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ABSTRACT

We consider the effect of legal access to alcohol on student achievement. We first estimate the effect using an RD design but argue that this approach is not well suited to the research question in our setting. Our preferred approach instead exploits the longitudinal nature of the data, identifying the effect by measuring the extent to which a student's performance changes after he gains legal access to alcohol, controlling flexibly for the expected evolution of grades as students make progress towards their degrees. 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.

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1 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 legal drinking age (MLDA). However, to the extent to which legal access to alcohol influences many factors that are likely to affect academic performance, we acknowledge that valid instrumental variables estimates of the effect of alcohol consumption cannot be obtained in our setting. In particular, because legal access is likely to affect how often students drink, how much they drink when they drink, where they spend their time (e.g., increasing the amount of time in bars and clubs), and who they spend time with, the exclusion restriction would likely fail if we were to use legal alcohol access as an instrument for some measure of drinking.[2] As such, we focus on the reduced-form effect of legal access on college performance—inclusive of several potential

[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 forthcoming), sexual activity (Chesson, Harrison, and Kassler 2000; Rees and Argys 2001; Sen 2002; Rashad 2004; Carpenter 2005b; Waddell forthcoming), employment (Mullahy and Sindelar 1996; Terza 2002; Dave and Kaestner 2002; MacDonald 2004), and teenagers' educational outcomes (Cook and Moore 1993; Dee and Evans 2003; Chatterji and DeSimone 2006), among others.

[2] We confirmed this concern in pilot surveys undertaken in upper-division undergraduate classes at the University of Oregon where 21-year-old students were asked to compare their behaviors in the four months after they turned 21 to their behaviors in the four months before. Based on this sample of 78 students, on a weekly basis, turning 21 increased the number of alcoholic beverages consumed 1.4, the number of days consuming an alcoholic drink by 0.5, the number of days drinking to the point of intoxication by 0.2, and the number of days going to a bar or club by 1.1. 25 percent of students reported they started to hang out with different people, and 21 percent reported that they changed the amount of time spent with earlier friends. More generally, 10 percent reported they started to hang out with a different crowd, suggesting that changing group dynamics may also be important around the 21st birthday. Among students reporting changes in who they spent time with, approximately two-thirds attributed the change to changes in drinking-related activities.

mechanisms—while remaining no less relevant to policy.

That said, and despite the fact that some individuals drink illegally prior to turning 21, Carpenter and Dobkin (2009) show that legal access has a significant impact on drinking behavior. Most relevant to our setting, they show that college-aged young adults just over age 21 report drinking 11-21 percent more than than those just under 21, depending on the measure of consumption used. While obtaining valid first-stage estimates is problematic in our setting for the reasons described above, that turning 21 is associated with such large increases in alcohol consumption suggests that increased consumption is an important mechanism through which performance may be impaired.

Our first identification strategy to estimate the effect of legal access 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 effects 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 (LATE) 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 take easier classes 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. 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 underage drinking prohibition is taken extremely seriously there—much more so than in other institutional settings in which 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 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 we anticipate leading to improved 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

[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 an instrument 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.

[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.

research. The effect is statistically significant, manifests in the term a student turns 21, is not strongly related to when within the relevant quarter a student has their 21st birthday, and persists into later academic terms. In addition, we find that effects are especially large for female students, "low-ability" students, and males who are most likely to be from disadvantaged backgrounds.

The rest of this paper is organized as follows. In Section 2 we discuss the data used in this analysis as well as the representative nature of the University of Oregon campus. In Section 3 we present an RD strategy and discuss the resulting estimates. In Section 4 we present our preferred longitudinal approach and discusses our main empirical findings. We conclude and discuss the implications of our results in Section 5.

2 Data

In this paper, we use 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.[5] Because our identification strategies use variation provided by the federal MLDA law, we require students in the sample to be observed at least one term in which they are at least 21 years old. The resulting sample consists of 13,102 students contributing 479,342 total observations.

