The coefficient on the interaction of the winning percentage with the indicator for being male provides the estimated effect of athletic success on the gender gap in grades. Whereas the gender-specific estimates were somewhat sensitive

[15]Although we think it is unlikely, we do note that it is possible that the success of the football team causes professors to grade male and female students differently. That said, we have verified that our main result is also evident if we focus on large classes (with at least 50 students) where this sort of professor behavior is less likely. In fact, the estimated effect is even larger when the sample is restricted in this manner.

to the inclusion of differing control variables, the estimated impact on the gender gap is quite stable as the coefficient estimates range from -0.052 to -0.072 across the five columns.[16] Further, they are statistically significant and virtually identical in columns (4) and (5), which display our preferred estimates. To put the magnitude of the estimate (-0.061) into context, it suggests that a 25-percentage-point increase in the football team's winning percentage will increase the gender gap in GPAs (0.18) by 8.5 percent.

*Estimated Effects Using Aggregate Data*

While we prefer the the approach described above because it allows us to control for several potential confounders, the pattern we have identified is sufficiently regular that it is evident in a plot of the mean difference between male and female grades and winning percentage across years. We provide such a plot in Figure 3, for 1999 through 2007. The correlation coefficient between the difference in average grades (i.e., male minus female) and winning percentage is -0.736. Moreover, aggregating grades to the nine fall-term observations at which the variation in winning percentage exists and regressing the difference between male and female GPAs on the winning percentage yields an estimated coefficient of -0.074 and a heteroskedasticity-robust standard-error estimate of 0.027—very close to the point estimates in our preferred specification in Table 9

[16]We have also estimated models that include course-credits fixed effects, course-enrollment fixed effects (in bins of 10 from 0–50, bins of 25 from 50–100, and bins of 50 from 100–250), and fixed effects for the number of credits the a student is taking in the current quarter. The inclusion of these controls changes the estimated effect by less than 0.001. In addition, we have considered using GPAs normalized at the class level as an outcome variable. These results are also very similar, albeit somewhat larger in magnitude, to our main results.

*Estimated Effects Across Letter-Grade Assignments*

In Table 10, we explore the potential for winning percentage to influence grades non-linearly across the grade distribution. Specifically, we replace "grade point" with binary letter-grade assignments on the left-hand side. Because it offers a clearer interpretation, in this subsection and the subsections that follow, we focus on the effect of athletic success on the gender gap in GPAs. We also focus on our preferred models that control for student fixed effects and subject-by-level fixed effects. However, because there is a tradeoff involved with controlling for time versus controlling for accumulated credits, in separate panels we present estimates that take each of these two approaches.

In Table 10, we replace "grade point" with binary letter-grade assignments on the left-hand side. As such, in Column (1), the coefficient on the interaction of the indicator for being male and the winning percentage is interpreted as the difference between males and females in the impact of athletic success on the probability of receiving a grade of "A" in a given class. Across columns (1) through (4), we observe meaningful decreases in the probabilities of receiving As and Bs and increases in the probabilities of receiving Cs or lower for males in response to the success of the team. Clearly, the largest effect on the gender gap appears to occur at the lowest end of the grade distribution—in the probability of receiving a failing grade.

*This Pattern is Unique to Fall Terms*

In Table 11, we investigate whether similar effects are found in winter and spring quarters, where one would not expect the winning percentage to affect student performance. Doing so provides evidence that only in the quarter we associate with football—the fall quarter—is there movement in the gender gap in academic performance that varies systematically with athletic success.[17] Among the eight coefficient estimates for the winter and spring quarters, corresponding to the overall effect of the winning percentage and the differential effect on males, none are statistically significant.

*Heterogeneity*

In tables 12 and 13, we explore the extent to which there are heterogeneous effects of athletic success on the gender gap. While such heterogeneity is interesting for a variety of reasons, a primary motivation for exploring heterogeneous impacts is to support the external validity of our estimates. For instance, if we see the same phenomenon across different groups at one institution, it lends credibility to the idea that similar effects might be present at institutions with different compositions of students. We first consider heterogeneity across ability and financial-aid eligibility to determine whether our main results are driven by individuals more likely to struggle with coursework or those from particular economic backgrounds. We then examine the possibility for heterogeneous effects across race.

Table 12 stratifies the estimates by various measures of ability and relative socio-economic status. In columns (1) through (4) the estimates are stratified by ability, where "low-ability" students are defined as those with cumulative SAT scores in the lowest tercile, and medium- and high-ability students are defined similarly.[18] These results provide strong evidence that the effect of athletic success on the gender gap is most prominent among lower-ability students. In particular, Column (2) suggests that, among low-ability students, the gender gap in grades increases by 0.03 in

[17] Over our sample time period, all bowl games that the football team participated in took place between the end of the fall quarter and the beginning of the winter quarter.

[18] Results are similar using high-school GPAs to construct the measure of ability.

response to a 25-percentage-point increase in the winning percentage, accounting for 14 percent of the existing gender gap (0.22) among those students. The estimated effect on the gender gap has the same sign for medium- and high-ability students but is smaller in magnitude and not statistically significant.

Columns (5) through (8) consider the effects stratified by "financial need" for the 70 percent of students who submitted a Free Application for Federal Student Aid (FAFSA).[19] These estimates suggest that the impact on the gender gap is largest among students from more-disadvantaged backgrounds. Again, the estimated effect on the gender gap is negative for all groups but is only significant for those in the highest tercile of financial need.

In Table 13 we provide estimates stratified by race. After again displaying the estimate based on the full sample, columns (2) and (3), show separate estimates for whites and non-whites. These estimates demonstrate that the winning percentage affects the gender gap among both whites and non-whites, but that the effect is particularly strong in the minority-student population. In columns (4) through (5), we further stratify non-white into Black, Hispanic, and Asian. Although estimates are imprecise at this level, there is suggestive evidence that the largest effects are found in the black-student population.

[19]To determine the level of the Pell grant, FAFSA-reported data are used to calculate two key measures: a cost of attendance (COA) (which varies across both institutions and students) and an expected family contribution (EFC) (which varies across students). The COA is a measure of the expected educational expenses a student will undertake. Individual institutions set the COA for a given student, based on the attributes of the institution and the student. For full-time students, their COA includes such costs as tuition and fees, books, supplies, transportation, other personal education related expenses, and room and board. For part-time students and those enrolled in correspondence courses COA expenses are more limited. Once the COA and EFC have been calculated, the value of the Pell award is formulaic. The level of an individual student's grant in a given year is the minimum of: (a) the difference between the Federal maximum Pell Grant and the student's EFC; and (b) the difference between the institution's COA and the student's EFC.

More broadly, we note that the estimated effects on the gender gap are negative in all regressions presented in tables 12 and 13. This suggests that the overall impact is not being driven by any one group in particular, even though there is heterogeneity in the effect on the gender gap among different groups.

### Estimated Effects on Drop-Out Behavior

In this section, we consider the effect on an outcome that is more-closely tied to students' long-run success—the probability of dropping out of school. Although it might seem more desirable to focus on the impact on graduation, we do not adopt this approach for two reasons. First, our panel is not long enough to be able to reliably measure graduation for many of the cohorts we observe. Second, while there is substantial variation in the team's winning percentage from year to year, there is much less variation from cohort to cohort (after averaging the team's success during their tenure) and the variation that does exist across cohorts is not as likely to be exogenous. As such, we instead focus on the probability of dropping out of the University before the next academic year, defined as an indicator variable equal to one if a student is not observed in the next fall term or any subsequent term and has not graduated. We do so with the caveat that this analysis may reflect impacts on the timing of drop-out behavior in addition to impacts on the probability of graduation.

An additional caveat to this exercise is that, lacking within-student variation from year to year, we cannot include student fixed effects in our regression models. Instead, we control for student characteristics with a rich set of covariates: math and verbal SAT score, high-school GPA, age at entry, race, and whether the student graduated from a private high-school. In addition, we control for the year and a student's accumulated credits in the current fall term, as in our previous analyses.

Before describing the results of this analysis, it is important to note that we often do not have clear predictions for the sign of the effect. On one hand, the success of the football team may directly reduce the probability that a student drops out because it leads to a more enjoyable college experience. On the other hand, the success of the football team may affect the probability that a student drops out through its impact on his or her GPA (and number of failed classes).

Column (1) of Table 14 shows the estimated effect based on the full sample.[20] The estimated coefficient on the winning percentage and its interaction with gender are both small and insignificant, suggesting that the success of the team does not affect drop-out decisions in the aggregate. However, columns (2) through (4) of Table 14 show that this masks substantial heterogeneity across ability. In particular, Column (2) reveals that, among those with low-SAT scores, a successful team reduces the probability that females drop out while having no significant effect on males. In light of the estimated effects on GPAs shown in Table 12, this impact on females may be driven by the positive impact on their GPAs but could also be a direct effect of the team's success; the lack of a significant impact on males suggests that the negative impact on their GPAs is offset by the direct effect of the team's success. The remaining columns of Table 14 explore heterogeneity across financial need, providing suggestive evidence that the team's success reduces the probability that high-need females drop out.

Table 15 considers heterogeneity across race. Generally, these estimates are too imprecise to draw any strong conclusions. However, they do suggest that the success of the football team reduces the probability that black females drop out and perhaps increases the probability that black males drop out. These results are consistent with

[20] Estimates are very similar if seniors are not included in the analysis.

the point estimates in Table 13, which suggested positive effects on black females' GPAs and negative effects on black males' GPAs.

## Survey Evidence on Mechanisms

To shed light on the underlying mechanisms driving our main results, we surveyed undergraduate classes during three regularly-scheduled class times in the 2011 spring term.[21] Of the classes surveyed, 183 students were enrolled in an introductory economics course and 80 were enrolled in either of two upper-division economics courses. The students enrolled in introductory economics are largely representative of the freshman and sophomore student body, as introductory economics is a general-education requirement for many majors and the majority (90 percent) of students are in their first or second year of school. The students we surveyed in the upper-division economics courses were primarily (90 percent) juniors and seniors.

### *Survey Design*

As part of the survey, we collected information on general student characteristics, interest in the university football team, and about known or anticipated behavioral changes around the outcomes of university football games. We focused on alcohol consumption, partying, studying, and class attendance, with questions worded to elicit differences in these behaviors when the football team wins relative to when the football team loses. In our reported survey results, we limit the sample to non-first-year students—53 percent of our sample. While the patterns we report are insensitive

[21] Our survey design is informed by Clotfelter (2011), who reports that students at highly selective big-time sports universities spend more time exercising and participating in team sports, are more likely to report binge drinking, and possibly spend less time studying and doing research.

to this restriction, this ensures that the survey respondents had experienced a regular-season loss.[22]

*Survey Results*

To broadly measure academic time use related to football success, we collected student responses to the question, "Does the success of the University of Oregon football team decrease the amount of time you study for classes?" Figure 4 summarizes student responses, where categorical responses range from "Definitely Not" to "Definitely." While both distributions are skewed right, the male distribution shows significantly higher mass to the right, which is consistent with relatively more males reporting a decrease in study time around a football "success." In fact, 24 percent of males report that athletic success either "Definitely" or "Probably" decreases their study time, compared to only 9 percent of females.

Figure 5 presents student responses to questions regarding changes in alcohol consumption, partying, studying, and class attendance—comparing behaviors when the team wins to when the team loses. In Panel A, we see that roughly 28 percent of males report a tendency to increase alcohol consumption when the football team wins rather than loses, while only 20 percent of females report the same.[23] Although this

[22]Figures for the first-year sample are available on request. In our preferred models, the influence of winning percentage on fall-term grades is independent of post-season bowl activity (i.e., subsequent to fall term grades being posted). However, while first-year students had not experienced a regular-season loss at the time of our survey, they had experienced a loss in the Bowl Championship Series game in January, 2011. However, one might worry that such a loss may overly influence first-year students' perceptions of how they respond to wins versus losses. Anticipating this, we also asked students to compare "big games" to other games, where big games are described as "a game against a rivalry team, a ranked team, a game that involves significant hype, etc." While we do not highlight these responses, similar patterns also appear in these survey responses.

[23]Conditional on reporting that one consumes alcohol, which we collected in the survey, the estimated difference increases to 12 percentage points (p=0.155).

difference is not statistically significant, the results indicate a large effect on males and females and remains suggestive of a more pronounced effect for males.

Panel B presents similar results for partying. Despite being unable to determine all activities encompassed by students' definitions of partying, this question allows us to broadly account for additional behavioral responses beyond alcohol consumption that are associated with increased excitement following a win. We observe that 28-percent of females report increased partying when the team wins versus 47 percent of males.[24]

In terms of educational activities, Panel C shows that the difference between males and females reporting that they study less when the team wins is approximately 14 percent. Panel D indicates that female students are slightly more likely to indicate an increased tendency to miss class associated with a win; however the result is small in magnitude and not significant.

In summary, our survey results lend strong support for a differential impact of athletic success on male and female behaviors. Both in absolute terms and relative to females, athletic success decreases males' academic time investments and increases time spent in distracting or risky behaviors. That said, we also find an impact on female behaviors, including studying, alcohol consumption, and partying. While our previous empirical analysis of grades can primarily speak to relative performance, our survey results suggest that the aggregate impact of athletic success on academic performance, or learning, likely extends to females.

[24]This difference is statistically significant at the five-percent level.

## Discussion and Conclusion

We identify the effect of football success with student-class-level data spanning nine football seasons, 1999 through 2007. Our preferred specifications include individual-student fixed effects to identify the effect off of longitudinal variation. That is, our preferred estimates are based on considering how a student's performance deviates from his or her own average performance as the winning percentage varies from its average, and then how this response varies across gender. With our analysis we show that male grades fall significantly with the success of the football team, both in absolute terms and relative to females. There is also pronounced heterogeneity among students, suggesting that the impact is largest among students from relatively-disadvantaged backgrounds and those of relatively low ability.

We also find evidence that the success of the football team reduces the probability that some students drop out. In particular, the groups of females whose GPAs increase with the success of the football team (i.e., those with low ability, those with high financial need, and blacks) are less likely to drop out of school after a successful season. At the same time, we cannot determine whether this result is driven by their improved academic performance or by more-direct effects of the team's success. That said, we find no evidence that the success of the football team has any impact on males' drop-out behavior, which may be due to offsetting effects of impaired academic performance and greater enjoyment of the academic year.

In addition to our main analysis, we offer insight into the underlying mechanisms that may be driving the systematic patterns evident in measured academic performance. In particular, we elicit student responses to questions about behaviors around football outcomes. Beyond confirming that there is a high level of student viewership and interest in football, survey responses reveal pronounced gender differences in behavioral responses to athletic success. Relative to females, males report being more likely to increase alcohol consumption, decrease studying, and increase partying around the success of the football team. Yet, both male and female students report that their behavior is responsive to athletic success. This suggests that female performance is likely affected by the performance of the football team as well but that this effect is masked by grade curving.

We view our research as taking one of the first steps toward documenting the non-monetary costs associated with college athletics. Of course, whether it is desirable to be investing large amounts of public and student money in college sports requires a broad consideration of all costs, in addition to the benefits that might be generated.

CHAPTER IV

# SUBSTANCE-ABUSE TREATMENT AND MORTALITY

## Introduction

The number of drug-induced deaths in the United States have more than doubled over the last decade—now totaling approximately 40,000 deaths annually and recently outnumbering traffic fatalities as the leading cause of injury-related deaths (Kochanek *et al.*, 2011). Moreover, these increases have occurred over a time period when many other injury-related deaths were declining and when resources devoted to drug-control policies were increasing.[1] Naturally, this has given rise to intense debate regarding the merits of intervention policies designed to limit drug abuse and its consequences. The primary objective of this paper is to analyze the effect of one such intervention policy—substance-abuse treatment—and its impact on drug-induced deaths.

Policies intending to reduce costs associated with drug abuse typically concentrate on either supply-side enforcement, such as regulations disrupting the distribution of an illicit drug, or a demand-side approach such as treatment or prevention. In the United States, federal spending primarily targets drug-supply-reducing policies, including law enforcement, interdiction, and international relations (ONDCP, 2005). In general, enforcement policies do not appear to be very effective.[2] Moreover, where researchers find reductions in drug-related outcomes due

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[1]For information on other preventable deaths see CDC WONDER online database, 2002-2009. See ONDCP (2005) for information on national drug control spending.

[2]Research considering supply side interventions includes drug-crime deterrence (Dinardo, 1993; Miron, 1999; Kuziemko and Levitt, 2004), disruptions in the supply of drug precursors (Dobkin and Nicosia, 2009; Dobkin, Nicosia and Wienburg, 2013; Cunningham and Liu, 2003, 2005) and, more broadly, the price-elasticities of illicit drugs (Chaloupka and Saffer, 1999; Caulkins and Reuter, 1998).

to enforcement efforts, they are typically temporary and, arguably, costly relative to alternatives. For example, Dobkin and Nicosia (2009) examine what they consider to be "quite possibly the DEA's greatest success in disrupting the supply of a major illicit substance," analyzing the effect of a major disruption in the supply of methamphetamine precursors in 1995. They find a temporary effect on drug prices, health outcomes and drug-related arrests—with drug prices returning to pre-intervention levels in four months and other outcomes recovering within 18 months.[3] Also examining enforcement policies, Kuziemko and Levitt (2004) find that the overall effect of dramatic increases in drug-offender incarceration rates from 1985-1996 led to a small reduction in related crime, and conclude that such law enforcement policies are likely not cost-effective. Finally, Dinardo (1993) finds no evidence that regional and time variation in drug seizures influence cocaine use or the price of cocaine.[4]

Substance-abuse treatment is one promising alternative approach to drug control, potentially reducing the demand for addictive substances and mitigating related externalities.[5] Moreover, it is a particularly relevant policy in light of changes brought about by the Affordable Care Act, which will require all health insurance sold on the exchanges to include services for substance-use disorders and will increase funding for these services (Buck, 2011). That said, researchers have yet to demonstrate a causal effect of substance-abuse treatment in reducing drug-related outcomes among the general population. This is primarily due to the nature of illicit-drug markets

[3]More recently, Dobkin, Nicosia and Wienburg (2013) find no changes in methamphetamine consumption or drug-related arrests associated with state laws targeting over-the-counter medicine used to produce methamphetamine.

