ABSTRACT

LEGAL ACCESS TO ALCOHOL AND ACADEMIC PERFORMANCE:
EVIDENCE FROM MIAMI UNIVERSITY

by Joung Yeob Ha

This paper estimates the effect of legal access to alcohol on academic performance. I find that legal access to alcohol has a negative effect on academic performance, but the magnitude is smaller than previously documented. Exploiting the richness of an administrative data set that links academic performance to student activities, I test for heterogeneous impacts across dimensions not previously studied. Notably I find there is no significant effect of legal access for fraternity and sorority members. Complementing the main analysis with a university wide survey on alcohol consumption provides additional clarity. Underage drinking is prevalent, suggesting that the marginal increase in drinking at age 21 may be smaller in this setting than others. Additionally, the marginal increase in drinking by fraternity and sorority members is smaller than that by non-members.

LEGAL ACCESS TO ALCOHOL AND ACADEMIC PERFORMANCE: EVIDENCE FROM MIAMI UNIVERSITY

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Joung Yeob Ha

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Advisor: Dr. Austin C. Smith

Reader: Dr. William E. Even

Reader: Dr. Charles C. Moul

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Joung Yeob Ha

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and

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Dr. Austin C. Smith

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Dr. William E. Even

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Dr. Charles C. Moul

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Dedication

This thesis is dedicated to my parents and Eunji for their unconditional love and support.

Acknowledgements

I would like to express sincere gratitude to my advisor Dr. Austin Smith for his guidance and support. I am deeply grateful for his patience and encouragement during this project. I would also like to thank Dr. William Even and Dr. Charles Moul for helpful comments and discussions. A special gratitude goes to Dr. Rose Marie Ward and Ms. Denise Krallman for providing me with useful data.

1 Introduction

Differences in drinking behavior among college students from different subgroups have been well documented.[1] However, we know considerably less about the effect of legal access to alcohol on academic performance for these subgroups.[2] Moreover, drinking behavior among college students has changed over time. From 2002 to 2011, both daily drinking and binge drinking among college students decreased, from 5% to 3.8%, and from 44.4% to 39.1% (Johnston et al. 2012; White and Hingson 2014). Changes in drinking behavior might alter the effect of legal access, making it important to examine a recent data set. This paper sheds light on the effect of legal access to alcohol on academic performance using administrative student data and online survey data regarding alcohol-related behavior at Miami University. One contribution of this paper is that it exploits more recent data than prior literature (Carrell, Hoekstra, and West 2011; Lindo, Swensen, and Waddell 2013) that range from 2007 and 2016. Another contribution is that online survey data regarding drinking behavior among students at Miami University provide more internal validity on my sample by considering institution-specific characteristics.

In this paper, I estimate the effect of legal access to alcohol on academic performance using two different identification strategies that have been used in previous papers. My first identification strategy uses a sharp regression discontinuity at age 21, when students may drink alcohol legally. This approach compares performance of students who achieve legality before the final exam to performance of students who cannot access alcohol legally until after the final exam. However, the regression discontinuity approach has limited external validity because it only estimates an effect right at the 21st birthday. My second identification strategy mitigates this drawback, and exploits individual fixed effects to compare performance of students who are pre-21 to their own post-21 performance. Notably this paper expands the heterogeneous effects of legal access to alcohol for different subgroups that include fraternity and sorority members (hereinafter referred to as Greek life members), merit scholarship recipients, Reserve Officers'

[1] These include more frequent drinking among fraternity and sorority members than non-members (DeSimone 2010b), an increase in alcohol consumption of male students affected by merit-aid programs (Cowan and White 2015), different compliance with the U.S. mandated minimum legal drinking age (MLDA) between typical college students and students at the U.S. Air Force Academy in which the MLDA is strictly enforced (Carrell, Hoekstra, and West 2011), and an increase in alcohol consumption among non-athlete students when the collegiate football team succeeds during the football season (Lindo, Swensen, and Waddell 2012).

[2] Different subgroups in Carrell, Hoekstra, and West (2011) include male/female students and high-/low-ability students. Lindo, Swensen, and Waddell (2013) consider students with high/low financial-aid eligibility as well.

Training Corps (ROTC), and student-athletes that show different drinking behavior.

I find that there are significant changes in drinking behavior at age 21, and legal access to alcohol reduces academic performance of students at Miami University by 0.015 standard deviations. This is a smaller effect than documented by the prior literature (Carrell, Hoekstra, and West 2011; Lindo, Swensen, and Waddell 2013), and online survey data suggest that this result is driven by pervasive underage drinking at Miami University. I also find substantial heterogeneous effects of legal access to alcohol on different subgroups.

The rest of this paper proceeds as follows. Section 2 reviews previous literature. In Section 3, I describe my administrative student data and online survey data regarding drinking behavior among students. I provide further details of my setting in Section 4. Section 5 presents my two identification strategies to estimate the effect of legal access to alcohol on academic performance. Section 6 reports my main results, the dynamic effects of legal access to alcohol, and the heterogeneous effects of legal access to alcohol across different subgroups. I conclude my results in Section 7.

2 Literature Review

Previous studies have reported negative impacts of drinking on academic performance among college students (Wolaver 2002; Williams, Powell, and Wechsler 2003).[3] Williams, Powell, and Wechsler (2003) suggest direct and indirect mechanisms through which alcohol consumption might affect academic performance. Though the direct mechanism is not clear, one possible channel is cognitive damage stemming from alcohol consumption. It is also possible that drinking might have positive effects on academic performance by relieving stress and allowing students to interact with high-performing students. The most obvious indirect mechanism is that drinking and alcohol-related social activities alter the time allocation of students. Since both drinking and studying are time consuming, any time spent drinking might reduce time spent studying. Using College Alcohol Study data from Harvard School of Public Health's college, Williams, Powell, and Wechsler (2003) find that the overall effect of drinking on academic performance is negative due to a reduction in study hours.

[3] DeSimone (2010a) also finds negative impacts of alcohol consumption on academic performance among high school students.

In contrast, the effect of legal access to alcohol on academic performance is relatively less known. Carrell, Hoekstra, and West (2011) find that alcohol consumption reduces academic achievement using a regression discontinuity design that exploits the discontinuity in drinking at age 21. They use student data from the U.S. Air Force Academy (USAFA) in which the U.S. mandated minimum legal drinking age (MLDA) is strictly enforced. This strength allows them to separate the effect of alcohol consumption on academic performance from the bias that might be stemming from underage drinking. However, their results might have limited external validity because their sample is different from typical college students. Lindo, Swensen, and Waddell (2013) exploit individual fixed effects as well to estimate the effect of legal access to alcohol on academic performance. They use student data from the University of Oregon, which suggest greater external validity than Carrell, Hoekstra, and West (2011) in terms of providing the effect of legal access to alcohol in a typical-college setting.