As one contribution of this paper is to provide insight into the effects of MLDA laws in a "typical-college setting," Table 1 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 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.[6]

[5] 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.

[6] 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.

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 very 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.

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 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.

3 RD Analysis

3.1 Empirical Strategy

In this section, we estimate the effect of having had one's 21st birthday before the end of the academic term on academic performance using the following regression equation:

$$G_{ijt} = \alpha_0 + \alpha_1 1\{AGE_{it} \geq 0\} + f(AGE_{it}) + \epsilon_{ijt} \tag{1}$$

where G_{ijt} is the normalized grade for student i in class j in term t. Normalized grade is calculated as a student's grade deviation from the class mean divided by the class standard deviation. 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, $f(AGE_{it})$ controls for a student's age at the end of the term in a flexible manner. In practice, we estimate models that do not control for age at the end of the term, models that control for age at the end of the term with a linear specification flexible on each side of the cutoff, and models that control for age at the end of the term with a quadratic specification flexible on each side of the cutoff, and consider 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 details—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 we know that drinking tends to increase when one turns 21 (Carpenter and Dobkin, 2009), 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.[7]

[7]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.

In the absence of estimated effects on drinking behavior, the results are appropriately characterized as intent-to-treat effects, measuring the reduced-form effect of the minimum drinking age law which is certainly of interest in itself. However, that 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, remains a disadvantage of this approach, something that we improve on with the identification strategy presented in the next section where we exploit the longitudinal nature of the data.

3.2 Results

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 Panel A reports unadjusted estimates, Panel B 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.[8]

Overall, the set of results in Table 2 provides evidence 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, Table 3 reports the results from a similar exercise but instead considers the effect of turning 20 before a quarter ends. In particular, these results test for a "twentieth birthday effect" which would raise the concern that the estimates in Table 2 might reflect a "21st birthday effect" that cannot be separated from the effect of gaining legal access to alcohol near the end of the term. Although the estimates in Table

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

3 are rarely significant, the fact that 26 of the 28 point estimates are negative casts further doubt on the validity of this strategy in our setting.[9]

4 Longitudinal Analysis

In this section, we use our preferred approach to estimate the effects of legal access to alcohol, which 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:

$$G_{ijt} = \theta AGE21_{it} + \beta X_{ijt} + \alpha_i + u_{ijt} \tag{2}$$

where G_{ijt} is the normalized grade for student i in class j in term t, $AGE21_{it}$ 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, α_i are a set of individual fixed effects, and u_{ijt} is a random error term. In practice, we always include "experience controls" in X_{ijt}, i.e., fixed effects for the number of accumulated credits (in intervals of four) to control for grade changes that are expected as a student progresses towards his degree.[10] For example, these variables are intended to control for phenomena such as "senioritis." As such, the estimation strategy essentially compares a student's grades after turning 21 to what would be expected based on his average prior performance and accumulated experience.[11]

[9] In the appendix we show that a similar exercise considering the effect of turning 22 before a quarter ends indicates limited evidence for the presence of a birthday effect. Carrell, Hoekstra, and West (2011) conduct a similar analysis and find no evidence of 20th or 22nd birthday effects at the U.S. Air Force Academy. In an attempt to separate the short-term 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, forthcoming; 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.

[10] 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 cumulative-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 cumulative credits fixed effects and the results are quite similar.

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

4.1 Main Results

Table 4 presents our main results. 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 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.[12]

Although the above estimates 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. This issue is explored in Table 5, which considers the effect of legal alcohol access on course difficulty and course loads. This analysis is identical to that in Table 4 except 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 but still can include term fixed effects. As a measure of course difficulty, the upper panel focuses on a student's expected GPA, which is based on

[12]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.

the average grades in the previous offering of each of the courses he is taking. The lower panel focuses on the number of credits a student takes in a given term. In the upper panel, there is evidence that legal access has a small influence on selection into courses based on difficulty—our preferred estimate in Column 4 suggest that turning 21 leads students to take course loads with 0.009 higher expected GPAs. In the lower panel, our preferred estimate reveals no significant impact on the number of credits a student takes in a given quarter.