[4]In related work Saffer, Chaloupka, and Dave (2001) analyze state spending on both supply and demand-side drug-control policies and argue that drug treatment spending may be a relatively efficient method to reduce drug abuse.

[5]Prevention is another primary demand-side intervention. In recent work, Anderson (2010) finds no effect of an aggressive anti-methamphetamine advertising campaign on youth methamphetamine use.

and the poor quality and availability of drug-use and treatment data. As such, analyses of substance-abuse treatment often rely on small samples of self-reported substance use and adverse consequences pre- and post-treatment. In such cases, attributing post-treatment changes in drug-related outcomes to treatment gives rise to serious concerns regarding bias induced by selection and the propensity for individuals entering treatment to regress to the mean.[6] Moreover, in studies where randomization is possible the findings are often difficult to generalize to broad populations due to small sample sizes or a narrow focus on specific treatment approaches and/or select demographic groups.

These limitations aside, a large literature provides evidence that treatment reduces drug-abuse and related problems. For instance, meta-analyses of resarch based on clinical trials consistently find significant declines in drug-use and related outcomes associated with all major modalities of treatment (Prendergast et al., 2002; Brewer et al., 1998; Griffith et al., 2000). Economic-evaluation studies further suggest that the benefits of treatment far outweigh the costs and that treatment is less costly than alternative drug-control policies such as incarceration or policing.[7] For example, Rydell and Everingham (1994) compare treatment and enforcement methods to reduce cocaine abuse using three evaluation criteria: consumption costs, number of users, and total societal costs. In each case they find a significant advantage for treatment, arguing that 34 million dollars in treatment has roughly the same impact as 246 million dollars devoted to enforcement strategies.

[6] A notable exception is Lu and Mcguire (2002), who consider the marginal productivity of outpatient treatment using an instrumental variables approach. They find a decrease in drug use associated with increased time in treatment.

[7] For reviews of related literature see Cartwright (2000), McCollister and French (2003).

Despite this evidence, questions remain regarding the efficacy of treatment in reducing drug abuse and its consequences. A common concern is that addictive behaviors may be unresponsive to treatment interventions as evidenced by low treatment completion rates (44%) and a high likelihood of relapse.[8] As such, opponents argue that treatment may not yield better results than currently-employed drug-control policies. In the case that treatment is beneficial, it is unclear whether treatment reaches those in need due to social stigma toward seeking treatment and institutional constraints. For instance, drug-court referrals, which offer drug offenders priority access to treatment as an alternative to incarceration, potentially crowd-out voluntary clients who are, arguably, more likely to benefit from treatment.[9] For example, Hser *et al* (2007) find that increases in court-referred treatment admissions in California displace clients seeking treatment on their own.

Finally, additional opposition to a treatment approach comes from community groups who argue that local services for drug users may actually be detrimental—citing perceived concerns for personal security and general neighborhood quality (Tempalski, 2007). Taken together, these issues question the efficacy a treatment-based approach to drug control and highlight the need to understand the underlying relationship between investments in treatment and drug-related consequences.

To my knowledge, this paper offers the first estimate of the causal effect of substance-abuse treatment on mortality. In particular, I provide a reduced-form analysis of substance-abuse treatment by estimating the effect of treatment facility openings and closings on county-level drug-induced mortality rates. My

[8] See SAMHSA (2008a) for treatment completion rates.

[9] Drug courts were first introduced in 1989 and have since spread to all 50 states, totaling 2,147 operating courts in 2008 (Stinchcomb, 2010). In 2007, 37 percent of reported admissions in the Treatment Episodes Data Set were referred to treatment from the criminal justice system (SAMHSA, 2008).

identification strategy leverages within-county variation in the number of substance-abuse treatment facilities over a period of 11 years (1998-2008), spanning 48 states. To account for the possibility that openings and closings systematically vary with other determinants of drug-induced deaths, I estimate models including state-by-year fixed effects in addition to a rich set of county-by-year controls. Finally, I offer evidence of an underlying mechanism associated with treatment by analyzing the effect of variation in the number of treatment facilities on substance-abuse treatment admissions.

The analysis reveals that treatment facilities significantly reduce mortality. The reduction in mortality is most prominent among drug-induced deaths, though I do find evidence of positive spillovers of treatment as facilities also reduce other drug-abuse-related causes including homicides, suicides and, to some extent, disease-related deaths. The preferred estimates indicate that an additional facility reduces a county's annual drug-induced mortality rate by 0.4 percent, suggesting that a 10 percent increase in the number of facilities lowers a county's drug-induced mortality rate by 2 percent. That the estimates are persistent across demographic and socio-economic groups suggests that the benefits of treatment are quite broad.

The remainder of this paper proceeds as follows. In Section 2, I discuss several key features of treatment and background on drug-overdose deaths. I describe the data and introduce the empirical methodology in sections 3 and 4. In Section 5, I present the main analysis on drug-induced mortality and the analysis of effects on other causes of death in Section 6. In Section 7, I show estimates using an alternative data source and an analysis of the effect of treatment facilities on treatment admissions. Finally, I conclude and discuss the implications of the results in Section 7.

## Background

According to the National Survey of Drug Use and Health, nine percent (23.1 million) of Americans age 12 or older are current illicit or non-medical drug users. One estimate of the economic costs of drug use—measured in terms of crime, health and productivity—totaled more than $193 billion in 2007 (NDIC, 2011). Though substance-abuse treatment is a promising avenue that may reduce these costs, treatment rates for those in need remain very low. In 2010, 90 percent of those classified as substance dependent or substance abusing did not receive treatment. Several commonly cited barriers to treatment take-up are cost constraints and a lack of insurance coverage.[10]

Evidence suggests, however, that reducing cost barriers alone is not sufficient to dramatically increase treatment admissions due to existing capacity constraints. For example, Dave and Mukerjee (2011) analyze the effect of state legislation that reduces out-of pocket costs for mental health and substance-abuse treatment and find a relatively small effect on treatment admissions. They argue that the effect on admissions is muted, in part, because of treatment capacity constraints.[11] As such, expansions in the number of treatment facilities likely relax capacity constraints leading to increased take-up of treatment.

[10]From 2007-2010, 38.1 percent of individuals making an effort to get treatment, but not receiving it report health coverage and cost as a primary reason. Other reported reasons for not receiving treatment include "not ready to stop using (30.3 percent), able to handle the problem without treatment (9.0 percent), no transportation/inconvenient (8.4 percent), might have negative effect on job (7.9 percent), had health coverage but did not cover treatment or did not cover cost (7.4 percent), might cause neighbors/community to have negative opinion (7.1 percent), and did not feel need for treatment at the time (6.5 percent)" (SAMHSA, 2011).

[11]The authors highlight supply and capacity constraints suggested by limited growth in the number of treatment facilities and increasing treatment waiting periods, which stand in stark contrast to the 35% increase in substance-related emergency department visits and a growing gap between those needing and those receiving treatment.

In this environment of existing capacity constraints and a growing need for treatment, the number of substance-abuse treatment facilities—the source of the variation used in this paper—may be a particularly relevant policy parameter. In the United States, over 13,500 stand-alone treatment centers are the primary setting for delivery of substance-abuse treatment, offering a wide range of drug-treatment programs and related services.[12] On average, these facilities are quite small—treating 276 individuals annually—and are heavily dependent on outside sources of funding.[13] The typical treatment episode includes detoxification and medically- and/or behaviorally-based treatment, followed by efforts to prevent or limit relapse after completion of treatment.[14]

Admissions rates into these specialized treatment facilities are higher for individuals who are white, male, unemployed, and less educated (SAMHSA, 2008).[15] According to the Treatment Episodes Data Set (TEDS), 96 percent of admissions relate to five main substances: alcohol (22 percent), alcohol with secondary drug abuse (18 percent), opiates (19 percent), marijuana/hashish (16 percent), cocaine (13 percent) and other stimulants (8 percent).[16]

Though an analysis of specific treatment programs is beyond the scope of this study, understanding several features of substance-abuse treatment provides insight

[12]According to National Survey of Substance Abuse Treatment Services (N-SSATS) data from 2002-2008, 87 percent of facilities offering treatment are not located in or operated by a hospital.

[13]These facilities are primarily privately owned (87 percent). 95 percent are licensed, certified, or accredited—typically by state health agencies/departments—to provide substance abuse treatment. (SAMHSA, 2008).

[14]In many cases, treatment facilities include ancillary services such as substance-abuse education, outreach activities, counseling, self-help groups, mental health services, and others (SAMHSA, 2008).

[15]In the United States, high rates of substance abuse or dependence are present across most demographic groups. That said, abuse rates are relatively higher among individuals aged 18-25, males, less educated, and those living in larger metropolitan areas (SAMHSA, 2011).

[16]The remaining 4 percent of admissions are related to sedatives, tranquilizers, hallucinogens, PCP, inhalants, over the counter medications, and other unreported drugs.

into the relationship between treatment and substance abuse. The majority of treatment services occur in an outpatient setting—in which clients do not reside at the treatment site.[17] Outpatient care delivers treatment programs and modalities, such as detoxification, methadone maintenance, regular outpatient, adolescent outpatient, and drug-court programs (SAMHSA, 2009). For more serious substance-abuse problems, facilities provide residential treatment in which clients temporarily live at the treatment site (e.g. inpatient detoxification, chemical dependency programs, therapeutic communities). While treatment modalities vary substantially and often target particular demographic groups or specific drug addictions, all modalities share similar goals to mitigate the consequences of drug abuse, encourage healthier lifestyles, and, ideally, promote abstinence among drug abusers.

Despite treatment innovations and a wide array of treatment options, drug-related problems continue to rise. In particular, drug-overdose deaths—a particularly costly consequence of drug abuse—have increased sixfold since 1980, with the most recent increases being largely attributable to a rise in prescription-drug abuse such as opioid analgesics (e.g. morphine, codeine, hydrocodone, oxycodone, fentanyl, and methadone).[18] For instance, over the past decade prescription opioid-related deaths have tripled and accounted for 40 percent of all drug-poisoning deaths in 2008. In the same year, heroine and cocaine—common recreational drugs—together account for approximately 20 percent of drug-poisoning deaths (Warner *et al*, 2011).

[17] Outpatient treatment services account for 90 percent of reported clients. The remaining admissions are provided in residential (9 percent) and hospital inpatient (1 percent) settings (SAMHSA, 2008).

[18] In the case of prescription drug abuse—where the majority of drug abusers obtain drugs from family or friends—typical supply-side interventions may be less effective relative to strategies such as treatment (SAMHSA, 2011).

In light of these trends, it is perhaps surprising that drug-induced mortality receives little attention within either the context of treatment or analyses of other drug-control interventions. For instance, in a recent review of opioid-dependence therapies Amato *et al* (2005) highlight that mortality is "seldom taken into account to assess the efficacy of treatments [due to the lack of long-term follow up in clinical trials]."

## Data

To identify county-level changes in the number of substance-abuse treatment facilities, I use data from the U.S. Census Bureau's County Business Patterns (CBP). CBP data report the annual number of substance-abuse treatment clinics (a single physical location) in each U.S. county for both outpatient and residential facilities from 1998-2008.[19] Although classified separately in the CBP data, residential and outpatient establishments often offer both residential and outpatient treatment services. For instance, 35 percent of facilities offering residential care also offer outpatient care.[20] Therefore, estimating the effects separately for outpatient and residential facilities would not be informative as residential and outpatient services are not distinctly identified. As such, I combine outpatient and residential classifications, using the total count of establishments as an indicator for county-level provision of substance-abuse treatment.

[19]The following six-digit NAICS codes identify treatment establishments: 621420 —"Outpatient mental health and substance abuse centers" and 623220—"Residential Mental Health and Substance Abuse Facilities." 55 percent of facilities within the sample are classified as outpatient centers. Many of these facilities also offer mental health services, though according to N-SSATS (2008) only 8 percent of known facilities indicate a primary focus on mental health services.

[20]Based on author's calculations using 2002-2008 N-SSATS data.

To calculate drug-induced mortality rates, I use restricted-use National Center for Health Statistics Multiple Cause of Death Data and population data from the National Cancer Institute's Surveillance Epidemiology and End Results (Cancer-SEER) program.[21] I measure drug-induced mortality using causes of death with specific reference to drug-induced poisoning, identified by International Classification of Diseases (ICD) codes.[22] Over the sample, drug-poisoning deaths average 25,286 annually and account for nine out of ten deaths categorized by the Center for Disease Control and Prevention (CDC) as drug induced.[23] Notably, drug-poisoning deaths are a relatively acute outcome, plausibly responsive to annual changes in treatment availability and are not inclusive of deaths that result from adverse effects of properly administered substances, such as an allergic reaction to a prescribed dosage, where we would not expect treatment to have a meaningful impact.

I limit my analysis to U.S. counties with at least one treatment facility over the 1998-2008 time period and counties with available identifiers in the 48 contiguous states.[24] The resulting data include drug-induced mortality and treatment facility data for 2,409 counties in 48 states, spanning 11 years.[25] In table 1 Column (1) I

[21] As reported by Stevens *et al* (2011), the Cancer-Seer population data are more accurate than data interpolated from the Census because they "are based on an algorithm that incorporates information from Vital statistics, IRS migration files, and the Social Security database." The Cancer-SEER population data are also used to construct county-by-year demographic controls.

[22] See Appendix Table 24 for a breakdown of ICD-10 and ICD-9 codes used in this analysis. This measure of drug-induced deaths also includes alcohol poisonings, which account for 1.2 percent of drug-induced deaths.

[23] The remaining CDC categories of drug-induced causes include deaths from chronic drug use, leading to medical conditions such mental and behavioral disorders or other long-term drug-induced diseases which may not be responsive to annual changes in treatment provision. For a complete list of deaths categorized as drug induced by the CDC see Kochanek et. al (2011).

[24] Specifically, I drop all counties in HI and AK in addition to 14 total counties in MT, VA, CO, and FL, where county identifiers are inconsistent or unavailable. I assume zero facilities when none are reported in a given year.

[25] Over the same time-frame, the aggregate number of facilities increases from 12,019 to 15,411.

present summary statistics, indicating an average of 49 facilities and 9.4 annual drug-induced deaths per 100,000 residents. Importantly, there is substantial variation in the number of facilities, with the average county experiencing 21 facility openings and 10 closings from 1998 to 2008.[26] Column (1) also shows relatively higher drug-induced mortality rates for males, whites, and individuals ages 15-64.

## Empirical Strategy

I begin by estimating a simple model that relates a county's drug-induced mortality rate to its number of treatment facilities:

$$Mortality_{ct} = \alpha_c + \alpha_t + \theta Facility_{c,t-1} + \epsilon_{ct}, \tag{4.1}$$

where $Mortality_{ct}$ is the drug-induced mortality rate per 100,000 residents in county $c$ in year $t$, $\alpha_c$ and $\alpha_t$ are county and year fixed effects, $Facility_{c,t-1}$ is the number of substance-abuse treatment facilities in county $c$ in year $t-1$, and $\epsilon_{ct}$ is a random error term.

This model includes county fixed effects to account for fixed county characteristics that may correlate with drug-induced mortality. So, for example, inherent differences between urban and rural counties or time-invariant measures of county public services and drug-control policies will not confound the estimates. I also include year fixed effects to account for aggregate time-varying shocks such as aggregate economic conditions and changes in the national drug-control strategy. My primary measure of facilities is the count of substance-abuse treatment facilities lagged one time period—implicitly assuming that previous year facility openings and closings

[26] Here I define an opening as a net increase in the number of facilities from one year to the next and define closings similarly.

impact current year drug-induced deaths.[27] For this and all subsequent specifications, I apply county-population weights and correct for possible clustered standard errors at the county level, as errors may be correlated across years within a given county.[28]

Using this baseline approach, $\theta$ measures the extent to which a county's drug-induced mortality rate deviates from its average as the number of treatment facilities changes within that county, controlling for common shocks. If $\hat{\theta}$ is the causal effect of one additional treatment facility, one must assume that the annual variation in the number of treatment facilities within a county is orthogonal to other determinants of drug-induced mortality. I relax this assumption in Eq. (2) by controlling for state-by-year fixed effects and a rich set of county-by-year controls. In particular, I estimate the following regression equation:

$$Mortality_{cst} = \alpha_c + \gamma_{st} + \theta Facility_{cs,t-1} + \beta X_{cst} + \epsilon_{cst}, \tag{4.2}$$

where the notation is similar to Eq. (1), $\gamma_{st}$ are state-by-year fixed effects, and $X_{cst}$ is a set of county-by-year controls, including the unemployment rate, firm births, per-capita income, the number of law enforcement officers and total crimes per 100,000, and the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old.

State-by-year fixed effects flexibly control for state-specific shocks such as changes in state-level policy measures influencing drug-abuse. For example, these controls will

[27]In Appendix Table 27, I also show estimates based on models with additional lags. The estimates are consistent with the prior that previous year investments in treatment facilities impact current year mortality rates. Relatedly, I show that there is no significant relationship between current-year mortality rates and subsequent-year number of treatment facilities, addressing the concern that counties are responding to increased drug-deaths by investing in additional facilities.

[28]In Appendix Table 25, I explore estimates using unweighted models. As expected given recent research by Solon, Haider, and Woolridge (2013) the estimates are similar but estimated with slightly less precision than the weighted estimates which correct for heteroskedstic errors.

account for year-to-year changes in funding for health and law-enforcement services which typically originate at the state or federal level.

Eq. (2) also controls for year-to-year changes in county-year covariates including demographic composition, local economic conditions, law enforcement, and crime. Including demographic controls speaks to the concern that age-, race-, or gender-related compositional changes in a county's population may affect drug-induced mortality rates and investments in treatment. The controls for local economic conditions include county unemployment rates, the number of new firm births, and per-capita incomes.[29] These controls account for the possibility that treatment facilities and drug-induced deaths are correlated with local economic conditions. In particular, existing studies indicate that unhealthy behaviors and mortality are typically pro-cyclical; with mortality-rates and unhealthy behaviors such as alcohol consumption, smoking and physical inactivity increasing in response to better economic conditions (Ruhm 2005, 2006).[30] Inasmuch as drug-induced mortality follows a similar pattern and facility openings are pro-cyclical, excluding these controls would lead to a downward bias.