Both Carrell, Hoekstra, and West (2011) and Lindo, Swensen, and Waddell (2013) find that legal access has negative effects on academic performance. However, the heterogeneous effects of legal access have not been considered despite previous studies that suggest different drinking behavior among different subgroups such as Greek life members, merit scholarship recipients, students with stiffer penalties for underage drinking, and non-athlete students (DeSimone 2010b; Cowan and White 2015; Carrell, Hoekstra, and West 2011; Lindo, Swensen, and Waddell 2012). This implies that we need further analysis for the heterogeneous effects of legal access to alcohol on academic performance across those subgroups.

3 Data

My primary data are student-course-semester level data from Miami University spanning Fall 2007 through Spring 2016. This is more recent data than what is used in the prior literature (Carrell, Hoekstra, and West 2011; Lindo, Swensen, and Waddell 2013) in which the median year is approximately 2002. Since my identification strategy exploits the variation in legal access to alcohol, I focus on students who enter at 18 or 19 years old and have taken at least one semester when they turn 21. I exclude the performance of students in summer and winter semesters. I also exclude students whose American College Testing (ACT) scores are missing. This leaves a sample that consists of 700,665 observations on 18,533 students.

Following Lindo, Swensen, and Waddell (2013), I use data from the Integrated Postsecondary Education Data System (IPEDS) to provide context for how Miami University compares to a typical-college setting. Summary statistics based on my sample and IPEDS data are shown in Table 1. Column 1 provides characteristics of students in my sample, and Column 2 shows summary statistics of Miami University using IPEDS data. Similarly, Columns 3 and 4 provide summary statistics of other four-year public institutions and flagship state universities both based on IPEDS data.[4]

Columns 1 and 2 verify that my sample represents the general student population of Miami University. They are similar in terms of ACT scores and composition of the student population except for the share of international students. Column 1 shows fewer international students than Column 2. Most international students in my sample are dropped because their ACT scores are missing. This precludes me from analyzing international students who might have different social norms regarding alcohol consumption in their home countries. Columns 2 and 3 report that Miami University shares similar characteristics with other four-year public institutions in terms of the fraction of female students and international students. The fraction of domestic non-minority students at Miami University are 82 percent and this is bigger than the average fraction of other public institutions. However, it is similar that white students account for a great part of student population. It is also similar in terms of the fraction of financial aid recipients while the composition of financial aid varies. Regarding ACT scores, Miami University is similar to flagship state universities. Based on the 25^{th} and 75^{th} percentile of ACT scores of incoming students, Miami University is similar to four-year public institutions such as Indiana University-Bloomington, UC Santa Barbara, and the University of Vermont.

My secondary data are responses of 3,978 students from the online Miami University Student Health Survey regarding drinking behavior conducted between March 3 and March 15, 2017.[5] The online survey includes questions regarding the number of drinking days in a week, the number of alcoholic drinks consumed in a typical drinking day, and peak consumption in the past 30 days. Summary statistics are shown in Table 2. Overall, Table 2 reports that students who are 21 or older consume more drinks and more often across different gender and Greek life membership. Details are discussed in Section 4.

[4] Columns 2 through 4 are calculated using IPEDS data for Fall 2011, which is the median semester of my data.
[5] Ward, Rose Marie. 2017. E-mail message to author, June 21.

4 Drinking Behavior at Miami University

One contribution of this paper is that I have a companion data set with online survey responses regarding alcohol-related behavior of students at Miami University. This allows me to consider institution-specific characteristics that may not be consistent with generalized longitudinal data such as the National Longitudinal Survey of Youth (NLSY97).[6] Also, it may be helpful to understand the difference between estimates of legal access in my setting and those in prior literature. Summary statistics from the survey responses are shown in Table 2. Panel A reports the number of days that students drink in a week. Panels B and C show the consumption of alcohol in a typical drinking day and peak consumption in the past 30 days.

In Table 2, I find that underage drinking is pervasive at Miami University. This result is consistent with data from AlcoholEdu for college, an online education platform for college freshmen. According to data, the proportion of incoming first year students who report themselves as abstainers (no drinks in the last year) before arriving on campus is similar to the national average. However, 24% of abstainers at Miami University start drinking within 4-6 weeks, which is higher than the national level (15%). Moreover, Workman (2014) reports that 21% of first year students at Miami University experience a black-out due to drinking. Table 2 is also consistent with the party school ranking in the U.S. from the Princeton Review. In the most recent years, Miami University has been ranked a top 20 party school in the U.S. while USAFA of Carrell, Hoekstra, and West (2011) has been ranked a top 20 sober school in the U.S.[7] This institution-specific characteristic of Miami University might suggest the possibility of smaller estimates of the effect of legal access to alcohol than those of prior literature.

Moreover, each panel suggests that students drink more intensely when they have legal access to alcohol. Students consume more drinks and more often. Notably Table 2 reports that Greek life members drink more intensely both in their pre- and post-21 periods than non-Greek life members, and male students drink more intensely than female students. Panel A in Table 2 reports that male students show a bigger increase in the number of drinking days in a week after being 21. Similarly, non-Greek life members show a bigger relative increase in the number of

[6] For example, Lindo, Swensen, and Waddell (2013) find that legal access has more negative effects on female students at the University of Oregon while the NLSY97 shows that male college students drink more often when they achieve legality.

[7] Miami University has been ranked the 9th, 16th, 11th, and 19th party school in the U.S. for 2013, 2014, 2015, and 2016, respectively. USAFA has been ranked the 13th and 16th sober school in the U.S. for 2012 and 2013.

drinking days in a week after they turn 21. Though these survey responses do not match exactly with the administrative student data set, this different drinking behavior might suggest the need for examination of the heterogeneous effects of legal access among male/female students and Greek/non-Greek life members.

However, it is worth mentioning that Table 2 shows only difference between drinking behavior among students who are pre-21 and students who are post-21. I cannot directly infer a causal relationship between legal access to alcohol and academic performance using the survey due to the lack of data linking consumption to student records. As such, I only find that there exists an increase in drinking after students have legal access to alcohol, and still rely on the results in Bacolod, Cunha, and Shen (2017), Carpenter and Dobkin (2009), and Lindo, Swensen, and Waddell (2013) to infer a causal effect of legal access on drinking.