4.2 Treatment-Effect Dynamics

In order to consider the dynamic effect of being able to drink legally, we 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.[13]

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 the next section in a series of falsification tests—our preferred estimates 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 would be noisier and less reliable than estimates that do not include these indicator variables and

[13]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.

instead use all pre-21 terms to form counterfactuals.

Our preferred estimates of the treatment effect dynamics, shown in Column 4 of Table 6, 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. In the next section, we show that there is no evidence of a similar 20th-birthday effect (in contrast to the RD-based analysis discussed above) which suggests that a 22nd-birthday effect may itself be related to legal alcohol access and its associated activities. That said, this estimate is not significantly different from the estimated effect of being in the term of one's 21st birthday (p-value = 0.07).

In Figure 1 we present a graphical analogue to our preferred approach to estimation. 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 4 of Table 6, 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.

Column 5 of Table 6 turns the attention to the timing of a student's 21st birthday during the quarter. In particular, in this column we replace the indicator for turning 21 in the current term with an indicator for turning 21 in weeks 10–11 of the current quarter, weeks 7–9 of the current quarter, weeks 4–6 of the current quarter, and weeks 1–3 of the current quarter. In large part, it is not clear what pattern of estimates we would expect this analysis to reveal. On one hand, the effects might be most severe for students who gain legal access at the beginning of the term since they will be exposed for a longer time, potentially impairing their learning throughout the entire quarter. On the other hand, an early-term birthday may allow students to "get it out of their system" early in the quarter, leading to greater focus near the end of the term when studying may be most productive.

The set of point estimates in Column 4 suggest that there are effects of gaining legal access to alcohol at any time during a given quarter. However, we note that the estimated effect of a being able to drink legally as of the tenth or eleventh week of a given quarter is relatively small, which is what we would expect since a share of these birthdays will have taken place after students have already completed their final exams.[14] The point estimates indicate that turning 21 in weeks 7–9 reduce current-term grades by 0.035 standard deviations, turning 21 in weeks 4–6 reduce grades by 0.048 standard deviations, and turning 21 in weeks 1–3 reduces grades by 0.038 standard deviations. As such, it appears as if the most severe effects arise for students who are able to start drinking legally midway through the quarter which does not provide clear evidence against either of the hypotheses described above. Further, the standard errors are too large to reject that the effect is the same for students gaining legal access to alcohol at different times during the quarter.

4.3 Falsification Exercise

In this section, we subject our preferred 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 effects are evident in terms before a student turns 21.

In order to maximize power, we take an incremental approach to adding indicator variables for terms preceding an individual's 21st birthday. As we alluded to in the previous section, if one adds many pre-treatment variables to such a regression model, the individual fixed effects and trends will be poorly measured and the resulting estimates will be extremely noisy. As such, a falsification test that simultaneously includes several pre-treatment variables may not be very convincing even if the "placebo tests" are not statistically significant.

Table 7 shows the results of this exercise, displaying our preferred estimate from Table 6 in Column 1. 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

[14] We do not have information on the exact date on which specific courses held final exams.

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 with turning 20.

4.4 Treatment-Effect Heterogeneity

In tables 8 and 9 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.

Table 8 stratifies the sample by student gender and ability, with "high ability" students defined as those with cumulative SAT scores above the sample median of 1120 and "low ability" students defined as those at or below the sample median.

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. Columns 3 and 4 suggests that similar differences exist across ability, with point estimates indicating that the effect on low-ability students is greater than the effect on high-ability students.

Columns 5 through 8 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.

Table 9 stratifies the estimates by financial-aid eligibility and gender for the seventy percent of students who submitted a Free Application for Federal Student Aid (FAFSA). Column 1 shows 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). 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.

5 Discussion and Conclusion

As a whole, our analysis suggests that legal access to alcohol does affect student performance, reducing grades by 0.03 standard deviations. To put this magnitude into context, it is equivalent to causing a student to perform as if his or her SAT score were 20 points lower.