Finally, including controls for the number of law-enforcement officers and county-level crime rates addresses the concern that counties respond to drug-related problems by investing in law-enforcement interventions.[31] These controls are important, for instance, if expenditures toward law enforcement are a substitute for investments in

[29]County unemployment rates are from the BLS Local Area Unemployment Statistics. Firm births include all county-level firm births reported by the U.S. Census Statistics of U.S. Businesses. Per capita income was obtained from the Bureau of Economic Analysis Local Area Personal Income Data.

[30]Mortality also increases with income receipt as measured by transfer payments, tax rebates, and social security benefits (Dobkin and Puller, 2007; Evans and Moore, 2010).

[31]The number of law-enforcement officers and county-level crime rates are calculated using the FBI's Uniform Crime Reports and agency-specific employment reports available in the Law Enforcement Officers Killed and Assaulted (LEOKA) database.

substance-abuse treatment. In such a case, not including controls for the number of officers would result in downward biased estimates. Alternatively, co-occurring investments in enforcement and treatment would yield an upward bias.[32]

Together, the inclusion of state-by-year fixed effects and county-by-year covariates in Eq. (2) addresses concerns regarding omitted variables that may be systematically related to the number of facilities and drug-overdose deaths. That said, one could argue that *other* unobservable factors may confound the estimates. As such, the degree to which including the current set of controls affects the magnitude and precision of the estimates is informative regarding the exogeneity of county-level facility openings. For instance, if the estimates are not particularly sensitive to the inclusion of these county-by-year covariates, we can be more confident that related unobservables are similarly not crucial for identification. Alternatively, if the estimates are sensitive to county-by-year controls, we may be more concerned about other unobservable factors.

With this specification, the estimate of $\theta$ is the extent to which changes in a county's drug-induced mortality rate (relative to its average) respond to changes in the number of facilities, controlling for state-specific time shocks and other covariates.[33] As a robustness check to this approach, in Appendix Table 26 I also report estimates using Poisson models and find results consistent with the OLS estimates.

An outstanding concern is whether or not individuals seek treatment outside the border of their county of residence. Given the that the the average county has 49

[32]In Appendix Table 28, I assess the importance of these issues by estimating models considering how the number of facilities responds to local economic conditions, law enforcement, and crime. While the number of facilities does seem weakly related to local economic conditions, changes in law enforcement and crime are not predictive of openings and closings.

[33]In Section 5.2, I also estimate models using mortality rates of various demographic groups as the dependent variable to explore heterogeneous effects.

facilities and that 90 percent of admissions occur in an outpatient setting requiring frequent travel to the facility, it seems reasonable to assume that most treatment occurs within the county of residence. That said, inasmuch as crossing county borders to seek treatment does occur, the estimates would understate the effect of a treatment facility.

## Results

### *Effects on Drug-Induced Mortality*

In Table 17 I present regression results based on the models specified in Eq. (1) and Eq. (2). In columns (1) through (5) I show a progression of estimates, starting with the baseline model in Column (1) and leading to my most-flexible specification in Column (5). Including county and year fixed effects, the estimate in Column (1) suggests that an additional facility reduces a county's drug-induced mortality rate by 0.036 (0.39 percent) per 100,000 residents and is significant at the one-percent level. In columns (2) through (5), I add state-by-year fixed effects as well as county-level time-varying controls for demographic changes, local economic conditions, and measures for law enforcement officers and crime. That the estimates are not particularly sensitive to these additional controls—remaining significant and similar in magnitude—lends confidence that variation in the number of county-level facilities is not systematically related to other county-level determinants of drug-induced mortality.

In Column (5), the estimate indicates a significant 0.4 percent decline in drug-induced mortality associated with an additional treatment facility. To put the magnitude into context, the estimate suggests that a 10 percent increase in the number of facilities decreases a county's drug-induced mortality rate by 2 percent.

The estimates together provide evidence that treatment, as measured by expansions in treatment facilities, does reduce drug-induced mortality.

### *Heterogeneous Responses to Treatment Facilities*

In tables 18, 19, and 20, I explore the extent to which there are heterogeneous effects of treatment facilities on drug-induced mortality across age, race, gender, and county characteristics. Documenting any differential effects of treatment provides useful information for public policy and practitioners of treatment and speaks to the possibility that my main results are driven by a particular demographic group. Moreover, differential responses to treatment are interesting as the propensity toward addiction and overdose, and the likelihood of seeking treatment varies dramatically by individual characteristics and circumstances. Accordingly, in the following sections, I also provide context as to the recent trends in substance abuse and treatment across different groups.

#### Heterogeneity by Age

As highlighted in Table 16, drug-induced mortality rates vary substantially across age categories. Figure 6 further emphasizes these differences, depicting the age profiles of these deaths and how they have changed over time. In particular, drug-induced mortality increases dramatically from 1998-2008 and follows a similar pattern—increasing then decreasing with age, with the highest mortality rates among the age 20-60 demographic. As such, when considering potential lives saved through treatment, this age demographic is of particular interest.

In Table 18, I present the estimated effects of treatment facilities on age-specific drug-induced mortality rates. Column (1) again provides the main result for all ages for comparison and columns (2) through (10) present the estimates in age bins of 10 years. Column (2) indicates no effect upon children less than 10, which is not surprising as drug-induced deaths of young children are primarily accidental poisonings and should not respond to changes in the number of facilities. In contrast, the remaining estimates for individuals older than 10—columns (3) through (10)—suggest significant reductions in drug-induced mortality. Though the magnitudes of the point estimates generally increase then decrease with age, there are no significants differences in the percentage impacts among individuals older than 10 years.

Perhaps contrary to our priors, expansions in treatment facilities also reduce drug-induced mortality among older populations (columns (8) and (9)). That said, from 2004-2009 substance-abuse treatment admissions of adults ages 50 or older have more than doubled, with older-adult treatment-admission rates increasing relative to those of younger adults.[34] These admissions are primarily driven by alcohol and prescription-drug abuse, though recent research suggests increasing use of illicit drugs, such as cocaine and heroin, among older adults (Arndt, Clayton, and Schultz, 2011).[35]

Heterogeneity by Race and Gender

I next explore the extent to which mortality rates by race and gender differentially respond to the number of treatment facilities. A breakdown by race is interesting as the type and severity of drug abuse often correlates with race and, as such, we may expect a differential response to treatment. Figure 2 Panel A presents the race-

[34] In 2005, TEDS reports 4,622 treatment admissions for individuals age 70 or older. For a discussion of substance-abuse disorders in the elderly see Menninger (2002).

[35] To explore this issue further, in Appendix Table 29 I consider the extent to which there are differential effects of treatment facilities on alcohol-poisoning mortality rates across age categories. The estimates suggest that facilities significantly affect alcohol-poisoning mortality rates only among individuals ages 40-79, with the largest effects among older adults ages 60-79. As such, reductions in alcohol-poisoning deaths account for at least a small part of the relatively large percentage impact on elderly adult drug-induced deaths in Table 18.

specific drug-induced mortality rates over the sample time frame, showing white rates surpassing those for blacks beginning in 2002. These trends are consistent with recent increases in prescription-drug abuse primarily among whites and—prior to 2002—relatively high black mortality rates corresponding with high rates of drug abuse (driven primarily by heroin and cocaine) among blacks.[36]

In Table 19, I first show the estimated effects by race after displaying the estimate for the full sample in Column (1). The magnitude of the percentage impacts across columns (2) through (4) indicate a relatively large effect of treatment on drug-induced mortality among minorities. In particular, an additional treatment facility reduces black mortality rates by 0.48 percent. Given that there are relatively few blacks in many counties, I also consider estimates restricting the sample to counties within the highest tercile of proportion of county residents that are black (greater than 8 percent black). The estimates—presented in Table A3 in the Appendix—indicate an even larger percentage impact (0.81) on drug-induced mortality rates among blacks.

I also estimate models focusing on gender-specific mortality rates. As highlighted in Table 16 and Panel B of Figure 7, drug-induced mortality rates are higher for males than females throughout 1998-2008. In addition, males account for 70 percent of reported treatment admissions (SAMSHA, 2009). That said, existing studies suggest that treatment initiation, completion, and time spent in treatment do not differ by gender (Green *et al*, 2002). The estimates, shown in columns (5) and (6) of Table 19, indicate a 0.4 percentage impact of treatment facilities on drug-induced mortality rates for males and females.

[36]For a discussion of trends in drug-induced deaths and drug abuse by race see Paulozzi and Annest, 2007.

Heterogeneity Across County Types

In Table 20, I show estimates stratified by county urban classification and per-capita income. The motivation for doing so is to consider differential effects of treatment facilities across place environments—which have been shown to contribute to differences in substance abuse and related outcomes. In particular, abuse rates are generally higher in urban areas and among individuals with low socioeconomic status (SAMHSA, 2011). That said, looking across urban classifications, Figure 8 shows only a small and declining difference between urban and rural drug-induced mortality rates.

After displaying the estimate based on the full sample, columns (2) through (3) separately consider the effect for large urban counties, medium and small urban counties, and rural counties, using the National Center for Heath Statistics (NCHS) urban-rural classification scheme for counties.[37] Column (1) indicates an estimated effect of 0.37 in large urban counties (average population of 1.4 million), Column (2) shows a slightly smaller effect in medium and small urban counties (average population of 164,000), and Column (3) indicates no significant effect of treatment facilities in rural counties (average population of 30,000). Reasons for the lack of an effect in rural counties may include relatively low substance-abuse rates, high stigma of seeking treatment, or relatively few treatment options in rural counties.[38] Notably however, the larger counties considered in columns (1) and (2) account for approximately 85 percent of the total population in the sample.

[37] See 2006 NCHS Urban-Rural Classification Scheme for Counties.

[38] The average number of substance-abuse treatment facilities in large urban, medium/small urban and rural counties is, respectively, 122, 20 and 2.

In columns (4) through (6), the estimates are stratified by per-capita incomes, where "low income" counties are defined as those with average per capita incomes in the lowest tercile, and medium- and high-income counties are defined similarly. Though the results are relatively noisy for low-income counties, the estimates provide evidence that the effect of treatment facilities is relatively large in counties with low average per-capita incomes.

Broadly speaking, tables 3-5 offer suggestive evidence that particular demographic groups and place environments are more responsive to treatment interventions. That said, the percentage responses to treatment are typically similar in magnitude and—with few exceptions—negative and precisely estimated, suggesting that the benefits of treatment facilities are quite broad and not driven by any one group in particular.

### Effects on Other Causes of Death

In this section, I investigate whether treatment facilities affect other causes of mortality. Though it is likely that substance-abuse treatment affects other causes of death, the expected sign of the effect is unknown. On one hand, a decline in drug-induced deaths may lead to an increase in other causes of death consistent with the competing risks model (see e.g. Arthur, 1981; Honore and Lleras-Muney, 2006). In particular, a host of risk factors act as competing influences contributing to mortality. As such, we might expect a decrease in drug-induced deaths to lead to an increase in deaths related to competing mortality risks such as motor-vehicle accidents or suicides. On the other hand and perhaps more likely, positive spillovers of substance-abuse treatment may lead to declines in other causes of death. As substance abuse has far-reaching effects on general health outcomes, drug-induced deaths may be one of many causes of death reduced by treatment.

As such, an analysis of the effects on other causes of death should not be thought of as a typical falsification exercise. However, in the context of identifying an effect of substance-abuse treatment, the extent to which the effects on non-drug-induced causes of death are smaller in magnitude is informative regarding the validity of the research design. Moreover, to the extent that drug treatment does reduce other causes of death, we would expect there to be a link between these deaths and drug abuse.

In this setting—where the number of deaths across causes varies widely—a natural approach is to estimate a log-linear model, where the estimates represent the percentage effect of an additional facility on mortality rates.[39] After first plotting the estimated effect on drug-induced mortality, Figure 9 provides the estimated effects on all causes, all causes excluding drug-induced deaths, and deaths associated with cardiovascular disease, cancer, infection / immune deficiency, other disease (respiratory, kidney and degenerative brain diseases), motor-vehicle accidents, suicide, and homicide.[40] The estimates indicate a relatively large effect of facilities on drug-induced mortality rates, lending added confidence that the research design is identifying an effect of treatment. In particular, the estimated effect on drug induced mortality rates (.5 percent) suggests that these deaths are much more responsive to treatment facilities than any other cause of death.

That said, the estimates also provide evidence for positive spillovers of treatment leading to fewer deaths associated with infection / immune deficiency, suicide, and

[39]To avoid dropping county-year observations with zero deaths, I replace zeros with ones before calculating the mortality rates. Results from Poisson regressions are similar to the results from the log-linear specification.

[40]See the Appendix Table 24 for specific ICD-10 and ICD-9 codes used for each category. I define cause of death categories similar to Stevens et al 2011

homicide.[41] These deaths contribute to a decline in the all causes category in Figure 9 and are plausibly related to drug abuse. For instance, drug abusers—injection abusers in particular—are at a high risk for infection and, moreover, substance abuse is generally associated with immune deficiencies (Sacerdote, 2006)[42] Similarly, there is a well-documented relationship between drug abuse and deaths related to suicide and homicide (Moscicki, 1995; Brownstein et al., 1992).

Deaths of individuals over the age of 65—accounting for the majority of deaths in Figure 9—likely add considerable noise to these estimates. As such, in Figure 10 I show results from the same models using log mortality rates of individuals less than 65 years old as dependent variables. In Figure 10 we see similar evidence of positive spillovers of treatment reducing deaths related to infection / immune deficiency, suicide and homicide. Combined with the effect on drug-induced mortality, these effects contribute to relatively small percentage declines in mortality rates related to all causes and non-drug induced causes. Once again, the estimated effects across all non-drug induced causes of death in Figure 10 are much smaller than the estimated effect on drug-induced mortality. Taken together, figures 9 and 10 suggest that treatment saves lives among other causes of death, consistent with positive spillovers of reduced drug abuse, but that the percentage impact is much larger for drug-induced deaths.

[41] Appendix table 31 show similar results using the linear model previously specified in Eq. (2).

[42] Within the ICD codes comprising the infections and immune deficiencies category, hepatitis C, sepsis, and entercolitis are particularly responsive to treatment facilities. In each case, substance abuse is a plausible contributing factor.

## Estimates Using Alternative Data on Facilities

To lend added confidence to my main results and provide insight into an underlying mechanism associated with treatment, in this section I estimate models using several additional data sources. I first estimate the effect of treatment facilities on drug-induced mortality using data which offer the ability to focus specifically on treatment facilities that are licensed or certified to provide substance-abuse treatment. I then estimate models that consider the effect of treatment facilities on substance-abuse treatment admissions using administrative admissions data.

### *N-SSATS and TEDS Data*

As an alternative source to measure the number of county substance-abuse treatment facilities, I use the National Survey of Substance Abuse Treatment Services (N-SSATS)—a survey administered to "all organized substance abuse treatment facilities known to SAMHSA [Substance Abuse and Mental Health Services Administration]." Though N-SSSATS provides valuable facility-specific details not available in CBP data, it has several shortcomings. In particular, in addition to being voluntary survey-based data, N-SSATS spans a shorter time-period than CBP data and does not correspond to the calendar year—instead providing a count of facilities at varying reference dates. In the analysis, I use N-SSATS data from 2002-2008—throughout which there is a consistent reference date of March.

These shortcomings noted, the detail available in N-SSATS offers some advantages. First, in my analysis of drug-induced mortality, these data allow me to consider a sample of facilities that are not concurrently providing primary health care and are licensed or certified to provide substance-abuse treatment.[43,44] As such, this measure uses facilities that are particularly focused on substance-abuse treatment.

Second, N-SSATS data separately identify funded and unfunded facilities, which is important when I consider the effect of facilities on substance-abuse treatment admissions. That is, county-level treatment admissions data are largely only available for *publicly-funded* facilities. Therefore, unlike CBP data, using N-SSATS data provides the ability to consider the effect of an additional funded facility on treatment admissions into funded facilities.

Substance-abuse treatment admissions data come from the Treatment Episodes Data Set (TEDS). TEDS is a national data-set collected by SAMHSA from state administrative systems and includes information on approximately 2 million annual substance-abuse treatment admissions. While publicly available by Metropolitan Statistical Area, in this analysis I use a special tabulation of TEDS which includes county-level admissions from 2002-2008. In this data, an admission is the initiation of a treatment episode in a non-hospital facility, where the duration of an episode can vary from days to months depending on the the type of service and completion of treatment.[45]

As alluded to previously, TEDS is not a census of all substance-abuse treatment admissions. These data primarily represent admissions into publicly-funded facilities and, with few exceptions, do not include admissions into facilities not receiving public funds. Focusing on a clear measure of admissions into publicly-funded facilities, I

[43]Approximately 95% of surveyed facilities are licensed or certified. Facilities offering primary health care account for 2 percent of all surveyed facilities.

[44]Primary care facilities account for 1-2 percent of all surveyed facilities. These sample restrictions are not made when analyzing treatment admissions as these categories of facilities may also report treatment admissions.

[45]As facilities may admit an individual multiple times within the calendar year, the admission count does not necessarily represent unique individuals.

restrict the sample to states that require publicly-funded facilities to report treatment admissions, in total dropping 4 states from the analysis of treatment admissions (Arkansas, Colorado, Maryland, and Massachusetts).[46,47]

Using these data, I employ sample restrictions similar to my main analysis—focusing on U.S. counties with at least one treatment facility from 2002-2008, counties within the 48 contiguous states and dropping counties with inconsistent or unavailable county identifiers. In Table 21 I show summary statistics for the samples using N-SSATS to identify treatment facilities, with Column (1) showing the statistics for the sample used to consider effects on drug-induced mortality and Column (2) showing the same for the sample used to consider treatment admissions. These statistics are largely similar to those in Table 1, with Column (2) also reporting an average of 7,192 county-level treatment admissions.

*Effects on Drug-Induced Mortality*

Analogous to Table 2, I first estimate the effect of treatment facilities on a county's drug-induced mortality rate. Using the models specified in Eq. (1) and Eq. (2), Table 22 presents these estimates, starting with the most parsimonious model in Column (1) and concluding with my most flexible specification in Column (5). The estimates across columns (1) through (5) closely resemble those based on CBP data, similarly indicating a significant effect of treatment facilities on drug-induced mortality rates. In particular, Column (6) indicates that that an additional facility

[46] The resulting sample includes 43 states.