5 Empirical strategy

5.1 Regression Discontinuity

Following Carrell, Hoekstra, and West (2011), my first identification strategy uses a sharp regression discontinuity design. I estimate the effect of being 21 at the end of classes (one week before the final examination week) on academic performance by using the following regression:

$$CourseGrade_{ijt} = \beta_0 + \beta_1 Age21_{it} + f(Age_{it}) + \varepsilon_{ijt} \quad (1)$$

where $CourseGrade_{ijt}$ is the normalized course grade for student i in course j in semester t.[8] The course grade is normalized by using a deviation of the student's grade from the course mean divided by the course standard deviation. Age_{it} is the age of the student at the end of the classes in days, centered on age 21. $Age21_{it}$ is an indicator variable that takes a value of one if Age_{it} is greater or equal to zero (i.e. if the student has turned 21). $f(Age_{it})$ is a flexible polynomial function in Age_{it}. ε_{ijt} is a random error term. I consider 240 days as a bandwidth on either side of the age 21 cutoff. It includes students with a birthday 240 days before the end of classes (i.e.

[8] Different sections of the same course are considered different classes due to differences in lecturers, materials, classmates, etc. For example, Intermediate Microeconomic Theory section A (ECO315A) is different from Intermediate Microeconomic Theory section B (ECO315B).

$Age_{it} = -240$) to students with a birthday 240 days after the end of classes (i.e. $Age_{it} = 240$). I examine different functional forms of $f(Age_{it})$ and different bandwidths ranging from 240 days to 20 days. I also estimate the regression above with a set of controls including birth-year fixed effects, course-by-semester-by-year fixed effects, cohort-by-semester-by-year fixed effects, number-of-semester fixed effects, ACT scores, gender, and indicator variables for Greek life members, merit scholarship recipients, ROTC, student-athletes, domestic non-minority, domestic minority, and international students. Course-by-semester-by-year fixed effects control for any common shocks between the same lecturers and/or semesters. Cohort-by-semester-by-year fixed effects control for any common shocks between the same incoming students. Number-of-semester fixed effects flexibly control for cumulative experience of students as they work toward the completion of their degree.

This sharp regression discontinuity design compares the normalized course grade of students who turn 21 just before the end of classes to students who turn 21 just after the end of classes. While the modest assumptions and clear intuition behind regression discontinuity are strengths, this approach has several shortcomings. First, the *local* estimates near the age cutoff might not generalize. The estimated effects of legal access on students who turn 21 at the end of classes might not have external validity on students who turn 21 at the beginning of the semester. For example, students who turn 21 at the beginning of the semester might adjust their drinking behavior over time so that they might show no significant effect of legal access at the end of classes. Another drawback of this specification is that it only exploits the variation in the final exam as a measurement of academic performance, which may account for a small portion of the entire course grade. This might underestimate the effect of legal access to alcohol on academic performance.

5.2 Individual Fixed Effects

My second identification strategy using individual fixed effects follows the approach in Lindo, Swensen, and Waddell (2013). This is my preferred specification because it mitigates some concern regarding my regression discontinuity specification mentioned above. Individual fixed effects compare the academic performance of students before they turn 21 to their own performance after they turn 21. Now, more of the course grade can be impacted by legal access,

rather than just the final exam portion. Moreover, my preferred identification strategy considers more students than the regression discontinuity approach who turn 21 at any time in the semester. It provides more external validity on the effect of legal access on academic performance. Formally, the regression is as follows:

$$CourseGrade_{ijt} = \theta Age21_{it} + \beta X_{ijt} + \alpha_i + u_{ijt} \quad (2)$$

where $CourseGrade_{ijt}$ is the normalized course grade for student i in course j in semester t. $Age21_{it}$ is an indicator variable that takes a value of one if student i can legally access alcohol at any time during semester t. X_{ijt} are a set of class- or semester-varying characteristics including indicator variables for Greek life members, merit scholarship recipients, ROTC, and student-athletes. α_i are a set of individual fixed effects. These individual fixed effects control for unobserved time-invariant characteristics that potentially affect the course grades, such as innate ability. I also examine the regression above with subject-by-level fixed effects, cohort-by-semester-by-year fixed effects, and number-of-semester fixed effects.[9]

6 Results

6.1 Regression Discontinuity

For regression discontinuity analysis, I start with a graphical presentation of the sample. Figure 1 shows the distribution of observations in my sample near the age 21 cutoff. I put raw data into discrete bins for Age_{it}, divided by 14 days. Each bubble indicates the average normalized course grades of students in each bin, plotted at the middle of the bins.[10] The regression lines indicate the predicted normalized course grades for each bin. In Figure 1, there is no apparent discontinuity at the cutoff. To see the remaining variation in normalized course grades after including covariates, I also present the average residuals and Age_{it}. In Figure 2, residuals are calculated from a regression of the normalized course grade on controls. Controls include birth-

[9] Subject-by-level fixed effects are fixed effects for different subjects and class levels. For example, subjects represent English, Mathematics, Economics, etc. Levels represent either 100-, 200-, 300-, or 400- class levels.

[10] For example, the first bubble on the right side of a vertical line at the cutoff indicates the average normalized course grades of all students who turn 21 in the range of 14 days prior to the end of classes (i.e. students range from $Age_{it} = 1$ to $Age_{it} = 14$).

year fixed effects, course-by-semester-by-year fixed effects, cohort-by-semester-by-year fixed effects, number-of-semester fixed effects, ACT scores, gender, and indicator variables for Greek life members, merit scholarship recipients, ROTC, student-athletes, domestic non-minority, domestic minority, and international students. If legal access to alcohol reduces normalized course grades, plotting residuals will show an apparent downward jump at the cutoff after considering the residual variation in normalized course grades using controls (Lee and Lemieux 2010). I do not find clear discontinuity at the age 21 cutoff.

Table 3 reports formal estimates of regression discontinuity. Each column indicates an estimate of the effect of legal access on academic performance with different bandwidths and different functional forms of $f(Age_{it})$ that allow none, linear, or quadratic functions. For robustness, I estimate the same specification with controls, without controls, and with different birthday effects and controls.

Panel A presents the estimates of the effect of legal access to alcohol on academic performance for the full sample without controls. There is no significant difference in academic performance between students who turn 21 just before the end of classes and students who turn 21 just after the end of classes. After adding controls, in Panel B, I find that most estimates are still not significant. This implies that there is no significant effect of gaining legal access at the end of classes.