In addition to what was discussed in the introduction, one of the benefits of our longitudinal analysis is its ability to speak to the extent to which the effect is sensitive to the timing of a student's 21st birthday within the term. The estimates suggest that the effect is just as great for those turning 21 at the end of a term as it is those turning 21 at the beginning of the term. 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, in contrast to this earlier work, we identify a significant effect on the performance of females that exceeds the estimated effect on the performance of males.

While these effects are small, and potentially resulting from a rational calculation in which students trade off higher grades in exchange for perceived-higher-quality leisure, our results do suggest that it may be important to consider other longer-term outcomes. In particular, given that our results provide suggestive evidence that the effect is persistent, there might be important impacts on subsequent labor-market outcomes. The literature's best evidence linking alcohol and labor market outcomes in the U.S. uses state-level aggregates (Dave and Kaestner 2001), survey data from the 1988 National Health Interview Survey (Mullahy and Sindelar 1996; Terza 2002), and from the National Longitudinal Survey of Youth (Renna 2009), where power is a challenge to identification. We see this as an important area for future research with a great need for improved sources of data.

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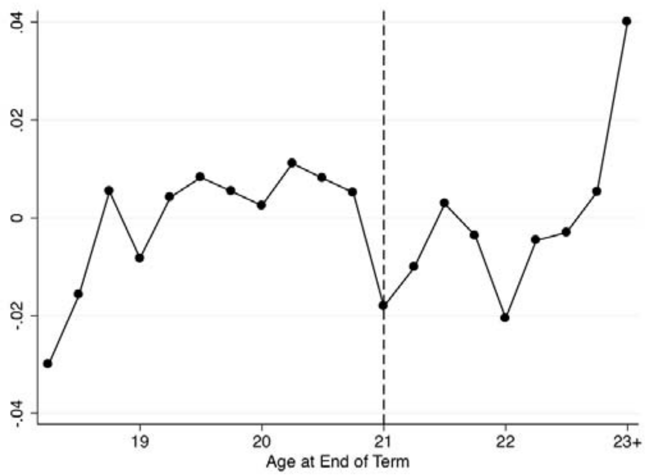
Figure 1
Normalized GPAs by Age
Adjusted for Individual, Accumulated Credits, and Course-type Fixed Effects



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.

Table 1
Summary Statistics

	Oregon (Sample)	Oregon (IPEDS)	Four-year Public U.S. Institutions (IPEDS)
SAT I Verbal 25th percentile score, incoming students	500	490	464
SAT I Verbal 75th percentile score, incoming students	620	610	568
SAT I Math 25th percentile score, incoming students	500	500	472
SAT I Math 75th percentile score, incoming students	620	610	578
Number of undergraduates	13,102	15,983	8,674
Fraction female	0.55	0.53	0.55
Fraction white	0.79	0.75	0.67
Fraction black	0.02	0.02	0.11
Fraction Hispanic	0.03	0.03	0.08
Fraction Asian	0.08	0.12	0.11
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 last 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 final column range from 352 to 653.

Table 2

RD-based Estimates of Legal Access to Alcohol (Turning 21) At The End of Term

Panel A: No Controls	(1) 240 days	(2) 240 days	(3) 210 days	(4) 210 days	(5) 180 days	(6) 180 days	(7) 150 days	(8) 120 days	(9) 100 days	(10) 80 days	(11) 80 days	(12) 60 days	(13) 40 days	(14) 20 days
Age $\geq$ 21	-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)
Panel B: Controls	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Age $\geq$ 21	-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)
Age Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Linear	Linear	Linear	None	None	None	None
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

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
RD-based Estimates of Turning 20 At The End of a Term

Panel A: No Controls	(1) 240 days	(2) 240 days	(3) 210 days	(4) 210 days	(5) 180 days	(6) 180 days	(7) 150 days	(8) 120 days	(9) 100 days	(10) 80 days	(11) 80 days	(12) 60 days	(13) 40 days	(14) 20 days
Age $\geq$ 20	-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)
Panel B: Controls	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Age $\geq$ 20	-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)
Age Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Linear	Linear	Linear	None	None	None	None
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