[47] While approximately 60 percent of all facilities do receive public funds, there remain substantial differences in state-reporting requirements that introduce uncertainty as to the portion of all admissions actually accounted for by TEDS. For instance, some states report only publicly funded admissions instead of all admissions in facilities receiving funding. Also, some states allow voluntary reporting of admissions into unfunded facilities.

reduces a county's drug-induced mortality-rate by 0.32 percent—slightly smaller than the impact in Table 17 (0.40 percent).

### *Effects on Treatment Admissions*

I next estimate the effect of changes in the number of publicly-funded facilities on treatment admissions into publicly funded facilities. Using N-SSATS data allows me to separately construct counts of the number of funded and unfunded facilities in each county. While I am particularly interested in the coefficient on the number of funded facilities—corresponding to my measure of treatment admissions—I also control for changes in the number of unfunded facilities. That is, funded-facility admissions are likely not independent of the number of unfunded facilities.[48] For instance, an additional unfunded facility may decrease reported admissions into funded facilities as individuals opt towards treatment at the unfunded facility. In this case, not accounting for changes in unfunded facilities would lead to a downward bias in the estimated effect of an additional funded facility. Alternatively, if increases in funded facilities correspond with decreases in unfunded facilities, the estimate may overstate the effect of an additional funded facility on treatment admissions.

As such, Table 23 presents estimates separately accounting for the number of funded and unfunded facilities. Once again progressively adding controls, columns (1) through (5) show a significant relationships between treatment admissions and the number of funded facilities. In Column (5), the estimate suggests that one additional funded facility leads to 26 additional treatment admissions. Not surprisingly, the estimates crucially depend on accounting for the number of unfunded facilities. In

[48]In recent work, Cohen, Freeborn, and McManus (2013) consider the market for outpatient substance-abuse treatment in small rural counties and find that public outpatient clinics crowd out their privately-owned counterparts. As private facilities are less likely to receive funding, this further stresses the importance of controlling for the number of unfunded facilities in my specifications.

particular, the results suggest that increases in unfunded facilities decrease admissions into unfunded facilities. That the estimate on unfunded facilities is relatively large in magnitude is somewhat unexpected, but plausible as unfunded facilities are more likely to offer outpatient services—a setting which accounts for the bulk of treatment admissions—and are more likely to offer ancillary services. Moreover, private-for-profit facilities are less likely to receive public funding and may incorporate newer ideas and technologies, attracting relatively more patients.

It is important to note that admissions provide one potential mechanism whereby treatment facilities may affect drug-induced mortality. Other mechanisms—including perceptions toward treatment or factors influencing the quality and accessibility treatment—may also contribute to declines in substance abuse. On the margin it is not clear which mechanisms are most important in reducing fatal overdoses. That treatment admissions do respond to the number of facilities, the estimates in Table 23 can be thought of as "proof of concept," demonstrating that treatment facilities lead to a change in an underlying factor associated with treatment. That said, given these many potential mechanisms and that these estimates are based on a subset of facilities, one should not interpret the estimated effect of treatment facilities on drug-induced mortality in the context of treatment admissions.

## Discussion and Conclusion

The recent dramatic rise in drug-overdose deaths has become an increasingly important public-health concern. In this paper, I provide evidence for the efficacy of substance-abuse treatment—as measured by treatment-facility openings and closings—in reducing these deaths. My main specification suggests that increasing the number of treatment facilities by 10 percent would lead to a 2 percent decline in the drug-induced mortality rate. The estimates are robust across a range of individual and county characteristics, though the benefits of treatment facilities are more pronounced among racial minorities, in urban counties, and in counties with low per-capita incomes. Offering evidence that the expansion of facilities is associated with an underlying mechanism of treatment, I show that an additional treatment facility leads to an increase in treatment admissions. My results also suggest indirect benefits of treatment in reducing other causes of death likely related to drug abuse.

Together, the estimates indicate that an additional facility reduces mortality by 1.2 total deaths in the average county per year. Considering these mortality benefits alone, a back of the envelope calculation suggests that the economic value of expansions in treatment facilities exceeds the associated costs. In particular, facilities average 276 annual admissions at an average cost of $2,515 per admission.[49] Given the total annual cost of a facility ($694,140) and the estimated reduction in mortality (1.2 deaths), the cost of saving one life is approximately $578,450 which is far less than the accepted range of estimates for the value of a statistical life.[50]

As a whole, the results offer encouraging support for the efficacy of substance-abuse treatment in reducing drug-related mortality, which is particularly relevant given the concern that current enforcement policies are ineffective. Moreover, as mortality is a relatively infrequent consequence of drug abuse, the estimates may only capture a small portion of the benefits of substance-abuse treatment. As such, these findings also highlight the need to better understand the extent to which treatment

[49] The average annual admissions are calculated using 2002-2008 N-SSATS data and the costs of admission are based on the Alcohol and Drug Services Study (Shepard *et. al,* 2003).

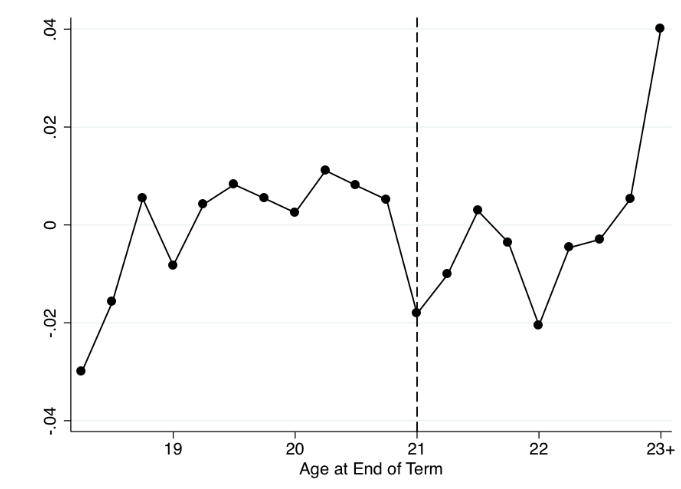
[50] For example, Viscusi and Aldy (2003) find that the median value of a statistical life (VSL) in the US is approximately $7 million and Aldy and Viscusi (2008) estimate age-group specific VSLs as follows: $3.2 million (ages 18-24), $9.9 million (25-34), $9.9 million (35-44), $8 million (45-54) and $3.8 million (55-62).

can affect other drug-related outcomes such as crime, labor productivity, and child abuse.

# APPENDIX A

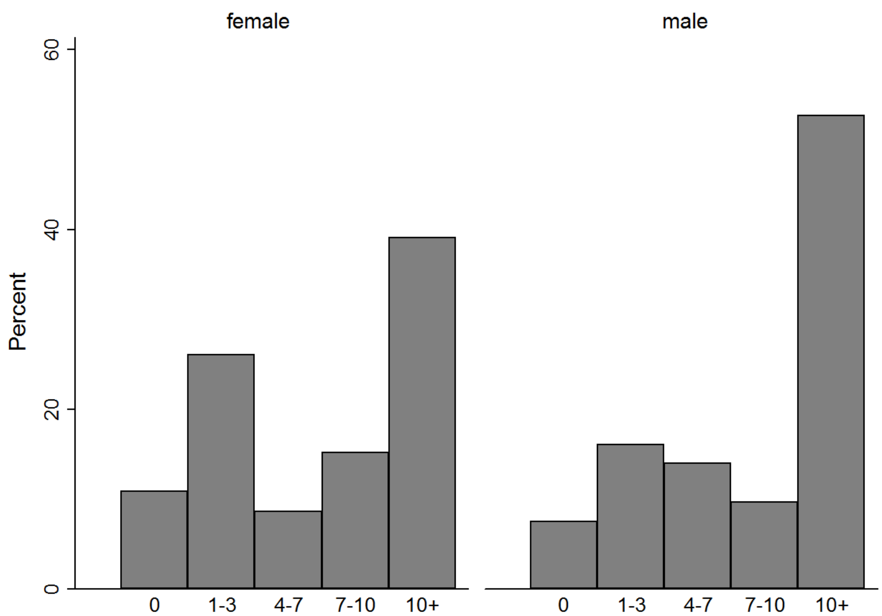
## FIGURES AND TABLES

FIGURE 1. Normalized GPAs by Age Adjusted for Individual, Accumulated Credits, and Course-type Fixed Effects



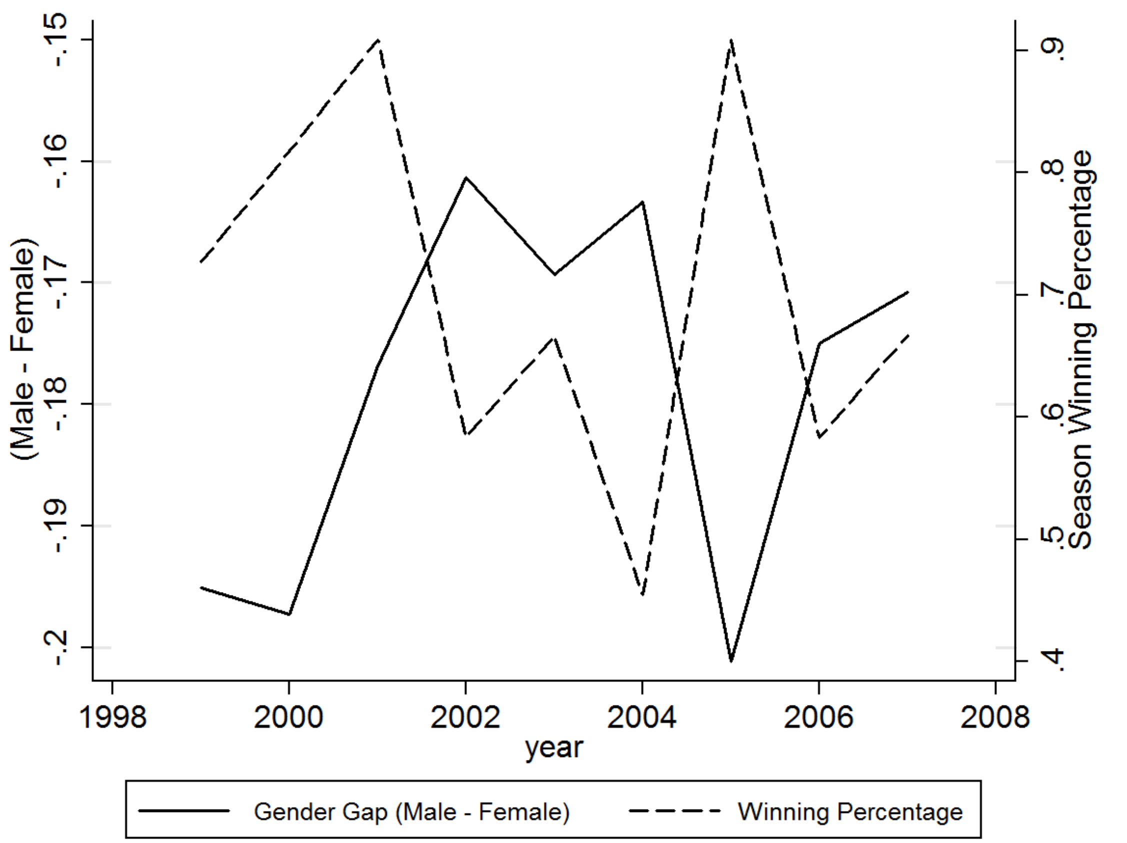

Notes: This figure plots average residuals from a regression of students' normalized GPAs on individual fixed effects, fixed effects for a student's cumulative credits at the beginning of a term, subject-by-level fixed effects, and term fixed effects.

FIGURE 2. Responses to the question: "Of the 12 regular-season University of Oregon football games in the 2010 season, how many did you watch on TV or in person?"



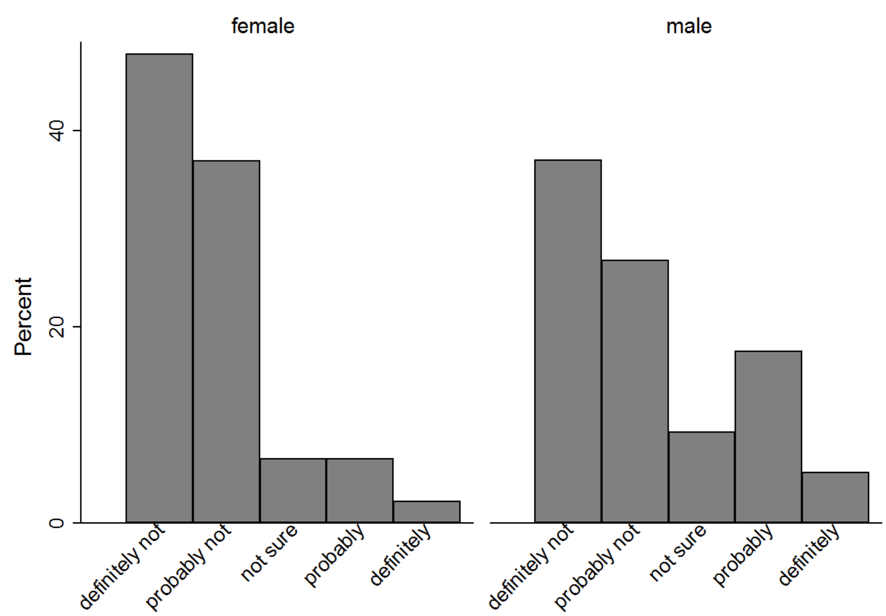

Notes: The sample has been limited to students who have been at the university for 2 or more years.

FIGURE 3. Does Athletic Success Affect the Gender Gap in GPAs?



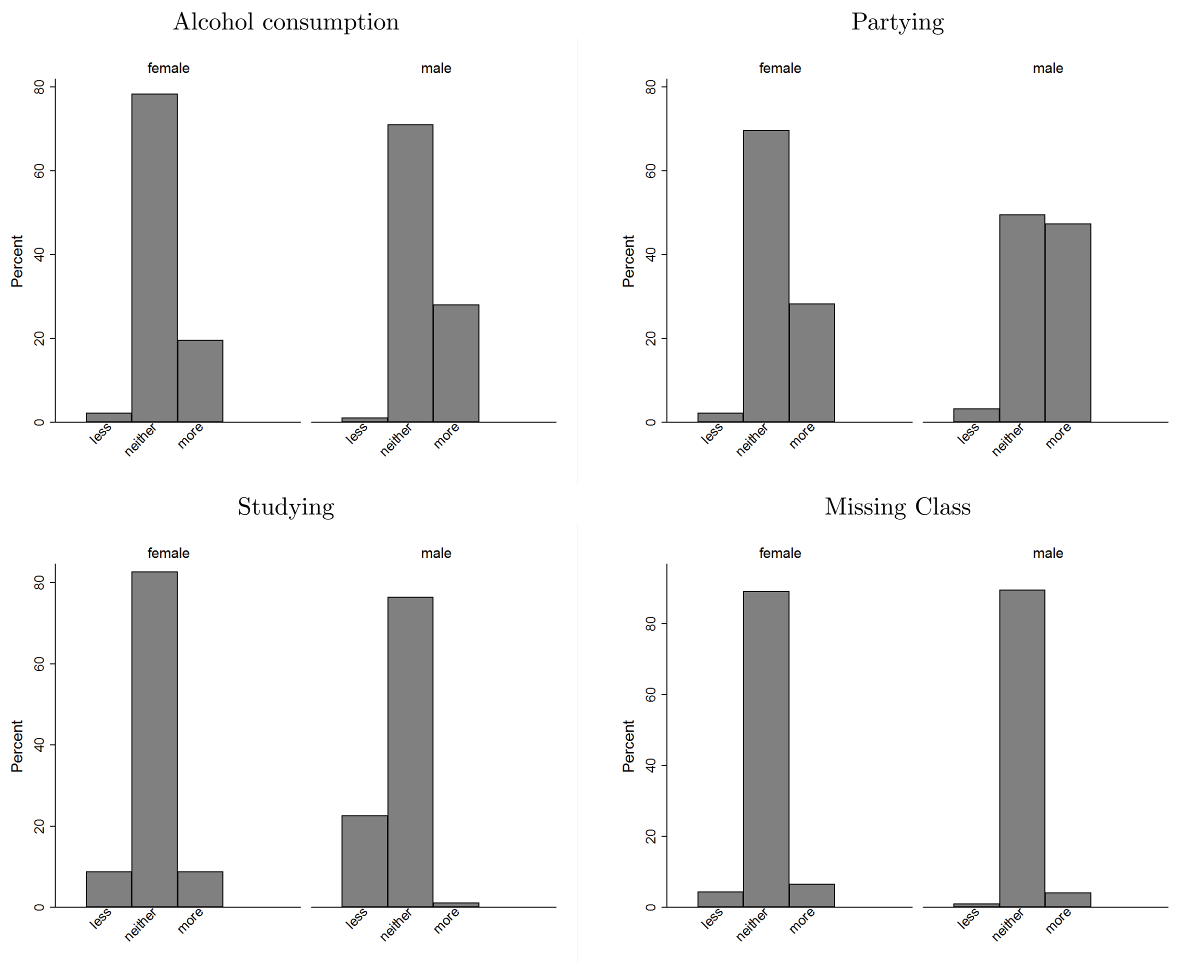

Notes: The sample is limited to fall term grades. The gender gap is defined as mean male GPA less mean female GPA, at the term level. Win percentage is the ratio of the University of Oregon football team's wins to total games played in a given season. A regression of the winning percentage on the gender gap (i.e., a model with only nine observations) yields an estimated coefficient of -0.069 with a standard error of 0.025.

FIGURE 4. Responses to the question: "Does the success of the University of Oregon football team decrease the amount of time you study for classes?"