Panels C and D report the effect of the 20th birthday and 22nd birthday, respectively. These robustness checks allow me to separate the effect of gaining legal access to alcohol from a birthday effect itself. While the 21st birthday is likely related to alcohol consumption, any birthday near the final exam may also have a negative effect on academic performance due to social activities even though they are not related to alcohol consumption. The results imply that there is no significant birthday effect.

A distinct difference of the setting used by Carrell, Hoekstra, and West (2011) is that at USAFA underage drinking is strictly banned. This unique feature of USAFA allows them to estimate a cleaner effect of drinking on academic performance. To apply this advantage to my setting, I consider only ROTC students who face stiffer penalties for underage consumption.[11] Panel E shows that the coefficient estimates do increase in magnitude as expected, but the

[11] Recent underage drinkers in the ROTC program were put on disciplinary probation, disciplinary leave of absence, or disenrolled from the ROTC program.

estimates are noisy due to the small sample size.

While this setting is straightforward in which students are strictly enforced to follow the MLDA, such as the U.S. Air Force Academy (Carrell, Hoekstra, and West 2011) and the U.S. Army (Bacolod, Cunha, and Shen 2017), it is less straightforward to apply this approach to a typical-college setting in which the MLDA is not usually enforced.

6.2 Individual Fixed Effects

6.2.1 Main Results

Table 4 shows the main results of my preferred specification. Each column reports the estimates of the effect of legal access to alcohol on academic performance using different fixed effects. The outcome variable is the normalized course grade. Column 1 uses individual fixed effects to control for any unobserved factors that might affect academic performance but are constant within an individual across time (e.g. innate ability). Column 1 shows that students who are 21 or older perform 0.064 standard deviations worse than when they were younger than 21.

Moving from Column 1 to Column 2, I address the concern that there might exist different common shocks between each subject and each course level by adding subject-by-level fixed effects to my regression (2), and the result is shown in Column 2. Now, the magnitude of the effect of legal access decreases from 0.064 standard deviations to 0.030 standard deviations. In Column 3, I include cohort-by-semester-year fixed effects to control for any common shock within the same cohorts such as changes in university policies. The effect of legal access is reduced from 0.030 standard deviations to 0.015 standard deviations. Moving from Column 3 to Column 4, I include number-of-semester fixed effects to control for cumulative experience of students as they work toward the completion of their degree. I find that the effect of legal access is same as Column 3. Overall, the normalized course grades fall when students have legal access to alcohol, and all estimates are statistically significant. After controlling for potential common shocks, I find that students who are 21 or older perform 0.015 standard deviations worse than when they were younger than 21.

To more deeply understand the main results, I estimate the effect of legal access on grade distribution, course difficulty, and course load as seen in Lindo, Swensen, and Waddell (2013). The results are shown in Table 5. From Column 1 to Column 4, I use a linear probability model

to estimate the effect of legal access on achieving a specific grade. The outcome variables are indicators for earning an A grade, B grade, C grade, and D or F grade, respectively. This analysis uses the same specification as Column 4 in Table 4, which includes individual fixed effects, subject-by-level fixed effects, cohort-by-semester-year fixed effects, and number-of-semester fixed effects. I find that gaining legal access has a negative effect on the probability of earning a good grade. Table 5 reports that students earn an A grade 0.7 percentage points less and C or worse grade approximately 0.4 percentage points more when they have legal access to alcohol. There is no significant effect on the probability of earning a B grade. These results support my main results that legal access has a negative effect on academic performance.

I also examine the effect of legal access on expected semester grade point average (GPA) and the number of credits taken in a semester to consider course-taking behavior of students regarding course difficulty and course load. The analysis in Column 5 enables me to observe whether students take easier courses in semesters during which they will be 21. Column 6 examines whether students take fewer credits when they gain legal access. Compared to Columns 1 through 4, these analyses are conducted at student-by-semester level rather than student-by-course-by-semester level. These different-level analyses provide fewer observations, which are shown in Columns 5 and 6. The dependent variable of Column 5, expected semester GPA is calculated as the course mean from the most recent previous offering of the course that a student is enrolled in this semester. I do not find any significant effects of legal access on expected semester GPA. The dependent variable of Column 6, course load is the number of credits that the student is taking in the semester. The estimate in Column 6 implies that students take slightly fewer credits after being 21.

6.2.2 Fixed Effects Dynamics

To see whether the effect of legal access changes over time, I consider the dynamic effects of legal access on academic performance. The results are shown in Table 6. The dependent variable is the normalized course grade, and my key independent variables are now a set of indicator variables for the age of students at the end of classes divided into six-month intervals. To focus on the impact of legal access to alcohol, the set of indicator variables take a value of one if the age of the student at the end of classes is between 21 and 21.5, between 21.5 and 22, …, between

23.5 and 24, and 24 or older, respectively. I use my preferred specification that includes individual fixed effects, subject-by-level fixed effects, cohort-by-semester-year fixed effects, and number-of-semester fixed effects. The omitted category is students who are younger than 21. In Column 1, the estimates generally become more negative as students become older, and all estimates are statistically significant. While students who just turn 21 perform 0.021 standard deviations worse after gaining legal access, academic performance of students who are 24 or older is 0.152 standard deviations less than their pre-21 performance. Since I control for senioritis or an increase in the level of difficulty as students work toward the completion of their degree using number-of-semester fixed effects and subject-by-level fixed effects, Column 1 suggests that legal access has more negative effects on the normalized course grade the longer the student has had legal access to alcohol.[12]

I also conduct a series of falsification tests from Columns 2 through 5 by including additional indicator variables for pre-21. Column 2 includes an additional indicator for age between 20.5 and 21, and the omitted category is students less than 20.5 years old. Column 3 adds additional indicators for age between 20.5 and 21 and age between 20 and 20.5. The omitted category in Column 3 is students who are younger than 20. Column 4 and Column 5 add more indicator variables for pre-21 in a similar fashion.

Since my goal is to identify the dynamic effects of legal access, including additional indicators for pre-21 might not be relevant because it changes the reference group. For example, in Column 2, the coefficient estimate for age between 21 and 21.5 is -0.022. This indicates that students who are between 21 and 21.5 perform 0.022 standard deviations worse than students who are below 20.5, *not 21*. In a similar fashion, Column 4 reports that students with age between 21 and 21.5 perform 0.039 standard deviations worse than students who are below 19.5. Though including additional pre-21 indicators does not provide exact dynamic effects of gaining legal access, these falsification tests support my main result in Section 6.2.1. From Column 2 through 4, I find that all coefficient estimates for age below 21 are not statistically significant while all estimates for age above 21 are statistically significant.[13] This indicates that legal access indeed has a negative effect on student academic performance while there is no significant

[12] I also examine the possibility that students put less efforts as they become older by including the number of credits taken in a semester (in three-credit intervals) in the regression. The results are similar.