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 4
Estimated 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 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.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5
Estimated Effect of Legal Access to Alcohol on Course-Taking Behavior

	(1)	(2)	(3)	(4)
Dependent Variable: Expected Term GPA				
Age > 21 During Term	0.009***	0.134***	0.007**	0.009***
	(0.002)	(0.002)	(0.003)	(0.003)
Individual Fixed Effects	no	yes	yes	yes
Accumulated-Credits Fixed Effects	no	no	yes	yes
Term Fixed Effects	no	no	no	yes
Number of Students	13,102	13,102	13,102	13,102
Observations	146,730	146,730	146,730	146,730
Dependent Variable: Course Load				
Age > 21 During Term	-1.249***	-1.132***	0.085**	0.053
	(0.023)	(0.023)	(0.035)	(0.035)
Individual Fixed Effects	no	yes	yes	yes
Accumulated-Credits Fixed Effects	no	no	yes	yes
Term Fixed Effects	no	no	no	yes
Number of Students	13,102	13,102	13,102	13,102
Observations	146,730	146,730	146,730	146,730

Notes: Expected term GPA is based on the average grades in the previous offering of each course a student is taking in a given term. Course load is the number of credits taken in a term. Accumulated-credits fixed effects are fixed effects for a student's cumulative credits 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 6
Dynamic Effects of Legal Access to Alcohol

	(1)	(2)	(3)	(4)	(5)
Term of 21st birthday	-0.084***	-0.084***	-0.036***	-0.036***	
	(0.006)	(0.005)	(0.007)	(0.006)	
Turned 21 in final week of term					-0.010
					(0.014)
Turned 21 with 1-3 weeks remaining in term					-0.035***
					(0.011)
Turned 21 with 4-6 weeks remaining in term					-0.048***
					(0.011)
Turned 21 with 7-10 weeks remaining in term					-0.038***
					(0.009)
Turned 21 1 term ago	-0.092***	-0.085***	-0.027***	-0.030***	-0.030***
	(0.007)	(0.006)	(0.008)	(0.008)	(0.008)
Turned 21 2 terms ago	-0.099***	-0.088***	-0.021**	-0.026***	-0.026***
	(0.008)	(0.007)	(0.009)	(0.009)	(0.009)
Turned 21 3 terms ago	-0.126***	-0.098***	-0.023**	-0.031***	-0.031***
	(0.008)	(0.007)	(0.011)	(0.011)	(0.011)
Turned 21 4 terms ago	-0.152***	-0.126***	-0.044***	-0.055***	-0.055***
	(0.009)	(0.007)	(0.012)	(0.012)	(0.012)
Turned 21 5 terms ago	-0.166***	-0.115***	-0.024*	-0.038***	-0.039***
	(0.010)	(0.008)	(0.014)	(0.014)	(0.014)
Turned 21 6+ terms ago	-0.299***	-0.103***	0.002	-0.021	-0.021
	(0.012)	(0.009)	(0.017)	(0.017)	(0.017)
Individual Fixed Effects	no	yes	yes	yes	yes
Accumulated-Credits Fixed Effects	no	no	yes	yes	yes
Course-Specific Controls	no	no	no	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 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.

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

Table 7
Dynamic Effects of Legal Access to Alcohol

	(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 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.