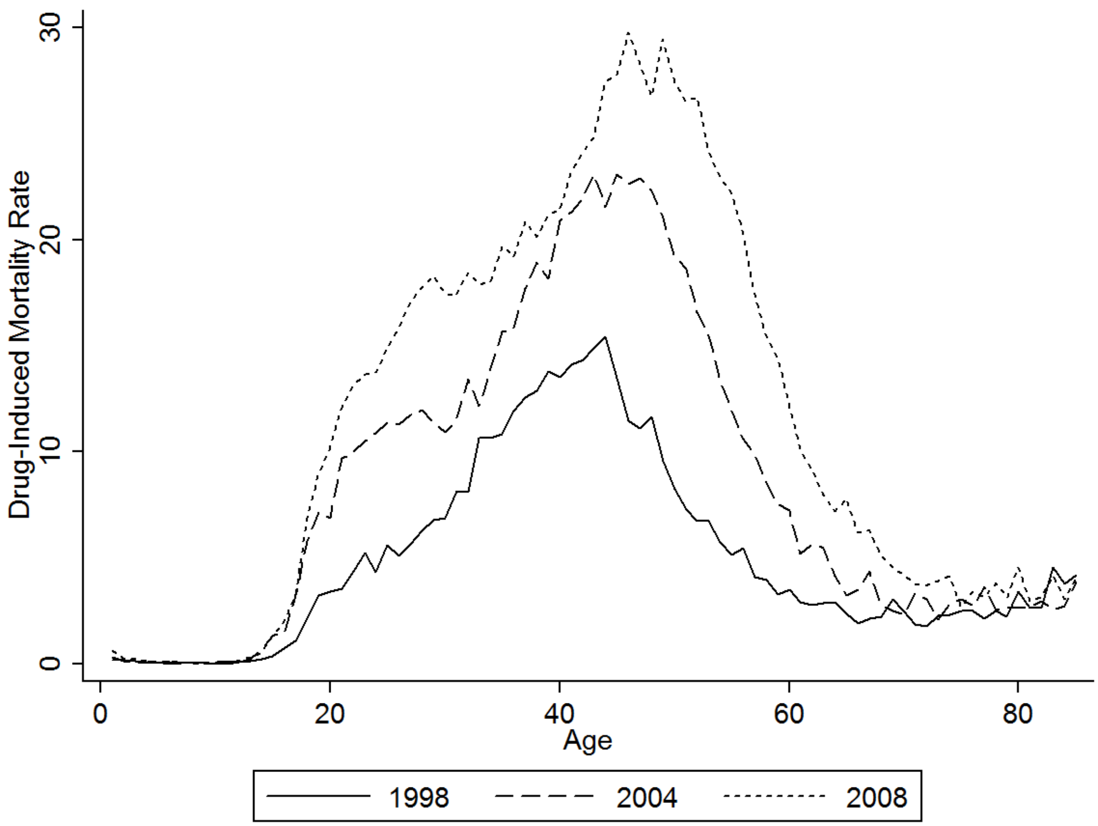

Notes: The sample has been limited to students who have been at the university for 2 or more years.

FIGURE 5. Responses to the question: “Compared to a loss, when the football team wins I tend to...”



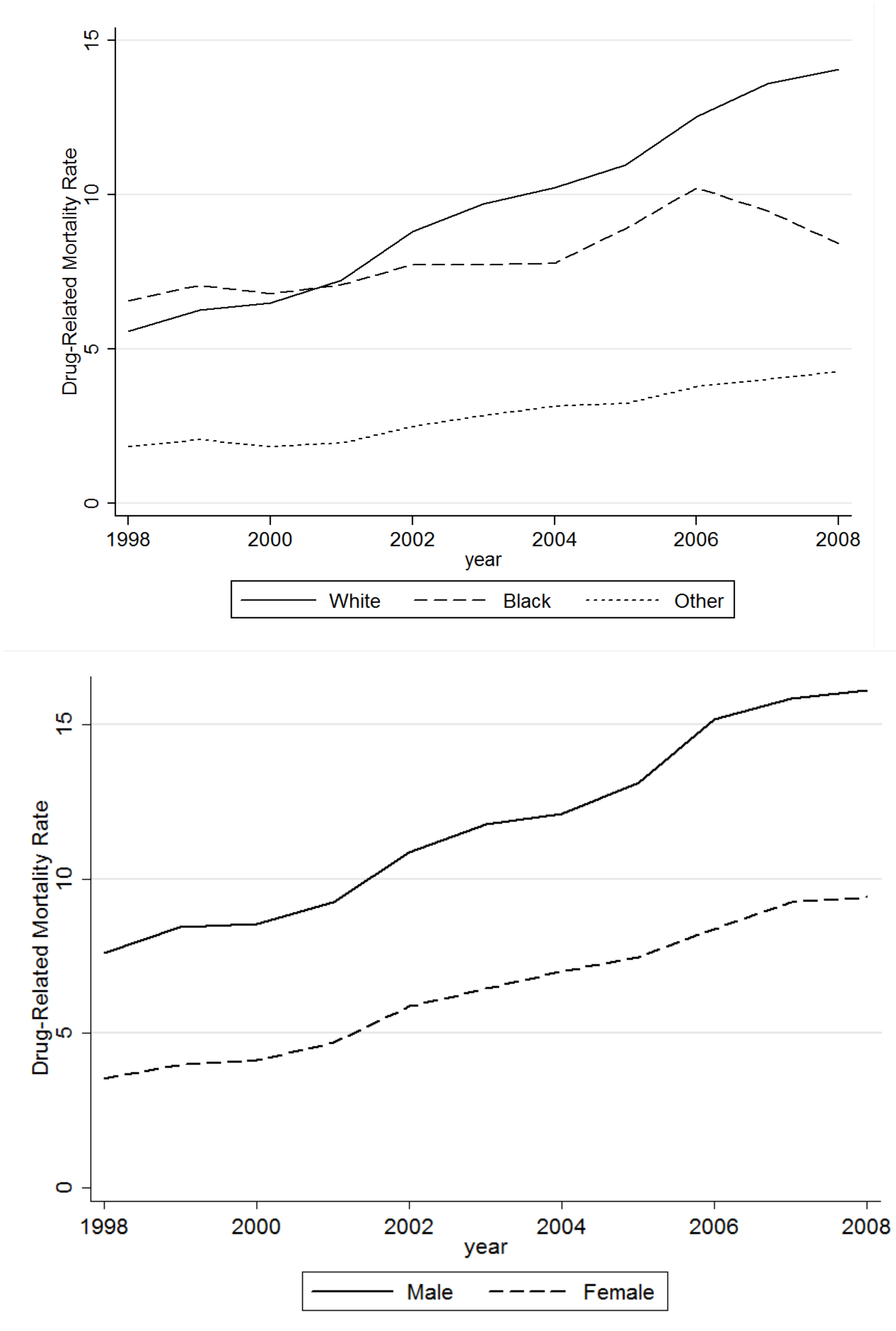

Notes: The sample has been limited to students who have been at the university for 2 or more years.

FIGURE 6. Age Profiles of Drug-Related Mortality



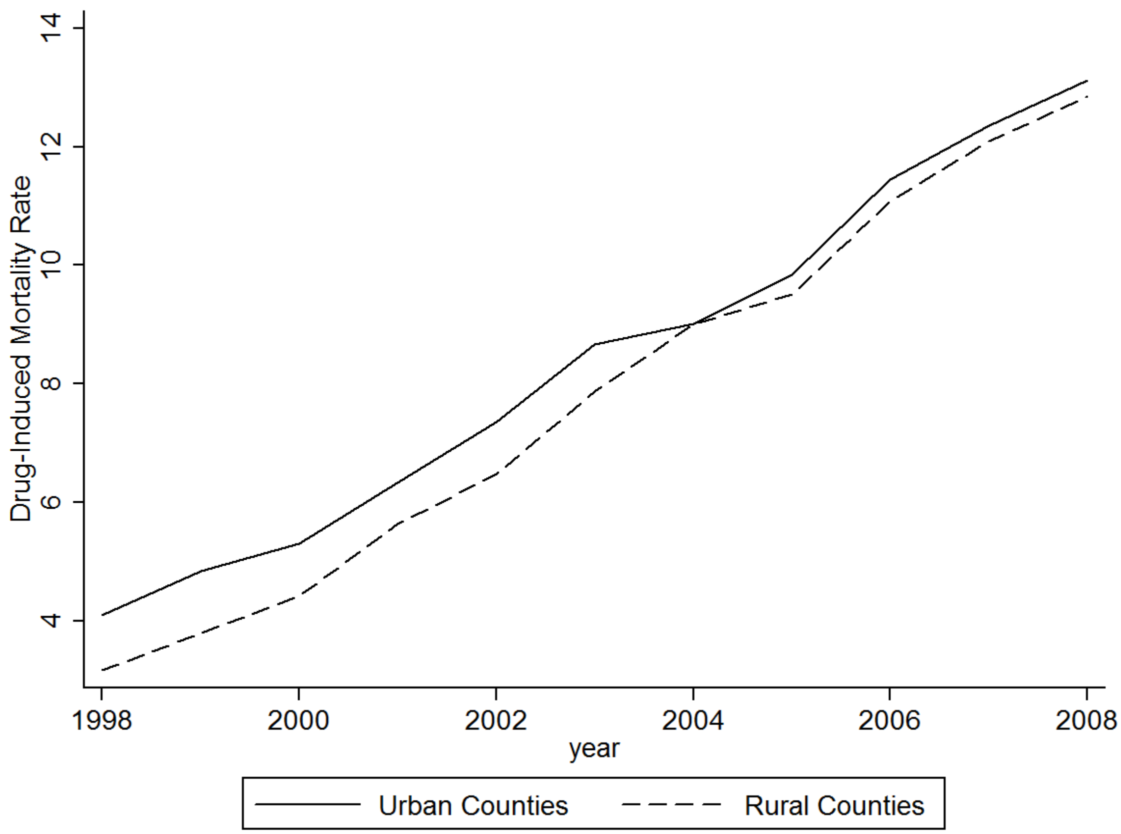

Notes: The drug-induced mortality rate is the number of drug poisonings per 100,000 residents.

FIGURE 7. Drug-Induced Mortality by Race and Gender



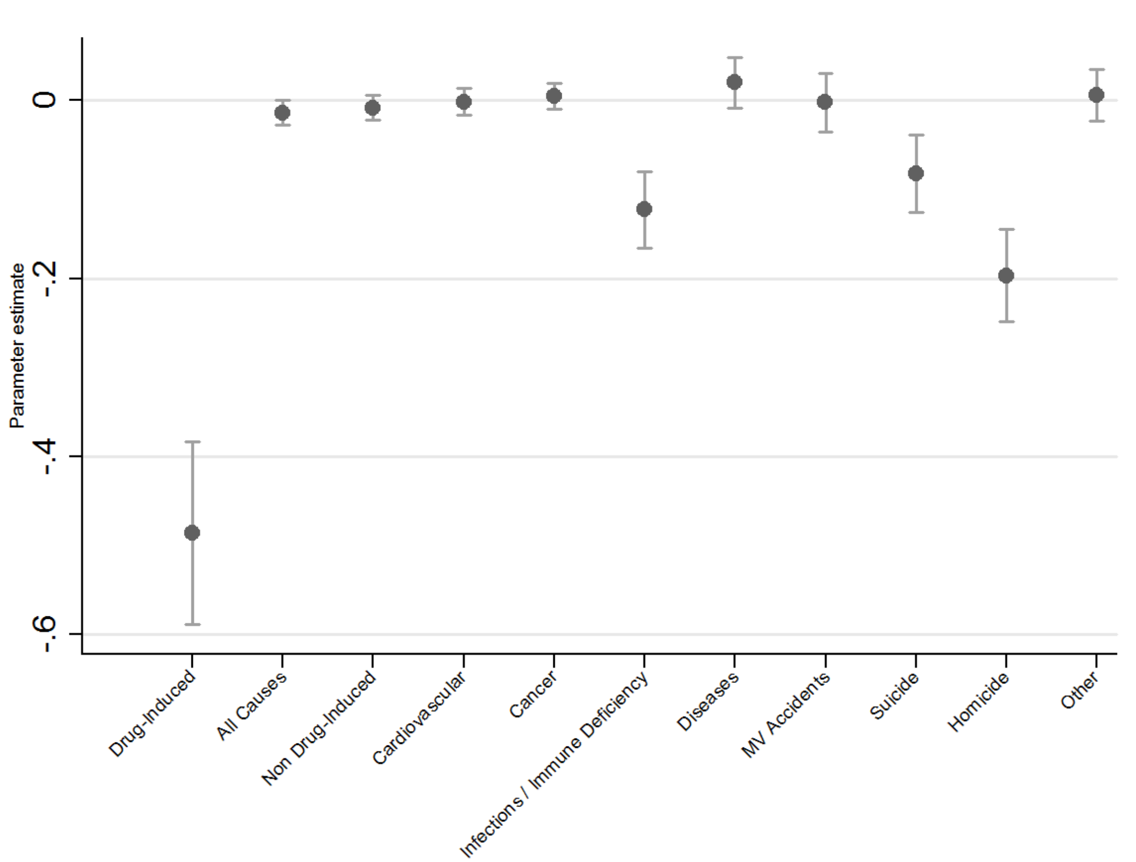

Notes: The drug-induced mortality rate is the number of drug poisonings per 100,000 residents.

FIGURE 8. Drug-Induced Mortality by Urban Classification



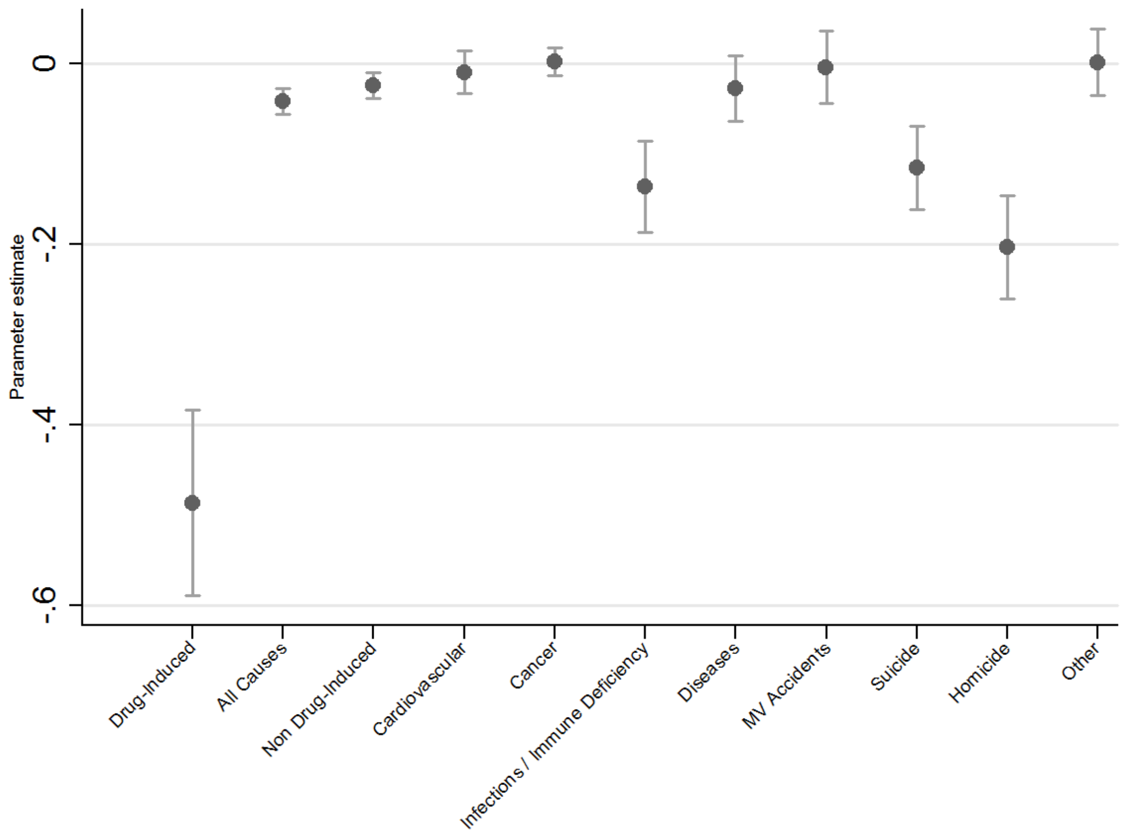

Notes: The drug-induced mortality rate is the number of drug poisonings per 100,000 residents. Urban and Rural categories are based on the National Center for Heath Statistics (NCHS) urban-rural classification scheme for counties. Counties within a metropolitan statistical area (MSA) are classified as urban while non-MSA counties are classified as rural.

FIGURE 9. Estimated Effects of Treatment Facilities on Log Mortality (All Ages)




Notes: The figure summarizes the results of regressions using logged drug-induced mortality rates across multiple causes of death as dependent variables. The figure plots the estimated coefficients and 95% confidence intervals associated with a change in the number of treatment facilities. Disease-related causes include respiratory, kidney and degenerative brain diseases. All estimates control for county-fixed effects, state-by-year fixed effects, county demographics demographic composition (fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old), county economic conditions (the unemployment rate, firm births, and per-capita income), and the number of law enforcement officers and total crimes per 100,000 residents. The estimates are weighted by county population and standard errors are corrected for possible clustering at the county level.

FIGURE 10. Estimated Effects of Treatment Facilities on Log Mortality (Ages less than 65)




Notes: The figure summarizes the results of regressions using logged drug-induced mortality rates among individuals less than age 65 across multiple causes of death as dependent variables. he figure plots the estimated coefficients and 95% confidence intervals associated with a change in the number of treatment facilities. Disease-related causes include respiratory, kidney and degenerative brain diseases. All estimates control for county-fixed effects, state-by-year fixed effects, county demographics demographic composition (fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old), county economic conditions (the unemployment rate, firm births, and per-capita income), and the number of law enforcement officers and total crimes per 100,000 residents. The estimates are weighted by county population and standard errors are corrected for possible clustering at the county level.

TABLE 1. Summary Statistics

| | Oregon (Sample) | Oregon (IPEDS) | Four-year Public U.S. Institutions (IPEDS) | NLSY97 (Sample) |
|---|---|---|---|---|
| SAT I Verbal 25th percentile score, incoming students | 500 | 490 | 464 | 470 |
| SAT I Verbal 75th percentile score, incoming students | 620 | 610 | 568 | 600 |
| SAT I Math 25th percentile score, incoming students | 500 | 500 | 472 | 470 |
| SAT I Math 75th percentile score, incoming students | 620 | 610 | 578 | 620 |
| Number of undergraduates | 13,102 | 15,983 | 8,674 | 2,298 |
| Fraction female | 0.55 | 0.53 | 0.55 | 0.54 |
| Fraction white | 0.79 | 0.75 | 0.67 | 0.81 |
| Fraction black | 0.02 | 0.02 | 0.11 | 0.11 |
| Fraction Hispanic | 0.03 | 0.03 | 0.08 | 0.07 |
| Fraction Asian | 0.08 | 0.12 | 0.11 | 0.00 |
| Total price for in-state students living on campus | | 14,734 | 13,272 | |
| Total price out-of-state students living on campus | | 26,170 | 20,022 | |
| Fraction receiving any financial aid | | 0.70 | 0.75 | |
| Fraction receiving federal-grant aid | | 0.18 | 0.34 | |
| Fraction receiving student-loan aid | | 0.40 | 0.45 | |

Notes: Data used in the first columns consists of University of Oregon undergraduates from 1998 through 2007. Financial aid statistics shown in the subsequent two columns are calculated using 2004 IPEDS data, while all other statistics in the same columns are calculated using 2003 IPEDS data. The number of institutions used to calculate the means in the third column range from 352 to 653. NLSY97 sample statistics use the last observed sampling weight for each respondent.