[13] Estimates in Column 5 are not significant because the small reference group makes the comparison difficult. The number of observations for students who are below 19 is 77,270, which is 11% of the entire sample.

difference between students below 21.

6.2.3 Fixed Effects with Heterogeneity

Given differences in drinking behavior among different subgroups, I examine heterogeneous effects of legal access to alcohol. Different drinking behavior includes more frequent drinking among Greek life members than non-Greek life members (DeSimone 2010b), an increase in alcohol consumption of male students affected by merit-aid programs (Cowan and White 2015), different compliance with the MLDA between typical college students and students at USAFA in which the MLDA is strictly enforced (Carrell, Hoekstra, and West 2011), and an increase in alcohol consumption among non-athlete students when the collegiate football team succeeds during the football season (Lindo, Swensen, and Waddell 2012). I test for heterogeneous impacts using my preferred specification that includes individual fixed effects, subject-by-level fixed effects, cohort-by-semester-year fixed effects, and number-of-semester fixed effects. The results are shown in Table 7. Consistent with the previous literature, I consider heterogeneous effects on students with different gender, ability, and financial need. Also, I examine different subgroups of my sample that include Greek life members, merit scholarship recipients, ROTC, and student-athletes. To the best of my knowledge this is the first assessment of the impact of the MLDA for those subgroups.

Overall, Table 7 reports that legal access to alcohol has a negative effect on both male and female students, and the magnitude of the effect on male students is slightly bigger. Male students perform 0.024 standard deviations worse if they have legal access while female students over 21 perform 0.015 standard deviations worse. In Panel A, I estimate the heterogeneous effects of legal access on high and low ability students. Based on the median ACT score of 26, high ability students correspond to students who have higher ACT scores than the median and low ability students correspond to students whose ACT scores are 26 or below. I find that the negative effect of legal access is significant only for high ability students. I divide high ability students by gender, and Panel A reports a significant effect of legal access on male students with high ability while there is no significant effect on female students with high ability. Panel B shows the heterogeneous effects of legal access on students with different financial backgrounds. High financial need corresponds to students whose Expected Family Contribution (EFC) is less

than $15,000, and low financial need corresponds to students whose EFC is $15,000 or above. I find that there are significant effects of legal access on students with low financial need. Similar to Panel A, Panel B reports a significant effect of legal access only on male students with low financial need. Academic performance of male students with low financial need fall by 0.033 standard deviations after being 21.

Panels C through F examine heterogeneous effects of legal access on different subgroups including Greek life members, merit scholarship recipients, ROTC, and student-athletes. In Column 2, I find that students who are non-Greek life members, students without merit scholarship, non-ROTC, and non-athlete students show statistically significant reductions in their academic performance after achieving legal access. The magnitudes are similar to one another, and the coefficient estimates are between -0.018 and -0.015.

In Panel C, the normalized course grades of non-Greek life members fall by 0.018 standard deviations after gaining legality. Interestingly, I find that the effect of legal access on Greek life members is not significant, and its magnitude is the smallest among all subgroups while I find significant effects of legal access on male Greek life members.

Panel D presents results that conflict with those in Panel A as both purport to measure a similar characteristic in terms of academic ability. One possible explanation of the estimates in Panel D is the channel of drinking behavior suggested in Cowan and White (2015). Though they do not formally prove the channel, they suggest that merit scholarship recipients drink more because they get stressed from the requirements to maintain the scholarship, and this drinking behavior is reinforced by the wealth effects of merit scholarship with an assumption that alcohol is a normal good. As such, students with merit scholarship may start drinking prior to 21 so that the effects of legal access do not show significant estimates. Alternatively, merit scholarship recipients might be more dedicated students and might not let drinking interfere with their academic achievement before or after age 21.

In Panel E, I find that the magnitude of the effect of legal access on ROTC is the biggest among subgroups but the estimate is not statistically significant due to the small sample size. Since ROTC students face stiffer penalties for underage drinking than non-ROTC students, this may support the pervasive drinking culture in my setting in which the MLDA is not strongly enforced.

Panel F reports that the normalized course grades of non-athlete students fall by 0.015

standard deviations after they turn 21. Also, I find that the negative effect of legal access on male non-athlete students is much bigger than that of female non-athlete students. Though I do not have information regarding any collegiate sports teams and their seasons at Miami University, it might be a possible scenario that bigger effects of legal access are derived from male non-athlete students who show more partying when the collegiate football team wins (Lindo, Swensen, and Waddell 2012), which might be highly correlated with both more alcohol consumption and worse academic performance.

7 Conclusion

Overall, I find that there exist significant changes in drinking behavior at age 21, and legal access to alcohol reduces academic performance of students at Miami University by 0.015 standard deviations. This is a smaller effect than found in the prior literature (Carrell, Hoekstra, and West 2011; Lindo, Swensen, and Waddell 2013).[14] Lindo, Swensen, and Waddell (2013) report that the negative effect of legal access to alcohol on academic performance is twice that of my results. This reduction in academic performance is similar to what would be expected if a student's SAT scores were 20 points lower. Summary statistics from online survey data suggest that these results may be driven by pervasive underage drinking at Miami University. However, it is unclear whether the smaller effects are derived from the recent decreases in alcohol consumption among college students, the pervasive drinking culture at Miami University, or both due to the lack of data.

I also find that there are substantial heterogeneous effects of legal access to alcohol on different subgroups. Legal access to alcohol has negative effects on non-Greek life members, students without the merit scholarship, non-ROTC, and non-athlete students. Moreover, male students are affected more negatively than female students among those subgroups.

[14] Carrell, Hoekstra, and West (2011) report that legal access reduces the normalized course grade by 0.1 standard deviations, and Lindo, Swensen, and Waddell (2013) report the normalized course grade are reduced by 0.03 standard deviations.