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

Table 8
Heterogeneity Across Gender and Ability

	(1) Male All Abilities	(2) Female All Abilities	(3) Both Genders High Ability	(4) Both Genders Low Ability	(5) Male High Ability	(6) Male Low Ability	(7) Female High Ability	(8) 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

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 9
Heterogeneity Across Gender and Financial Aid Eligibility

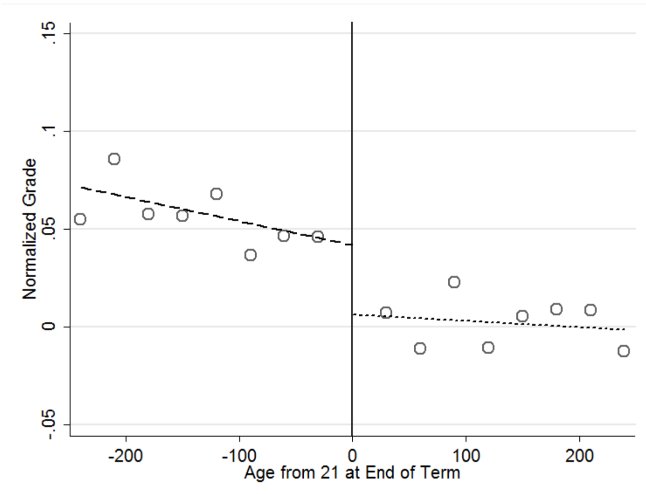
	(1) Both Genders All Eligibilities	(2) Both Genders High Eligibility	(3) Both Genders Low Eligibility	(4) Male High Eligibility	(5) Male Low Eligibility	(6) Female High Eligibility	(7) Female Low Eligibility
Age > 21 During Term	-0.042***	-0.051***	-0.040***	-0.045***	-0.015	-0.057***	-0.063***
	(0.007)	(0.010)	(0.010)	(0.017)	(0.015)	(0.013)	(0.013)
Individual Fixed Effects	yes	yes	yes	yes	yes	yes	yes
Accumulated-Credits Fixed Effects	yes	yes	yes	yes	yes	yes	yes
Course-Specific Controls	yes	yes	yes	yes	yes	yes	yes
Number of Students	9,113	4,556	4,557	1,887	2,013	2,669	2,544
Observations	335,915	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-eligibility group consists of students with eligibility above the sample median while the low-eligibility group consists of those with eligibility below the sample median.
* significant at 10%; ** significant at 5%; *** significant at 1%

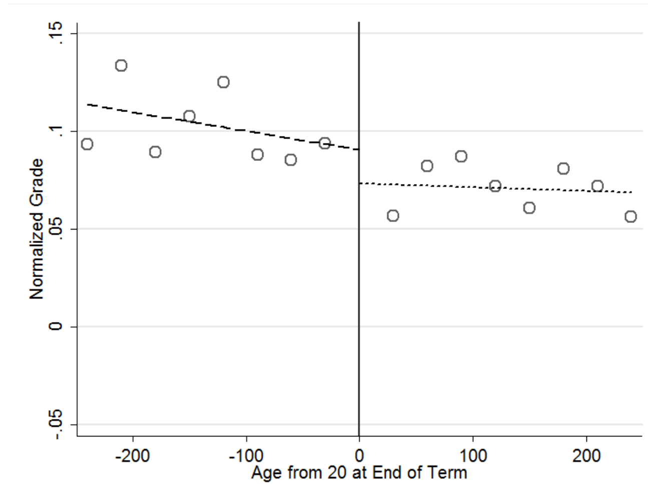
Appendix

Figure A1
Graphical Analysis of RD-Based Estimates

Panel A
Estimated Effect of Turning 21 At End of Term



Panel B
Estimated Effect of Turning 20 At End of Term



Notes: Each hollow circle corresponds to the mean within a thirty-day bin. The line is fitted using data 240 days on each side of the threshhold.

Table A1

RD-based Estimates of Turning 22 At The End of a Term

Panel A: No Controls	(1) 240 days	(2) 240 days	(3) 210 days	(4) 210 days	(5) 180 days	(6) 180 days	(7) 150 days	(8) 120 days	(9) 100 days	(10) 80 days	(11) 80 days	(12) 60 days	(13) 40 days	(14) 20 days
Age $\geq$ 22	-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)
Panel B: Controls	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Age $\geq$ 22	-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)
Age Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Linear	Linear	Linear	None	None	None	None
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, quarter-by-year-at-the-university 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 day-of-birth level.

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