TABLE 2. RD-based Estimates of The Effect of Turning 21 (And Other Ages) At The End of Term on Grades

| Bandwidth | 240 days | 240 days | 210 days | 210 days | 180 days | 180 days | 150 days | 120 days | 100 days | 80 days | 80 days | 60 days | 40 days | 20 days |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Age Polynomial | Linear | Quadratic | Linear | Quadratic | Linear | Quadratic | Linear | Linear | Linear | Linear | None | None | None | None |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
| *Panel A: Estimated Effect of Turning 21* | | | | | | | | | | | | | | |
| Estimated effect | -0.035*** | -0.026 | -0.033*** | -0.030 | -0.036** | -0.024 | -0.031** | -0.024 | -0.038* | -0.037 | -0.038*** | -0.048*** | -0.031* | -0.027 |
| | (0.011) | (0.019) | (0.012) | (0.021) | (0.014) | (0.021) | (0.014) | (0.017) | (0.020) | (0.024) | (0.009) | (0.012) | (0.016) | (0.023) |
| Estimated effect including controls | -0.026** | -0.031* | -0.023** | -0.040** | -0.030** | -0.029 | -0.023 | -0.018 | -0.032 | -0.045* | -0.075*** | -0.080*** | -0.072*** | -0.047 |
| | (0.011) | (0.018) | (0.012) | (0.020) | (0.014) | (0.020) | (0.015) | (0.018) | (0.021) | (0.025) | (0.013) | (0.016) | (0.021) | (0.036) |
| Observations | 156,956 | 156,956 | 138,574 | 138,574 | 119,608 | 119,608 | 100,344 | 81,589 | 68,903 | 54,963 | 54,963 | 41,473 | 27,655 | 14,239 |
| *Panel B: Estimated Effect of Turning 20* | | | | | | | | | | | | | | |
| Estimated effect | -0.017 | -0.014 | -0.013 | -0.023 | -0.021 | -0.014 | -0.009 | -0.012 | -0.027 | -0.047** | -0.011 | -0.020* | -0.027* | -0.040* |
| | (0.010) | (0.018) | (0.011) | (0.019) | (0.014) | (0.019) | (0.014) | (0.015) | (0.019) | (0.023) | (0.009) | (0.012) | (0.016) | (0.021) |
| Estimated effect including controls | -0.009 | -0.010 | -0.007 | -0.014 | -0.013 | -0.003 | 0.008 | 0.006 | -0.011 | -0.036 | -0.027** | -0.029** | -0.032* | -0.035 |
| | (0.010) | (0.016) | (0.011) | (0.018) | (0.013) | (0.018) | (0.013) | (0.015) | (0.018) | (0.022) | (0.011) | (0.014) | (0.019) | (0.027) |
| Observations | 163,568 | 163,568 | 144,184 | 144,184 | 123,830 | 123,830 | 103,701 | 83,931 | 70,338 | 55,546 | 55,546 | 41,762 | 27,790 | 13,985 |
| *Panel C: Estimated Effect of Turning 22* | | | | | | | | | | | | | | |
| Estimated effect | -0.012 | -0.016 | -0.010 | -0.025 | -0.018 | -0.016 | -0.015 | -0.014 | -0.037 | -0.034 | -0.028** | -0.045*** | -0.039** | 0.002 |
| | (0.013) | (0.022) | (0.014) | (0.025) | (0.016) | (0.025) | (0.017) | (0.020) | (0.024) | (0.029) | (0.011) | (0.014) | (0.019) | (0.028) |
| Estimated effect including controls | -0.001 | -0.012 | 0.001 | -0.022 | -0.013 | -0.005 | -0.013 | -0.008 | -0.018 | -0.025 | -0.052*** | -0.053*** | -0.037 | 0.041 |
| | (0.014) | (0.023) | (0.015) | (0.025) | (0.017) | (0.027) | (0.019) | (0.022) | (0.027) | (0.033) | (0.016) | (0.020) | (0.028) | (0.049) |
| Observations | 114,397 | 114,397 | 102,009 | 102,009 | 88,277 | 88,277 | 74,321 | 60,664 | 51,108 | 40,810 | 40,810 | 30,747 | 20,670 | 10,444 |

Notes: The dependent variable is a student's normalized course grade. Controls include course-by-quarter-by-year fixed effects, birth-year fixed effects, accumulated-credits fixed effects, gender, math and verbal SAT scores, high-school GPA, and indicator variables for university athlete, private-school attendance, Black, Hispanic, and Asian. Standard errors (in parentheses) are corrected for clustering at the date-of-birth level.

* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 3. Fixed-Effects-Based Estimates of the Effect of Legal Access to Alcohol on Grades

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Age > 21 During Term | -0.146*** | -0.097*** | -0.033*** | -0.033*** |
| | (0.005) | (0.004) | (0.006) | (0.006) |
| Individual Fixed Effects | no | yes | yes | yes |
| Accumulated-Credits Fixed Effects | no | no | yes | yes |
| Course-Specific Controls | no | no | no | yes |
| Number of Students | 13,102 | 13,102 | 13,102 | 13,102 |
| Observations | 479,342 | 479,342 | 479,342 | 479,342 |

Notes: The dependent variable is equal to the student's normalized course grade. Accumulated-credits fixed effects are fixed effects for a student's accumulated credits at the beginning of a term. Course-specific controls include subject-by-level fixed effects and term fixed effects. Standard errors (in parentheses) are corrected for clustering at the individual level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 4. Fixed-Effects-Based Estimates of the Effect of Legal Access to Alcohol on the Grade Distribution, Course Difficulty, and Course Load

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | A Grade | B Grade | C Grade | D or F Grade | Expected Term GPA | Course Load |
| Age > 21 | -0.008*** | 0.000 | 0.007*** | 0.000 | 0.009*** | 0.053 |
| | (0.003) | (0.003) | (0.002) | (0.001) | (0.003) | (0.035) |
| Individual Fixed Effects | yes | yes | yes | yes | yes | yes |
| Accumulated-Credits Fixed Effects | yes | yes | yes | yes | yes | yes |
| Course-Specific Controls | yes | yes | yes | yes | - | - |
| Number of Students | 13,102 | 13,102 | 13,102 | 13,102 | 13,102 | 13,102 |
| Observations | 479,342 | 479,342 | 479,342 | 479,342 | 146,730 | 146,730 |

Notes: The analysis in columns 1–4 is based on data at the at the student-by-course level whereas the analysis in columns 5–6 is based on data at the student-by-term level (and thus does not control for course characteristics). The outcome variable for Column 5, a student's expected term GPA, is calculated based on the average grades in the previous offering of each course a student is taking in a given term. The outcome variable for Column 6, course load, is the number of credits taken in a term. Accumulated-credits fixed effects are fixed effects for a student's accumulated credits (in four-credit intervals) at the beginning of a term. Standard errors (in parentheses) are corrected for clustering at the individual level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 5. Fixed-Effects-Based Estimates of the Dynamic Effects of Legal Access to Alcohol on Grades

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Turns 21 in 4 terms | | | | | 0.002 |
| | | | | | (0.006) |
| Turns 21 in 3 terms | | | | 0.008 | 0.009 |
| | | | | (0.006) | (0.008) |
| Turns 21 in 2 terms | | | 0.004 | 0.008 | 0.009 |
| | | | (0.006) | (0.007) | (0.009) |
| Turns 21 in 1 term | | 0.002 | 0.003 | 0.008 | 0.009 |
| | | (0.007) | (0.007) | (0.009) | (0.010) |
| Term of 21st birthday | -0.036*** | -0.035*** | -0.033*** | -0.028*** | -0.027** |
| | (0.006) | (0.008) | (0.009) | (0.010) | (0.012) |
| Turned 21 1 term ago | -0.030*** | -0.029*** | -0.027** | -0.021* | -0.020 |
| | (0.008) | (0.009) | (0.011) | (0.012) | (0.014) |
| Turned 21 2 terms ago | -0.026*** | -0.025** | -0.023* | -0.017 | -0.015 |
| | (0.009) | (0.011) | (0.012) | (0.014) | (0.016) |
| Turned 21 3 terms ago | -0.031*** | -0.030** | -0.027** | -0.021 | -0.020 |
| | (0.011) | (0.013) | (0.014) | (0.015) | (0.017) |
| Turned 21 4 terms ago | -0.055*** | -0.054*** | -0.051*** | -0.044*** | -0.043** |
| | (0.012) | (0.014) | (0.015) | (0.017) | (0.019) |
| Turned 21 5 terms ago | -0.038*** | -0.037** | -0.035** | -0.027 | -0.026 |
| | (0.014) | (0.016) | (0.017) | (0.019) | (0.021) |
| Turned 21 6+ terms ago | -0.021 | -0.020 | -0.017 | -0.010 | -0.008 |
| | (0.017) | (0.019) | (0.020) | (0.022) | (0.023) |
| Individual Fixed Effects | yes | yes | yes | yes | yes |
| Accumulated-Credits Fixed Effects | yes | yes | yes | yes | yes |
| Course-Specific Controls | yes | yes | yes | yes | yes |
| Number of Students | 13,102 | 13,102 | 13,102 | 13,102 | 13,102 |
| Observations | 479,342 | 479,342 | 479,342 | 479,342 | 479,342 |

Notes: The dependent variable is equal to the student's normalized course grade. Accumulated-credits fixed effects are fixed effects for a student's accumulated credits at the beginning of a term. Course-specific controls include subject-by-level fixed effects and term fixed effects. Standard errors (in parentheses) are corrected for clustering at the individual level.

* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 6. Heterogeneity Across Gender, Ability, and Financial-Aid Eligibility

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| *Panel A: Gender and Ability* | Male | Female | High Ability | Low Ability | Male High Ability | Male Low Ability | Female High Ability | Female Low Ability |
| Age > 21 During Term | -0.024** | -0.045*** | -0.021** | -0.046*** | -0.006 | -0.047*** | -0.039*** | -0.051*** |
| | (0.010) | (0.008) | (0.009) | (0.009) | (0.013) | (0.014) | (0.012) | (0.011) |
| Individual Fixed Effects | yes | yes | yes | yes | yes | yes | yes | yes |
| Accumulated-Credits Fixed Effects | yes | yes | yes | yes | yes | yes | yes | yes |
| Course-Specific Controls | yes | yes | yes | yes | yes | yes | yes | yes |
| Number of Students | 5,903 | 7,199 | 6,332 | 6,770 | 3,221 | 2,682 | 3,111 | 4,088 |
| Observations | 218,479 | 260,863 | 234,099 | 245,243 | 119,946 | 98,533 | 114,153 | 146,710 |
| *Panel B: Gender and Financial Aid* | Male | Female | High Eligibility | Low Eligibility | Male High Eligibility | Male Low Eligibility | Female High Eligibility | Female Low Eligibility |
| Age > 21 During Term | -0.026** | -0.058*** | -0.051*** | -0.040*** | -0.045*** | -0.015 | -0.057*** | -0.063*** |
| | (0.011) | (0.009) | (0.010) | (0.010) | (0.017) | (0.015) | (0.013) | (0.013) |
| Individual Fixed Effects | yes | yes | yes | yes | yes | yes | yes | yes |
| Accumulated-Credits Fixed Effects | yes | yes | yes | yes | yes | yes | yes | yes |
| Course-Specific Controls | yes | yes | yes | yes | yes | yes | yes | yes |
| Number of Students | 3,900 | 5,213 | 4,556 | 4,557 | 1,887 | 2,013 | 2,669 | 2,544 |
| Observations | 145,471 | 190,444 | 166,504 | 169,411 | 69,764 | 75,707 | 96,740 | 93,704 |

Notes: The dependent variable is equal to the student's normalized course grade. Accumulated-credits fixed effects are fixed effects for a student's cumulative credits at the beginning of a term. Course-specific controls include subject-by-level fixed effects and term fixed effects. Standard errors (in parentheses) are corrected for clustering at the individual level. The high-ability group consists of students with SAT scores above the sample median (1120) while the low-ability group consists of those with SAT scores at or below the sample median.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 7. Estimated Effects on Drinking Behaviors During The Previous 30 Days Using NLSY97 Data

| | (1) Full Sample | (2) Male | (3) Female | (4) High Ability | (5) Low Ability | (6) Male High Ability | (7) Male Low Ability | (8) Female High Ability | (9) Female Low Ability |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: Drank* | | | | | | | | | |
| Age > 21 | 0.065*** | 0.051** | 0.077*** | 0.088*** | 0.054*** | 0.084** | 0.026 | 0.091** | 0.072*** |
| | (0.014) | (0.021) | (0.017) | (0.024) | (0.017) | (0.033) | (0.031) | (0.036) | (0.021) |
| Students | 2,298 | 997 | 1,301 | 693 | 1,419 | 348 | 522 | 313 | 818 |
| Observations | 9,023 | 3,943 | 5,080 | 2,789 | 5,521 | 1,422 | 2,029 | 1,253 | 3,203 |
| Pre-21 Mean | 0.66 | 0.67 | 0.65 | 0.66 | 0.66 | 0.68 | 0.66 | 0.66 | 0.65 |
| *Panel B: Days Drank* | | | | | | | | | |
| Age > 21 | 1.394*** | 1.643*** | 1.173*** | 1.727*** | 1.265*** | 1.597*** | 1.695*** | 2.027*** | 0.976*** |
| | (0.184) | (0.300) | (0.227) | (0.293) | (0.255) | (0.443) | (0.471) | (0.401) | (0.304) |
| Students | 2,298 | 997 | 1,301 | 693 | 1,419 | 348 | 522 | 313 | 818 |
| Observations | 9,023 | 3,943 | 5,080 | 2,789 | 5,521 | 1,422 | 2,029 | 1,253 | 3,203 |
| Pre-21 Mean | 4.27 | 4.80 | 3.83 | 3.95 | 4.42 | 4.42 | 5.04 | 3.51 | 3.97 |
| *Panel C: Drinks When Drank* | | | | | | | | | |
| Age > 21 | -0.190* | -0.234 | -0.155 | -0.189 | -0.147 | -0.384 | -0.026 | 0.034 | -0.243 |
| | (0.111) | (0.178) | (0.139) | (0.169) | (0.153) | (0.255) | (0.256) | (0.227) | (0.201) |
| Students | 1,905 | 841 | 1,064 | 607 | 1,147 | 308 | 427 | 271 | 654 |
| Observations | 6,163 | 2,745 | 3,418 | 2,023 | 3,662 | 1,045 | 1,360 | 908 | 2,100 |
| Pre-21 Mean | 4.54 | 5.35 | 3.86 | 4.31 | 4.67 | 4.91 | 5.60 | 3.61 | 4.03 |
| *Panel D: Days Drank 5+ Drinks* | | | | | | | | | |
| Age > 21 | 0.409*** | 0.520*** | 0.302** | 0.377** | 0.440*** | 0.350 | 0.403 | 0.478* | 0.319 |
| | (0.118) | (0.186) | (0.151) | (0.187) | (0.165) | (0.278) | (0.274) | (0.268) | (0.211) |
| Students | 2,298 | 997 | 1,301 | 693 | 1,419 | 348 | 522 | 313 | 818 |
| Observations | 9,023 | 3,943 | 5,080 | 2,789 | 5,521 | 1,422 | 2,029 | 1,253 | 3,203 |
| Pre-21 Mean | 2.11 | 2.71 | 1.61 | 1.97 | 2.19 | 2.39 | 2.99 | 1.54 | 1.66 |

The sample is restricted to individuals currently enrolled in a four-year college between 1998 and 2009. All regressions are weighted using sampling weights and control for individual fixed effects and a quadratic in age. Standard errors (in parentheses) are corrected for clustering at the individual level. Consistent with the definitions used in our analysis of University of Oregon transcript data, the high-ability group consists of students with actual (or ASVAB-predicted) SAT scores above 1120 while the low-ability group consists of those with scores at or below the 1120.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 8. Summary Statistics

| | All Students | Male | Female |
|---|---|---|---|
| Grade Point Average (GPA) | 3.04 | 2.94 | 3.12 |
| 1st Year GPA | 3.00 | 2.90 | 3.07 |
| 2nd Year GPA | 3.01 | 2.92 | 3.08 |
| 3rd Year GPA | 3.08 | 3.00 | 3.15 |
| 4th Year GPA | 3.14 | 3.02 | 3.24 |
| 5+ Year GPA | 2.98 | 2.90 | 3.10 |
| High-School GPA | 3.49 | 3.40 | 3.56 |
| SAT | 1121 | 1134 | 1095 |
| White | 0.80 | - | - |
| Asian | 0.08 | - | - |
| Black | 0.02 | - | - |
| Hispanic | 0.04 | - | - |
| Winning Percentage | 0.70 | - | - |
| Number of Undergraduates | 29,737 | 13,184 | 16,553 |
| Number of Student-Class Observations | 267,322 | 119,191 | 148,131 |

Notes: Sample data consist of non-athlete University of Oregon undergraduates from 1999 through 2007. Winning percentage is the ratio of the University of Oregon football team's wins to total games played in a given season.