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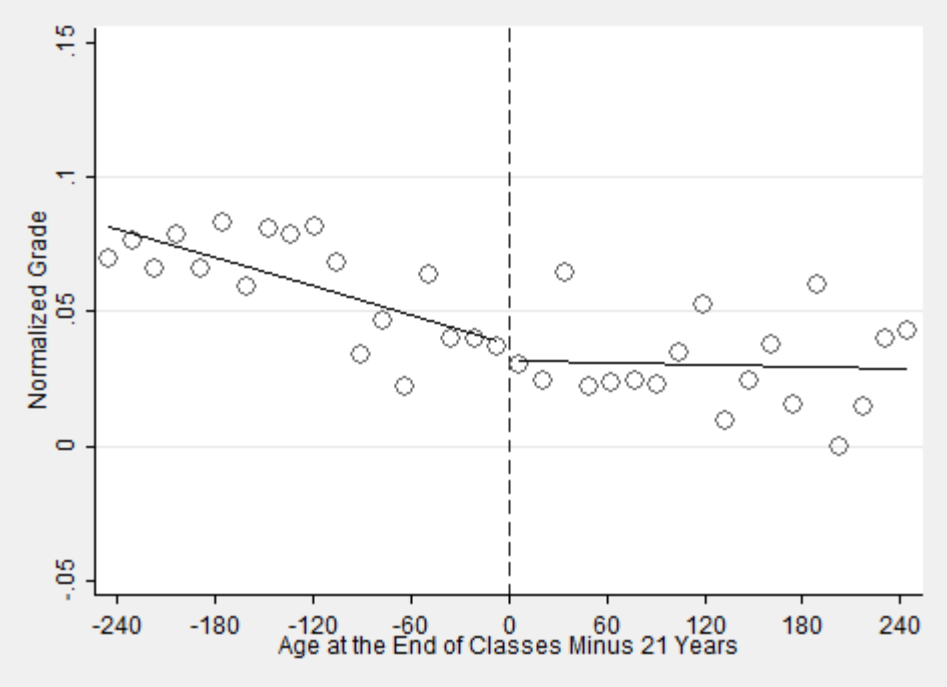

Figure 1: Regression Discontinuity Estimates of the Effect of Legal Access to Alcohol on Academic Performance

Notes: Each bubble indicates the average of normalized course grades for the bandwidth of 14 days. The regression lines indicate the predicted normalized course grades for each bin.

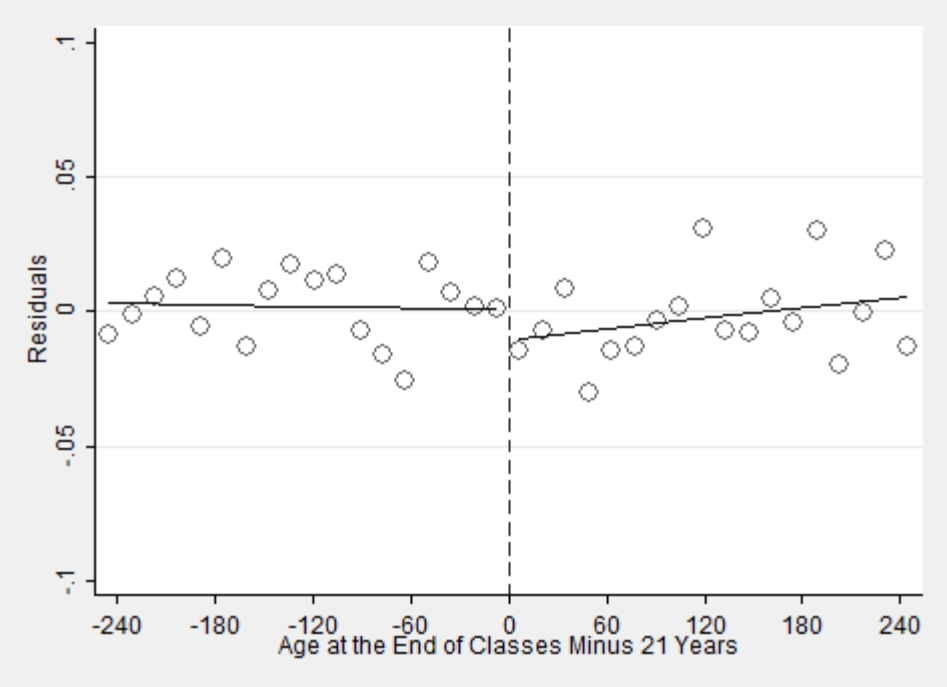

Figure 2: Regression Discontinuity Estimates of the Effect of Legal Access to Alcohol on Academic Performance - Residuals

Notes: The residuals are calculated from a regression of the normalized course grade on controls. Controls include birth-year fixed effects, course-by-semester-by-year fixed effects, cohort-by-semester-by-year fixed effects, number-of-semester fixed effects, ACT scores, gender, and indicator variables for Greek life members, merit scholarship recipients, ROTC, student-athletes, domestic non-minority, domestic minority, and international students.

Table 1: Descriptive Statistics

	Miami University (Sample)	Miami University (IPEDS)	Four-year Public U.S. Institutions (IPEDS)	Flagship State Universities (IPEDS)
	(1)	(2)	(3)	(4)
ACT Composite 25th percentile scores	24	24	20	23
ACT Composite 75th percentile scores	29	29	25	28
Admission rate		74	67	67
Graduation rate		81	49	68
Number of undergraduates	18,533	14,936	11,350	21,814
Fraction female	0.53	0.52	0.54	0.51
Fraction domestic minority	0.11	0.12	0.30	0.22
Fraction domestic non-minority	0.88	0.82	0.63	0.71
Fraction international	0.01	0.05	0.03	0.04
Total price for in-state students living on campus		30,536	20,564	22,651
Total price for out-of-state students living on campus		45,708	30,705	37,471
Fraction receiving any financial aid		0.76	0.86	0.81
Fraction receiving federal-grant aid		0.17	0.41	0.25
Fraction receiving student-loan aid		0.45	0.60	0.48

Notes: Data used in the first column come from Miami University undergraduates during Fall 2007 through Spring 2016. Last three columns are calculated using Fall 2011 IPEDS data. The number of institutions for Column 3 and Column 4 are 437 and 45, respectively. The number of undergraduates in Column 1 is not comparable to those in Columns 2 through 4.