TABLE 9. Estimated Effect of Athletic Success on Male and Female Grades

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Panel A: Males** | | | | | | |
| Winning Percentage | -0.142*** | -0.064*** | -0.061*** | -0.039** | -0.040** | -0.069*** |
| | (0.023) | (0.022) | (0.021) | (0.020) | (0.019) | (0.019) |
| Time Trend | no | yes | yes | yes | yes | no |
| Student Controls | no | no | yes | - | - | - |
| Student Fixed Effects | no | no | no | yes | yes | yes |
| Subject-by-Level Fixed Effects | no | no | no | no | yes | yes |
| Accumulated-Credits Fixed Effects | no | no | no | no | no | yes |
| Number of Unique Students | 13,184 | 13,184 | 13,184 | 13,184 | 13,184 | 13,184 |
| Student-Class Observations | 119,191 | 119,191 | 119,191 | 119,191 | 119,191 | 119,191 |
| **Panel B: Females** | | | | | | |
| Winning Percentage | -0.073*** | 0.002 | 0.000 | 0.034** | 0.026* | 0.005 |
| | (0.019) | (0.018) | (0.017) | (0.016) | (0.015) | (0.015) |
| Time Trend | no | yes | yes | yes | yes | no |
| Student Controls | no | no | yes | - | - | - |
| Student Fixed Effects | no | no | no | yes | yes | yes |
| Subject-by-Level Fixed Effects | no | no | no | no | yes | yes |
| Accumulated-Credits Fixed Effects | no | no | no | no | no | yes |
| Number of Unique Students | 16,553 | 16,553 | 16,553 | 16,553 | 16,553 | 16,553 |
| Student-Class Observations | 148,131 | 148,131 | 148,131 | 148,131 | 148,131 | 148,131 |
| **Panel C: Pooled Sample** | | | | | | |
| Winning Percentage | -0.073*** | 0.004 | 0.003 | 0.025 | 0.024 | -0.001 |
| | (0.019) | (0.019) | (0.017) | (0.016) | (0.015) | (0.015) |
| Male × Winning Percentage | -0.069** | -0.072** | -0.067** | -0.052** | -0.061** | -0.062*** |
| | (0.030) | (0.030) | (0.027) | (0.026) | (0.024) | (0.024) |
| Male | -0.129*** | -0.128*** | -0.077*** | | | |
| | (0.022) | (0.022) | (0.020) | | | |
| Time Trend | no | yes | yes | yes | yes | no |
| Student Controls | no | no | yes | - | - | - |
| Student Fixed Effects | no | no | no | yes | yes | yes |
| Subject-by-Level Fixed Effects | no | no | no | no | yes | yes |
| Accumulated-Credits Fixed Effects | no | no | no | no | no | yes |
| Number of Unique Students | 29,737 | 29,737 | 29,737 | 29,737 | 29,737 | 29,737 |
| Student-Class Observations | 267,322 | 267,322 | 267,322 | 267,322 | 267,322 | 267,322 |

Notes: Panels A and B provide the estimates for male and female students respectively. The dependent variable is the grade points received by a student in a given class, ranging from 0 to 4.3. Student controls include math and verbal SAT scores, high-school GPA, age, and indicator variables for, private-school attendance, Black, Hispanic, and Asian. The sample has been limited to fall-term grades. Winning percentage is the ratio of the University of Oregon football team's wins to total games played in a given season. Standard errors (in parentheses) are corrected for clustering at the student level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 10. Estimated Effects Across Letter Grade Assignments

| Outcome: | A (1) | B (2) | C (3) | Fail (4) |
|---|---|---|---|---|
| **Panel A: Controlling for time** | | | | |
| Winning Percentage | -0.003 | 0.017* | -0.009 | -0.005 |
| | (0.009) | (0.010) | (0.007) | (0.004) |
| Male × Winning Percentage | -0.008 | -0.015 | 0.008 | 0.015** |
| | (0.013) | (0.014) | (0.011) | (0.007) |
| Number of Unique Students | 29,737 | 29,737 | 29,737 | 29,737 |
| Student-Class Observations | 267,322 | 267,322 | 267,322 | 267,322 |
| **Panel B: Controlling for accumulated credits** | | | | |
| Winning Percentage | -0.007 | 0.013 | -0.007 | 0.001 |
| | (0.009) | (0.010) | (0.007) | (0.004) |
| Male × Winning Percentage | -0.010 | -0.013 | 0.009 | 0.014** |
| | (0.013) | (0.014) | (0.011) | (0.007) |
| Number of Unique Students | 29,737 | 29,737 | 29,737 | 29,737 |
| Student-Class Observations | 267,322 | 267,322 | 267,322 | 267,322 |

Notes: All estimates control for student fixed effects and subject-by-level fixed effects. Panel A additionally controls for a quadratic in time while Panel B additionally controls for a student's accumulated credits at the beginning of the term. The dependent variables are indicator variables for letter grade assignments corresponding to each column letter. The sample has been limited to fall term grades. Winning percentage is the ratio of the University of Oregon football team's wins to total games played in a given season. Standard errors (in parentheses) are corrected for clustering at the student level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 11. Estimated Effects on GPAs, By Term

| | Fall (1) | Winter (2) | Spring (3) |
|---|---|---|---|
| **Panel A: Controlling for time** | | | |
| Winning Percentage | 0.024 | 0.002 | 0.004 |
| | (0.015) | (0.015) | (0.016) |
| Male × Winning Percentage | -0.061** | -0.023 | -0.018 |
| | (0.024) | (0.024) | (0.025) |
| Number of Unique Students | 29,737 | 29,490 | 25,298 |
| Student-Class Observations | 267,322 | 271,489 | 207,837 |
| **Panel B: Controlling for accumulated credits** | | | |
| Winning Percentage | -0.001 | -0.021 | -0.017 |
| | (0.015) | (0.015) | (0.016) |
| Male × Winning Percentage | -0.062*** | -0.020 | -0.016 |
| | (0.024) | (0.024) | (0.025) |
| Number of Unique Students | 29,737 | 29,490 | 25,298 |
| Student-Class Observations | 267,322 | 271,489 | 207,837 |

Notes: All estimates control for student fixed effects and subject-by-level fixed effects. Panel A additionally controls for a quadratic in time while Panel B additionally controls for a student's accumulated credits at the beginning of the term. The dependent variable is the grade points received by a student in a given class, ranging from 0 to 4.3. Placebo quarters include the Winter and Spring quarters during which the football team does not play any games. Winning percentage is the ratio of the University of Oregon football team's wins to total games played in a given season. Standard errors (in parentheses) are corrected for clustering at the student level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 12. Estimated Effects on GPAs, By Ability and Financial Need

| | SAT | | | | Financial Need | | | |
|---|---|---|---|---|---|---|---|---|
| | All | Low | Med | High | All | Low | Med | High |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Panel A: Controlling for time** | | | | | | | | |
| Winning Percentage | 0.024 | 0.057** | -0.018 | 0.030 | 0.035 | 0.010 | 0.039 | 0.054* |
| | (0.015) | (0.028) | (0.026) | (0.024) | (0.018) | (0.029) | (0.030) | (0.032) |
| Male × Winning Percentage | -0.061** | -0.123*** | -0.053 | -0.036 | -0.076*** | -0.043 | -0.078 | -0.109** |
| | (0.024) | (0.046) | (0.042) | (0.037) | (0.029) | (0.047) | (0.048) | (0.054) |
| Number of Unique Students | 29,737 | 9,755 | 9,554 | 10,428 | 20,668 | 6,927 | 6,840 | 6,901 |
| Student-Class Observations | 267,322 | 80,868 | 86,367 | 100,087 | 186,969 | 64,635 | 63,169 | 59,165 |
| **Panel B: Controlling for accumulated credits** | | | | | | | | |
| Winning Percentage | -0.001 | 0.031 | -0.040 | 0.006 | 0.013 | -0.001 | 0.002 | 0.035 |
| | (0.015) | (0.028) | (0.026) | (0.024) | (0.018) | (0.029) | (0.030) | (0.032) |
| Male × Winning Percentage | -0.062*** | -0.130*** | -0.050 | -0.040 | -0.077*** | -0.043 | -0.081* | -0.106** |
| | (0.024) | (0.046) | (0.042) | (0.037) | (0.028) | (0.047) | (0.048) | (0.054) |
| Number of Unique Students | 29,737 | 9,755 | 9,554 | 10,428 | 20,668 | 6,927 | 6,840 | 6,901 |
| Student-Class Observations | 267,322 | 80,868 | 86,367 | 100,087 | 186,969 | 64,635 | 63,169 | 59,165 |

Notes: All estimates control for student fixed effects and subject-by-level fixed effects. Panel A additionally controls for a quadratic in time while Panel B additionally controls for a student's accumulated credits at the beginning of the term. The dependent variable is the grade points received by a student in a given class, ranging from 0 to 4.3. The SAT groups (columns 2-4) are defined by student SAT score terciles. The financial-need groups (columns 6-8) are defined by student eligibility terciles. Standard errors (in parentheses) are corrected for clustering at the student level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 13. Estimated Effects on GPAs, By Race

| | All (1) | White (2) | Non-White (3) | Black (4) | Hispanic (5) | Asian (6) |
|---|---|---|---|---|---|---|
| **Panel A: Controlling for time** | | | | | | |
| Winning Percentage | 0.024 | 0.011 | 0.128*** | 0.166 | 0.003 | 0.150*** |
| | (0.015) | (0.016) | (0.044) | (0.155) | (0.087) | (0.053) |
| Male × Winning Percentage | -0.061** | -0.047* | -0.170** | -0.348 | -0.155 | -0.114 |
| | (0.024) | (0.025) | (0.069) | (0.222) | (0.143) | (0.083) |
| Number of Unique Students | 29,737 | 25,844 | 3,893 | 482 | 1,034 | 2,383 |
| Student-Class Observations | 267,322 | 232,469 | 34,853 | 3,968 | 8,801 | 22,124 |
| **Panel B: Controlling for accumulated credits** | | | | | | |
| Winning Percentage | -0.001 | -0.012 | 0.087** | 0.093 | -0.046 | 0.113** |
| | (0.015) | (0.016) | (0.044) | (0.154) | (0.086) | (0.054) |
| Male × Winning Percentage | -0.062*** | -0.048* | -0.165** | -0.317 | -0.152 | -0.113 |
| | (0.024) | (0.025) | (0.069) | (0.228) | (0.144) | (0.083) |
| Number of Unique Students | 29,737 | 25,844 | 3,893 | 482 | 1,034 | 2,383 |
| Student-Class Observations | 267,322 | 232,469 | 34,853 | 3,968 | 8,801 | 22,124 |

Notes: All estimates control for student fixed effects and subject-by-level fixed effects. Panel A additionally controls for a quadratic in time while Panel B additionally controls for a student's accumulated credits at the beginning of the term. The dependent variable is the grade points received by a student in a given class, ranging from 0 to 4.3. Standard errors (in parentheses) are corrected for clustering at the student level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 14. Estimated Effects on Dropping Out, By Ability and Financial Need

| | SAT | | | | Financial Need | | | |
|---|---|---|---|---|---|---|---|---|
| | All | Low | Med | High | All | Low | Med | High |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Winning Percentage | -0.014 | -0.051*** | 0.011 | 0.004 | -0.015 | -0.003 | -0.008 | -0.034* |
| | (0.009) | (0.018) | (0.016) | (0.015) | (0.011) | (0.018) | (0.019) | (0.020) |
| Male $\times$ Winning Percentage | 0.004 | 0.063** | -0.023 | -0.024 | 0.001 | -0.000 | 0.002 | 0.006 |
| | (0.013) | (0.027) | (0.022) | (0.020) | (0.016) | (0.026) | (0.026) | (0.030) |
| Male | -0.010 | -0.047** | 0.005 | 0.010 | -0.007 | -0.006 | -0.013 | -0.003 |
| | (0.009) | (0.020) | (0.016) | (0.014) | (0.011) | (0.018) | (0.019) | (0.022) |
| Time Trend | yes | yes | yes | yes | yes | yes | yes | yes |
| Student Controls | yes | yes | yes | yes | yes | yes | yes | yes |
| Accumulated Credits Fixed Effects | yes | yes | yes | yes | yes | yes | yes | yes |
| Number of Unique Students | 26,802 | 8,702 | 9,171 | 8,929 | 18,627 | 6,208 | 6,209 | 6,210 |
| Student-Class Observations | 71,372 | 22,384 | 24,476 | 24,512 | 49,537 | 16,643 | 17,005 | 15,889 |

Notes: The dependent variable is an indicator for not being observed in the next fall quarter or any subsequent quarter and not having graduated. The dependent variable is undefined for the last year of data, resulting in fewer observations than our previous analysis. Student controls include math and verbal SAT scores, high-school GPA, age, and indicator variables for, private-school attendance, Black, Hispanic, and Asian. The SAT groups (columns 1-3) are defined by student SAT score terciles. The financial need groups (columns 4-6) are defined by student eligibility terciles. Standard errors (in parentheses) are corrected for clustering at the student level.

* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 15. Estimated Effects on Dropping Out, By Race

| | All<br>(1) | White<br>(2) | Non-White<br>(3) | Black<br>(4) | Hispanic<br>(5) | Asian<br>(6) |
|---|---|---|---|---|---|---|
| Winning Percentage | -0.014 | -0.011 | -0.036 | -0.138* | -0.046 | -0.002 |
| | (0.009) | (0.010) | (0.025) | (0.080) | (0.051) | (0.030) |
| Male $\times$ Winning Percentage | 0.004 | 0.000 | 0.034 | 0.225* | 0.008 | -0.011 |
| | (0.013) | (0.014) | (0.037) | (0.129) | (0.075) | (0.045) |
| Male | -0.010 | -0.010 | -0.009 | -0.113 | -0.006 | 0.021 |
| | (0.009) | (0.010) | (0.026) | (0.094) | (0.054) | (0.032) |
| Time Trend | yes | yes | yes | yes | yes | yes |
| Student Controls | yes | yes | yes | yes | yes | yes |
| Accumulated Credits Fixed Effects | yes | yes | yes | yes | yes | yes |
| Number of Unique Students | 26,802 | 23,338 | 3,464 | 431 | 911 | 2,126 |
| Student-Class Observations | 71,372 | 62,048 | 9,324 | 1,100 | 2,363 | 5,870 |

Notes: The dependent variable is an indicator for not being observed in the next fall quarter or any subsequent quarter and not having graduated. The dependent variable is undefined for the last year of data, resulting in fewer observations than our previous analysis. Student controls include math and verbal SAT scores, high-school GPA, age, and indicator variables for, private-school attendance, Black, Hispanic, and Asian. The SAT groups (columns 1-3) are defined by student SAT score terciles. Standard errors (in parentheses) are corrected for clustering at the student level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 16. Summary Statistics

| | (1)<br>(Drug-Induced Mortality) |
|---|---|
| **Substance-Abuse Treatment Facilities** | |
| Facilities | 48.68 |
| Facilities (per 100,000) | 4.90 |
| **Drug-Induced Mortality (per 100,000)** | |
| All | 9.39 |
| Female | 6.67 |
| Male | 12.23 |
| White | 10.17 |
| Black | 8.02 |
| Other | 2.96 |
| Ages 0-15 | 0.18 |
| Ages 15-39 | 11.05 |
| Ages 40-64 | 16.36 |
| Ages 65+ | 3.29 |
| Counties | 2,409 |
| County-Year Observations | 24,062 |

Notes: All statistics are weighted by county population. The sample is limited to U.S. counties within the 48 contiguous states with consistent county identifiers and at least one substance-abuse treatment facility over the sample time frame (1998-2008).

TABLE 17. Estimated Effects of Treatment Facilities on Drug-Induced Mortality

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Number of Facilities | -0.036*** | -0.037*** | -0.037*** | -0.038*** | -0.038*** |
| | (0.006) | (0.005) | (0.005) | (0.005) | (0.005) |
| County and Year FE | yes | yes | yes | yes | yes |
| State by Year FE | no | yes | yes | yes | yes |
| Demographic Controls | no | no | yes | yes | yes |
| Controls for Economic Conditions | no | no | no | yes | yes |
| Controls for Law Enforcement/Crime | no | no | no | no | yes |
| Counties | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 |
| County-Year Observations | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 |
| Mean | 9.390 | 9.390 | 9.390 | 9.390 | 9.390 |
| % Impact | -0.386 | -0.390 | -0.399 | -0.400 | -0.402 |

Notes: The dependent variable is a county's annual drug-induced mortality rate per 100,000 residents. Demographic controls include the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old. Controls for economic conditions include the unemployment rate, firm births, and per-capita income. Controls for law enforcement and crime include the number of law enforcement officers and total crimes per 100,000 residents. Number of Facilities is the count of treatment facilities in a county by year. The estimates are weighted by county population and standard errors (in parentheses) are corrected for possible clustering at the county level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 18. Heterogeneity in the Effect of Treatment Facilities on Drug-Induced Mortality by Age

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | All Ages | Age-adjusted Mortality Rates | | | | | | | |
| | | <10 | 10-19 | 20-29 | 30-39 | 40-49 | 50-59 | 60-69 | 70+ |
| Number of Facilities | -0.038*** | -0.000 | -0.007** | -0.038*** | -0.069*** | -0.074*** | -0.067*** | -0.028*** | -0.017*** |
| | (0.005) | (0.000) | (0.003) | (0.007) | (0.010) | (0.013) | (0.016) | (0.009) | (0.004) |
| Counties | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 |
| County-Year Observations | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 |
| Mean | 9.390 | 0.170 | 1.809 | 10.81 | 15.34 | 21.40 | 13.89 | 4.927 | 3.121 |
| % Impact | -0.402 | -0.167 | -0.396 | -0.354 | -0.450 | -0.346 | -0.479 | -0.564 | -0.537 |

Notes: The dependent variables are a county's annual age-adjusted drug-induced mortality rate per 100,000 residents. County-by-year controls include the unemployment rate, firm births, per-capita income, the number of law enforcement officers and total crimes per 100,000 residents, and the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old. Number of Facilities is the count of treatment facilities in a county by year. The estimates are weighted by county population and standard errors (in parentheses) are corrected for possible clustering at the county level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 19. Heterogeneity in the Effect of Treatment Facilities on Drug-Induced Mortality Across Race and Gender

| | (1) All | (2) blacks | (3) whites | (4) other | (5) male | (6) female |
|---|---|---|---|---|---|---|
| Number of Facilities | -0.038*** | -0.039*** | -0.039*** | -0.012** | -0.049*** | -0.027*** |
| | (0.005) | (0.014) | (0.006) | (0.005) | (0.007) | (0.004) |
| Counties | 2,409 | 2,407 | 2,409 | 2,409 | 2,409 | 2,409 |
| County-Year Observations | 24,062 | 23,975 | 24,062 | 24,051 | 24,062 | 24,062 |
| Mean | 9.390 | 8.019 | 10.17 | 2.960 | 12.23 | 6.674 |
| % Impact | -0.402 | -0.481 | -0.386 | -0.421 | -0.401 | -0.402 |

Notes: The dependent variables area county's annual demographic-specific drug-induced mortality rate per 100,000 residents corresponding to each column heading. County-by-year controls include the unemployment rate, firm births, per-capita income, the number of law enforcement officers and total crimes per 100,000 residents, and the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old. Number of Facilities is the count of treatment facilities in a county by year. The estimates are weighted by county population and standard errors (in parentheses) are corrected for possible clustering at the county level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 20. Heterogeneity in the Effect of Treatment Facilities on Drug-Induced Mortality Across County Characteristics

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | All Counties | Urban Classification | | | Income | | |
| | | Large Urban | Med/Small Urban | Rural | Low | Med | High |
| Number of Facilities | -0.038*** | -0.037*** | -0.029*** | 0.020 | -0.087 | -0.052** | -0.040*** |
| | (0.005) | (0.010) | (0.011) | (0.051) | (0.059) | (0.025) | (0.005) |
| Counties | 2,409 | 62 | 916 | 1,431 | 802 | 802 | 804 |
| County-Year Observations | 24,062 | 610 | 9,142 | 14,310 | 8,020 | 8,014 | 8,018 |
| Mean | 9.390 | 10.39 | 9.006 | 8.788 | 9.332 | 9.065 | 9.486 |
| % Impact | 0.402 | 0.351 | 0.323 | 0.233 | 0.930 | 0.574 | 0.419 |

Notes: The dependent variable is a county's annual drug-induced mortality rate per 100,000 residents. County-by-year controls include the unemployment rate, firm births, per-capita income, the number of law enforcement officers and total crimes per 100,000 residents, and the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old. Number of Facilities is the count of treatment facilities in a county by year. The estimates are weighted by county population and standard errors (in parentheses) are corrected for possible clustering at the county level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 21. Summary Statistics (N-SSATS Data)

| | (1) N-SSATS Sample (Drug-Induced Mortality) | (2) N-SSATS Sample (Treatment Admissions) |
|---|---|---|
| **Substance-Abuse Treatment Facilities** | | |
| Facilities | 34.41 | 46.51 |
| Facilities (per 100,000) | 4.89 | 3.8 |
| Treatment Admissions | - | 7,190 |
| **Drug-Induced Mortality (per 100,000)** | | |
| All | 10.65 | 10.55 |
| Female | 7.71 | 7.6 |
| Male | 13.73 | 13.63 |
| White | 11.57 | 11.5 |
| Black | 8.67 | 8.82 |
| Other | 3.37 | 3.15 |
| Ages 0-15 | 0.22 | 0.23 |
| Ages 15-39 | 12.41 | 12.13 |
| Ages 40-64 | 18.63 | 18.64 |
| Ages 65+ | 3.58 | 3.59 |
| Counties | 2,182 | 1,471 |
| County-Year Observations | 15,262 | 8,583 |

Notes: All statistics are weighted by county population. The samples are limited to U.S. counties within the 48 contiguous states with consistent county identifiers and at least one substance-abuse treatment facility over the sample time frame (1998-2008). In Column (1), the sample is limited to non-primary care facilities that are licensed, certified, or accredited to provide substance-abuse treatment. The sample in Column (2) includes counties reporting treatment admissions and is limited to states that require publicly funded facilities to report substance-abuse treatment admissions.

TABLE 22. Estimated Effects of Treatment Facilities on Mortality Using N-SSATS Data

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Number of Facilities | -0.041*** | -0.032*** | -0.036*** | -0.034*** | -0.034*** |
| | (0.012) | (0.008) | (0.008) | (0.009) | (0.009) |
| County and Year FE | yes | yes | yes | yes | yes |
| State by Year FE | no | yes | yes | yes | yes |
| Demographic Controls | no | no | yes | yes | yes |
| Controls for Economic Conditions | no | no | no | yes | yes |
| Controls for Law Enforcement/Crime | no | no | no | no | yes |
| Counties | 2,182 | 2,182 | 2,182 | 2,182 | 2,182 |
| County-Year Observations | 15,262 | 15,262 | 15,262 | 15,262 | 15,262 |
| Mean | 10.65 | 10.65 | 10.65 | 10.65 | 10.65 |
| % Impact | 0.389 | 0.302 | 0.335 | 0.317 | 0.317 |

Notes: The dependent variable is a county's annual drug-induced mortality rate per 100,000 residents. Demographic controls include the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old. Controls for economic conditions include the unemployment rate, firm births, and per-capita income. Controls for law enforcement and crime include the number of law enforcement officers and total crimes per 100,000 residents. Number of Facilities is the count of treatment facilities in a county by year. The estimates are weighted by county population and standard errors (in parentheses) are corrected for possible clustering at the county level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 23. Estimated Effects of Treatment Facilities on SAT Admissions Using N-SSATS Data

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Number of Publicly-Funded Facilities | 25.817** | 26.526** | 29.230** | 25.658** | 26.336** |
| | (12.255) | (11.693) | (11.676) | (11.826) | (11.862) |
| Unfunded Facilities | -50.202** | -35.418* | -34.341** | -40.628** | -41.065** |
| | (24.104) | (18.490) | (17.019) | (18.913) | (18.811) |
| County and Year FE | yes | yes | yes | yes | yes |
| State by Year FE | no | yes | yes | yes | yes |
| Demographic Controls | no | no | yes | yes | yes |
| Controls for Economic Conditions | no | no | no | yes | yes |
| Controls for Law Enforcement/Crime | no | no | no | no | yes |
| Counties | 1,471 | 1,471 | 1,471 | 1,471 | 1,471 |
| County-Year Observations | 8,583 | 8,583 | 8,583 | 8,583 | 8,583 |
| Mean | 7,190 | 7,190 | 7,190 | 7,190 | 7,190 |
| % Impact | 0.359 | 0.369 | 0.407 | 0.357 | 0.366 |

Notes: The dependent variable is a county's annual number of substance-abuse treatment admissions into publicly-funded facilities. Demographic controls include the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old. Controls for economic conditions include the unemployment rate, firm births, and per-capita income. Controls for law enforcement and crime include the number of law enforcement officers and total crimes per 100,000 residents. Number of Facilities is the count of treatment facilities in a county by year. The estimates are weighted by county population and standard errors (in parentheses) are corrected for possible clustering at the county level.
* significant at 10%; ** significant at 5%; *** significant at 1%

# APPENDIX B

## SUPPLEMENTARY MATERIALS

TABLE 24. Classification of Cause of Death

| Death Category | ICD-10 (1999-2008) | ICD-9 (1998) |
|---|---|---|
| Drug-Induced Mortality | X40-X45, X60-X65, X85, Y10-Y15 | *282 Category Recode:* 31700, 33800, 35300 |
| Cardiovascular | *113 Category Recode:* 53-75 | *72 Category Recode:* 320-490 |
| Cancer | *113 Category Recode:* 19-44 | *72 Category Recode:* 160-250 |
| Disease/Respiratory/Infection | R092, N390, D849, F03, F019, F069 F79, F179, F109, F329, G35, G122, G319, G409, G419, G459, G700, G809, G825, G919, G931, G934, M349, R54, R628, N289<br><br>*113 Category Recode:* 76-89, 1-18, 50, 51,52, 98-102 | 500, 501, 512, 514, 515, 5070, 5109, 5119, 5130, 5168, 5183, 5184, 5185, 5188, 5191, 5198, 7991, 2791, 2793, 5990, 2900, 2901, 2902, 2904, 2949, 2989, 3109, 311, 319, 3239, 3310, 3314, 3319, 3320, 3352, 340, 3419, 7855, 797, 3440, 3451, 3453, 3459, 3481, 3483, 3485, 3489, 3568, 3580, 5939, 5996, 5920<br><br>*72 Category Recode:* 500-580, 10-140, 650-680 |
| Motor-Vehicle Accidents | *113 Category Recode:* 115-123 | *72 Category Recode:* 800 |
| Suicide | *113 Category Recode:* 124-126 | *72 Category Recode:* 820 |
| Homocide | *113 Category Recode:* 127-130 | *72 Category Recode:* 830 |

Notes: The ninth revision of the International Classification of Diseases (ICD) corresponds with 1998 data and the tenth revision with 1999-2008 data. To group deaths into the above 6 categories, I use either the actual ICD code or a recode of ICD codes grouped into categories. In particular, I use the the 283 category and 72 category recodes corresponding with ICD-9 codes and the 113 category recode corresponding with ICD-10 codes.

TABLE 25. Unweighted Estimated Effects of Treatment Facilities on Drug-Induced Mortality

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Number of Facilities | -0.058*** | -0.041*** | -0.035*** | -0.031*** | -0.031*** |
| | (0.012) | (0.011) | (0.011) | (0.011) | (0.011) |
| County and Year FE | yes | yes | yes | yes | yes |
| State by Year FE | no | yes | yes | yes | yes |
| Demographic Controls | no | no | yes | yes | yes |
| Controls for Economic Conditions | no | no | no | yes | yes |
| Controls for Law Enforcement/Crime | no | no | no | no | yes |
| Counties | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 |
| County-Year Observations | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 |
| Mean | 8.500 | 8.500 | 8.500 | 8.500 | 8.500 |
| % Impact | 0.677 | 0.484 | 0.415 | 0.362 | 0.367 |

Notes: The dependent variable is a county's annual drug-induced mortality rate per 100,000 residents. Demographic controls include the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old. Controls for economic conditions include the unemployment rate, firm births, and per-capita income. Controls for law enforcement and crime include the number of law enforcement officers and total crimes per 100,000 residents. Number of Facilities is the count of treatment facilities in a county by year. Standard errors (in parentheses) are corrected for possible clustering at the county level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 26. Poisson Estimates of the Effect of Treatment Facilities on Drug-Induced Mortality

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Number of Facilities | -0.005*** | -0.004*** | -0.004*** | -0.004*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| County and Year FE | yes | yes | yes | yes |
| Demographic Controls | no | yes | yes | yes |
| Controls for Economic Conditions | no | no | yes | yes |
| Controls for Law Enforcement/Crime | no | no | no | yes |
| Counties | 2,384 | 2,384 | 2,384 | 2,384 |
| County-Year Observations | 23,812 | 23,812 | 23,812 | 23,812 |

Notes: The dependent variable is a county's annual number of drug-induced deaths. Demographic controls include county population and the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old. Controls for economic conditions include the unemployment rate, firm births, and per-capita income. Controls for law enforcement and crime include the number of law enforcement officers and total crimes per 100,000 residents. Number of Facilities is the count of treatment facilities in a county by year. Huber-White robust estimates of the standard errors are in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 27. Estimated effect of Facilities on Drug-Induced Mortality using Lags and Leads

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Number of Facilities$_{t-2}$ | | -0.021*** | | |
| | | (0.006) | | |
| Number of Facilities$_{t-1}$ | -0.038*** | -0.022*** | -0.025*** | -0.026*** |
| | (0.005) | (0.006) | (0.006) | (0.006) |
| Number of Facilities$_{t}$ | | | -0.015** | -0.010 |
| | | | (0.007) | (0.007) |
| Number of Facilities$_{t+1}$ | | | | 0.000 |
| | | | | (0.009) |
| Counties | 2,409 | 2,409 | 2,409 | 2,409 |
| County-Year Observations | 24,062 | 21,653 | 24,062 | 21,653 |

Notes: The dependent variable is a county's annual drug-induced mortality rate per 100,000 residents. County-by-year controls include the unemployment rate, firm births, per-capita income, the number of law enforcement officers and total crimes per 100,000, and the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old. The estimates are weighted by county population and standard errors (in parentheses) are corrected for possible clustering at the county level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 28. Estimated Effects of Covariates on the Number of Facilities

| | (1) | (2) | (3) |
|---|---|---|---|
| Lagged Firm Births | 0.012*** | | 0.012*** |
| | (0.003) | | (0.003) |
| Lagged Per-Capita Income | 0.000** | | 0.000** |
| | (0.000) | | (0.000) |
| Lagged Unemployment Rate | -0.509 | | -0.506 |
| | (0.391) | | (0.391) |
| Lagged Employed Officers | | -0.008 | -0.007 |
| | | (0.006) | (0.005) |
| Lagged Crime Rate | | 0.001 | 0.002 |
| | | (0.002) | (0.002) |
| County and State-by-Year FE | yes | yes | yes |
| Demographic Controls | yes | yes | yes |
| Counties | 2,409 | 2,409 | 2,409 |
| County-Year Observations | 24,062 | 24,062 | 24,062 |

Notes: The dependent variable is a county's number of substance-abuse treatment facilities. Demographic controls include the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old. The estimates are weighted by county population and standard errors (in parentheses) are corrected for possible clustering at the county level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 29. The Effect of Facilities on Alcohol Poisoning Deaths Across Age

| | (1) All | (2) <10 | (3) 10-19 | (4) 20-29 | (5) 30-39 | (6) 40-49 | (7) 50-59 | (8) 60-69 | (9) 70-79 | (10) 80+ |
|---|---|---|---|---|---|---|---|---|---|---|
| Number of Facilities | -0.002*** | -0.000 | -0.000 | -0.001 | -0.001 | -0.004*** | -0.003** | -0.004*** | -0.002*** | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Counties | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 |
| County-Year Observations | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 |
| Mean | 0.226 | 0.000902 | 0.0509 | 0.189 | 0.319 | 0.511 | 0.421 | 0.199 | 0.0726 | 0.0278 |
| % Impact | 0.690 | 0.694 | 0.876 | 0.763 | 0.244 | 0.783 | 0.649 | 1.910 | 3.405 | 1.391 |

Notes: The dependent variable is a county's age-adjusted drug-induced mortality rate per 100,000 residents. County-by-year controls include the unemployment rate, firm births, per-capita income, the number of law enforcement officers and total crimes per 100,000 residents, and the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old. Number of Facilities is the count of treatment facilities in a county by year. The estimates are weighted by county population and standard errors (in parentheses) are corrected for possible clustering at the county level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 30. Estimated Effects on Gender and Race Using Counties with Larger Black Populations

| | (1)<br>all | (2)<br>blacks | (3)<br>whites | (4)<br>other | (5)<br>male | (6)<br>female |
|---|---|---|---|---|---|---|
| Facility | -0.041*** | -0.066*** | -0.044*** | -0.002 | -0.057*** | -0.026*** |
| | (0.009) | (0.022) | (0.011) | (0.006) | (0.011) | (0.008) |
| Counties | 803 | 803 | 803 | 803 | 803 | 803 |
| County-Year Observations | 8,014 | 8,014 | 8,014 | 8,012 | 8,014 | 8,014 |
| Mean | 9.463 | 8.157 | 10.53 | 2.220 | 12.60 | 6.495 |
| % Impact | 0.437 | 0.811 | 0.417 | 0.0774 | 0.450 | 0.405 |

Notes: The sample is limited to counties within the highest tercile of proportion of county residents that are black (greater than 8 percent black). The dependent variables area county's annual demographic-specific drug-induced mortality rate per 100,000 residents corresponding to each column heading. County-by-year controls include the unemployment rate, firm births, per-capita income, the number of law enforcement officers and total crimes per 100,000 residents, and the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old. Number of Facilities is the count of treatment facilities in a county by year. The estimates are weighted by county population and standard errors (in parentheses) are corrected for possible clustering at the county level.
* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 31. Estimated Effects of Treatment Facilities on Other Causes of Death

| | (1)<br>Drug-Induced | (2)<br>All Deaths | (3)<br>Non Drug-Induced | (4)<br>Cardiov | (5)<br>Cancer | (6)<br>Infection / Immune | (7)<br>Disease | (8)<br>MV Acc | (9)<br>Suicide | (10)<br>Homicide | (11)<br>Other |
|---|---|---|---|---|---|---|---|---|---|---|---|
| **Panel A: All Ages** | | | | | | | | | | | |
| Number of Facilities | -0.038*** | -0.098 | -0.055 | 0.011 | 0.004 | -0.030*** | -0.019 | 0.002 | -0.008*** | -0.018*** | 0.003 |
| | (0.005) | (0.063) | (0.062) | (0.028) | (0.014) | (0.010) | (0.018) | (0.002) | (0.002) | (0.003) | (0.014) |
| Counties | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 |
| County-Year Observations | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 |
| Mean | 9.390 | 828.2 | 811.0 | 301.2 | 195.3 | 26.90 | 150.9 | 14.70 | 9.523 | 6.252 | 106.2 |
| % Impact | 0.402 | 0.0119 | 0.00679 | 0.00363 | 0.00181 | 0.110 | 0.0128 | 0.0134 | 0.0796 | 0.290 | 0.00302 |
| **Panel B: Ages less than 65** | | | | | | | | | | | |
| Number of Facilities | -0.041*** | -0.104*** | -0.062*** | -0.006 | -0.002 | -0.016** | -0.009*** | 0.002 | -0.011*** | -0.022*** | 0.002 |
| | (0.005) | (0.019) | (0.018) | (0.006) | (0.005) | (0.007) | (0.003) | (0.003) | (0.002) | (0.004) | (0.010) |
| Counties | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 | 2,409 |
| County-Year Observations | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 | 24,062 |
| Mean | 10.31 | 249.9 | 239.5 | 58.14 | 67.20 | 11.54 | 19.91 | 15.98 | 9.547 | 7.576 | 49.92 |
| % Impact | 0.400 | 0.0414 | 0.0260 | 0.00963 | 0.00368 | 0.138 | 0.0432 | 0.00956 | 0.111 | 0.290 | 0.00308 |

Notes: The dependent variable is a county's annual mortality rate per 100,000 residents corresponding to each column title. County-by-year controls include the unemployment rate, firm births, per-capita income, the number of law enforcement officers and total crimes per 100,000 residents, and the fraction of the county population that are: white, black, male, less than 18 years old, 18-64 years old, and greater than 64 years old. Number of Facilities is the count of treatment facilities in a county by year. The estimates are weighted by county population and standard errors (in parentheses) are corrected for possible clustering at the county level.
* significant at 10%; ** significant at 5%; *** significant at 1%

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