Table 2: Drinking Behavior at Miami University

Panel A: Drinking days in a week	All sample					
	Age < 21	Age ≥ 21	T-statistics			
Mean	1.34	1.96	-12.44			
Standard deviation	(1.34)	(1.55)				
Observations	2647	1331				
	Male			Female		
	Age < 21	Age ≥ 21	T-statistics	Age < 21	Age ≥ 21	T-statistics
Mean	1.45	2.22	-9.02	1.30	1.78	-7.91
Standard deviation	(1.46)	(1.66)		(1.27)	(1.43)	
Observations	890	564		1726	745	
	Greek			Non-Greek		
	Age < 21	Age ≥ 21	T-statistics	Age < 21	Age ≥ 21	T-statistics
Mean	2.00	2.68	-10.25	0.77	1.44	-10.86
Standard deviation	(1.27)	(1.32)		(1.11)	(1.50)	
Observations	1237	563		1410	768	
Panel B: Consumption in a typical drinking day	All sample					
	Age < 21	Age ≥ 21	T-statistics			
Mean	3.01	3.43	-4.07			
Standard deviation	(2.97)	(3.12)				
Observations	2645	1332				
	Male			Female		
	Age < 21	Age ≥ 21	T-statistics	Age < 21	Age ≥ 21	T-statistics
Mean	3.84	4.24	-1.91	2.58	2.85	-2.81
Standard deviation	(3.78)	(3.96)		(2.34)	(2.12)	
Observations	890	565		1724	745	
	Greek			Non-Greek		
	Age < 21	Age ≥ 21	T-statistics	Age < 21	Age ≥ 21	T-statistics
Mean	4.01	4.55	-3.31	2.13	2.60	-3.90
Standard deviation	(2.98)	(3.31)		(2.66)	(2.70)	
Observations	1234	563		1411	769	
Panel C: Peak consumption in the past 30 days	All sample					
	Age <21	Age ≥21	T-statistics			
Mean	4.62	6.18	-9.03			
Standard deviation	(4.93)	(5.25)				
Observations	2644	1334				
	Male			Female		
	Age <21	Age ≥21	T-statistics	Age < 21	Age ≥ 21	T-statistics
Mean	6.12	7.75	-4.95	3.86	5.06	-6.69
Standard deviation	(6.24)	(6.05)		(3.84)	(4.20)	
Observations	887	567		1726	745	
	Greek			Non-Greek		
	Age <21	Age ≥21	T-statistics	Age < 21	Age ≥ 21	T-statistics
Mean	6.42	8.19	-6.74	3.05	4.70	-8.03
Standard deviation	(5.00)	(5.25)		(4.30)	(4.73)	
Observations	1234	565		1410	769	

Notes: T-statistics are calculated by Welch's T-test with the null hypothesis that mean difference between students below 21 and above 21 is equal to zero.
Source: Miami University Student Health Survey, Spring 2017.

Table 3: Regression Discontinuity Estimates of the Effect of Legal Access to Alcohol on Academic Performance

Bandwidth Age polynomial	240 days Linear (1)	240 days Quadratic (2)	210 days Linear (3)	210 days Quadratic (4)	180 days Linear (5)	180 days Quadratic (6)	150 days Linear (7)	120 days Linear (8)	100 days Linear (9)	80 days Linear (10)	80 days None (11)	60 days None (12)	40 days None (13)	20 days None (14)
Panel A: no controls														
Discontinuity at age 21	-0.004	0.005	0.001	-0.002	0.002	-0.001	0.010	-0.002	-0.004	-0.004	-0.011	-0.013	0.001	-0.002
	(0.011)	(0.021)	(0.012)	(0.021)	(0.014)	(0.024)	(0.017)	(0.019)	(0.020)	(0.022)	(0.011)	(0.013)	(0.015)	(0.022)
Observations	233,715	233,715	205,360	205,360	176,974	176,974	147,658	118,370	100,425	81,388	81,388	61,536	41,174	20,820
Panel B: with controls														
Discontinuity at age 21	-0.029**	-0.023	-0.024*	-0.027	-0.024	-0.023	-0.024	-0.030	-0.030	-0.036	-0.032**	-0.049***	-0.030	-0.039
	(0.012)	(0.020)	(0.014)	(0.020)	(0.015)	(0.023)	(0.018)	(0.019)	(0.021)	(0.023)	(0.014)	(0.016)	(0.021)	(0.035)
Observations	233,715	233,715	205,360	205,360	176,974	176,974	147,658	118,370	100,425	81,388	81,388	61,536	41,174	20,820
Panel C: with controls														
Discontinuity at age 20	-0.014	-0.012	-0.012	-0.012	-0.016	-0.011	-0.012	-0.019	-0.019	-0.010	-0.024*	-0.032**	-0.021	-0.017
	(0.012)	(0.018)	(0.013)	(0.018)	(0.014)	(0.021)	(0.016)	(0.018)	(0.019)	(0.020)	(0.013)	(0.015)	(0.018)	(0.028)
Observations	242,822	242,822	212,620	212,620	182,979	182,979	153,343	123,381	104,621	84,513	84,513	63,975	42,534	21,276
Panel D: with controls														
Discontinuity at age 22	-0.018	-0.001	-0.014	-0.011	-0.011	-0.015	-0.004	-0.009	-0.014	-0.001	-0.019	-0.006	0.014	0.030
	(0.015)	(0.023)	(0.016)	(0.024)	(0.018)	(0.027)	(0.021)	(0.022)	(0.024)	(0.026)	(0.016)	(0.019)	(0.023)	(0.041)
Observations	175,846	175,846	155,673	155,673	135,088	135,088	113,939	93,313	79,891	64,678	64,678	48,684	32,516	16,343
Panel E: ROTC with controls														
Discontinuity at age 21	-0.219	-0.421	-0.263	-0.427	-0.390	-0.320	-0.447	-0.462	-0.334	-0.529	-0.484	-1.135	-0.861	-0.860
	(0.381)	(0.608)	(0.442)	(0.646)	(0.553)	(0.803)	(0.647)	(0.798)	(0.969)	(1.130)	(0.695)	(1.208)	(1.834)	(2.501)
Observations	2,371	2,371	2,090	2,090	1,816	1,816	1,535	1,193	1,002	833	833	570	414	195

Notes: The dependent variable is the normalized course grade. Controls include birth-year fixed effects, course-by-semester-by-year fixed effects, cohort-by-semester-by-year fixed effects, number-of-semester fixed effects, ACT scores, gender, and indicator variables for Greek life members, merit scholarship recipients, ROTC, student-athletes, domestic non-minority, domestic minority, and international students. Standard errors clustered by the birthday level are in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

Table 4: Fixed Effects Estimates of the Effect of Legal Access to Alcohol on Academic Performance

	(1)	(2)	(3)	(4)
Age ≥ 21 at any time during semester	-0.064***	-0.030***	-0.015***	-0.015***
	(0.004)	(0.004)	(0.005)	(0.005)
Individual fixed effects	Yes	Yes	Yes	Yes
Subject-by-level fixed effects	No	Yes	Yes	Yes
Cohort-by-semester-by-year fixed effects	No	No	Yes	Yes
Number-of-semester fixed effects	No	No	No	Yes
Number of students	18,533	18,533	18,533	18,533
Observations	700,665	700,665	700,665	700,665

Notes: The dependent variable is the normalized course grade. Number-of-semester fixed effects are fixed effects for number of semesters taken by a student. Standard errors clustered by the individual level are in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

Table 5: The Effect of Legal Access to Alcohol on Grade Distribution, Course Difficulty, and Course Load

	(1)	(2)	(3)	(4)	(5)	(6)
	A grade	B grade	C grade	D or F grade	Expected semester GPA	Course load
Age ≥ 21 at any time during semester	-0.007***	0.000	0.004**	0.003***	-0.004	-0.049*
	(0.003)	(0.003)	(0.002)	(0.001)	(0.003)	(0.029)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Subject-by-level fixed effects	Yes	Yes	Yes	Yes	-	-
Cohort-by-semester-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number-of-semester fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of students	18,533	18,533	18,533	18,533	18,533	18,533
Observations	700,665	700,665	700,665	700,665	137,994	137,994

Notes: From Columns 1 through 4, the dependent variable is the probability of earning an A, B, C, or D or F, respectively. The dependent variable of Column 5, expected semester GPA is calculated as the course mean from the most recent previous offering of the course that a student is enrolled in this semester. The dependent variable of Column 6, course load is the number of credits that the student is taking in the semester. Standard errors clustered by the individual level are in parentheses. *Significant at 10%; **Significant at 5%; *** Significant at 1%.

Table 6: The Dynamic Effects of Legal Access to Alcohol on Academic Performance

	(1)	(2)	(3)	(4)	(5)
Age between 19 and 19.5					0.012
					(0.007)
Age between 19.5 and 20				-0.004	0.012
				(0.007)	(0.012)
Age between 20 and 20.5			-0.008	-0.012	0.009
			(0.006)	(0.010)	(0.016)
Age between 20.5 and 21		-0.001	-0.009	-0.015	0.011
		(0.006)	(0.009)	(0.013)	(0.020)
Age between 21 and 21.5	-0.021***	-0.022**	-0.033***	-0.039**	-0.008
	(0.006)	(0.009)	(0.012)	(0.017)	(0.025)
Age between 21.5 and 22	-0.035***	-0.036***	-0.048***	-0.056***	-0.020
	(0.009)	(0.013)	(0.016)	(0.020)	(0.029)
Age between 22 and 22.5	-0.063***	-0.065***	-0.079***	-0.087***	-0.046
	(0.013)	(0.016)	(0.019)	(0.024)	(0.033)
Age between 22.5 and 23	-0.082***	-0.084***	-0.100***	-0.110***	-0.063
	(0.018)	(0.021)	(0.024)	(0.029)	(0.038)
Age between 23 and 23.5	-0.100***	-0.102***	-0.120***	-0.131***	-0.079
	(0.031)	(0.033)	(0.035)	(0.039)	(0.048)
Age between 23.5 and 24	-0.095**	-0.098**	-0.117**	-0.130**	-0.073
	(0.048)	(0.049)	(0.051)	(0.055)	(0.063)
Age $\geq$ 24	-0.152*	-0.155*	-0.176*	-0.190**	-0.127
	(0.089)	(0.090)	(0.091)	(0.093)	(0.099)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes
Subject-by-level fixed effects	Yes	Yes	Yes	Yes	Yes
Cohort-by-semester-by-year fixed effects	Yes	Yes	Yes	Yes	Yes
Number-of-semester fixed effects	Yes	Yes	Yes	Yes	Yes
Number of Students	18,533	18,533	18,533	18,533	18,533
Observations	700,665	700,665	700,665	700,665	700,665

Notes: The dependent variable is the normalized course grade. Standard errors clustered by the individual level are in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

Table 7: The Heterogeneous Effects of Legal Access to Alcohol on Academic Performance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Gender & ability	Male	Female	High ability	Low ability	Male high ability	Male low ability	Female high ability	Female low ability
Age ≥ 21 at any time during semester	-0.024***	-0.015**	-0.023***	-0.011	-0.035***	-0.015	-0.015	-0.017*
	(0.008)	(0.007)	(0.008)	(0.008)	(0.012)	(0.012)	(0.010)	(0.010)
Observations	331,491	369,174	337,702	362,963	175,693	155,798	162,009	207,165
Panel B: Gender & financial need	Male	Female	High need	Low need	Male high need	Male low need	Female high need	Female low need
Age ≥ 21 at any time during semester	-0.024***	-0.015**	-0.010	-0.018***	-0.001	-0.033***	-0.025*	-0.010
	(0.008)	(0.007)	(0.011)	(0.006)	(0.018)	(0.009)	(0.014)	(0.008)
Observations	331,491	369,174	187,636	513,029	83,086	248,405	104,550	264,624
Panel C: Greek life member	Greek	Non-Greek	Male Greek	Male non-Greek	Female Greek	Female non-Greek		
Age ≥ 21 at any time during semester	-0.004	-0.018**	-0.024*	-0.022**	0.000	-0.020**		
	(0.008)	(0.007)	(0.013)	(0.011)	(0.011)	(0.010)		
Observations	239,913	460,752	106,434	225,057	133,479	235,695		
Panel D: Merit scholarship recipient	Merit recipient	Non-merit recipient	Male merit recipient	Male non-merit recipient	Female merit recipient	Female non-merit recipient		
Age ≥ 21 at any time during semester	-0.007	-0.016**	-0.001	-0.026**	-0.016	-0.015		
	(0.011)	(0.007)	(0.017)	(0.011)	(0.014)	(0.009)		
Observations	283,339	417,326	126,367	205,124	156,972	212,202		
Panel E: ROTC	ROTC	Non-ROTC	Male ROTC	Male non-ROTC	Female ROTC	Female non-ROTC		
Age ≥ 21 at any time during semester	-0.046	-0.015***	-0.056	-0.023***	0.080	-0.015**		
	(0.052)	(0.006)	(0.057)	(0.008)	(0.101)	(0.007)		
Observations	6,331	694,334	4,991	326,500	1,340	367,834		
Panel F: Student-athlete	Athlete	Non-athlete	Male athlete	Male non-athlete	Female athlete	Female non-athlete		
Age ≥ 21 at any time during semester	-0.019	-0.015***	-0.035	-0.025***	-0.025	-0.013*		
	(0.028)	(0.006)	(0.039)	(0.009)	(0.039)	(0.007)		
Observations	27,023	673,642	14,200	317,291	12,823	356,351		

Notes: The dependent variable is the normalized course grade. High ability students are students with ACT scores above the sample median, and low ability students are students whose ACT scores are equal to or below the sample median. Students with high financial need are students whose EFC is lower than $15,000, and students with low financial need are students whose EFC is $15,000 or above. Standard errors clustered by the individual level are in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.