The Minimum Legal Drinking Age and Morbidity in the US

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We provide the first evaluation of the effect of the US minimum legal drinking age (MLDA) on nonfatal injuries. Using administrative records from several states and a regression discontinuity approach, we document that inpatient hospital admissions and emergency department (ED) visits increase by 8.4 and 71.3 per 10,000 person-years, respectively, at age 21. These effects are due mainly to an increase in the rate at which young men experience accidental injuries, alcohol overdoses, and injuries inflicted by others. Our results suggest that the literature's disproportionate focus on mortality leads to a significant underestimation the benefits of tighter alcohol control.

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1. Introduction

A large body of research in economics and public health has examined the effects of policies restricting access to alcohol on mortality rates.[1] Evidence from a variety of quasi-experimental research designs has convincingly shown that reducing access to alcohol (and, by implication, reducing alcohol consumption) significantly reduces mortality risk due to motor vehicle and other types of accidents, homicide, suicide, and alcohol overdoses (Cook and Tauchen 1992, Cook et al. 2005, Dee 1999, Biderman et al. 2009, and others). This has been shown to be especially true for young adults (Carpenter and Dobkin 2009, Wagenaar and Toomey 2001, Birckmayer and Hemenway 1999, Carpenter 2004, and others).

There is far less evidence, however, on the extent to which easier access to alcohol and subsequent alcohol consumption affect the rate of nonfatal injuries.[2] This is a serious gap in the literature for two key reasons. First, unlike alcohol-related deaths, the majority of the costs of alcohol-related injuries (which can be large, particularly if the injury results in disability) are transferred to other people through the mechanism of insurance. Second, nonfatal injuries are far more common than deaths, particularly among young adults. For example, the overall

[1] Arguably the first to do so using modern quasi-experimental methods is Cook and Tauchen (1982), who demonstrate that alcohol tax increases reduce death rates from liver cirrhosis.

[2] Some epidemiologic research has focused on injuries from drunk driving accidents (see, for example, Hingson and Winter 2003), but mortality is by far the more commonly studied outcome related to alcohol consumption in general and drunk driving specifically.

death rate due to injuries for 20 year olds in the United States is 7 per 10,000 person-years. By comparison, the rates of health events severe enough to require an inpatient hospital stay or treatment in an Emergency Department (ED) are 341 and 3,951 per 10,000 person-years, respectively, in the samples included in this study. Moreover, public health and medical research suggests that about a third of emergency room visits due to nonfatal injuries are alcohol-related (SAMHSA 2006). These factors suggest that it is important to understand the relationship between alcohol access and nonfatal injuries.

What explains the substantial gap in our understanding of how much regulations targeting alcohol affect morbidity? Rehm et al. (2003) note that "information on alcohol-related morbidity ... is limited because studies with morbidity as the endpoint demand substantial resources to assess individual outcomes in an objective and standardized way." That is, data constraints have limited the ability of researchers to comprehensively study alcohol access and morbidity. For example, while panel data evaluations of the effects of changes in state alcohol policies (such as alcohol taxes, drinking ages, or drunk driving laws) on mortality are enabled by high quality annual data on deaths at the state level, there are far fewer high quality datasets on population rates of nonfatal injury that support these types of analyses.[3]

[3] For example, although several studies have used state experimentation with lower drinking ages in the 1970s and 1980s to study mortality rates (see, for example, Birckmayer and Hemenway

We are aware of only three economics studies that use quasi-experimental designs to evaluate the effects of stricter alcohol controls on nonfatal injuries (as opposed to mortality)[4]; all use variation in alcohol access induced by minimum drinking ages. Conover and Scrimgeour (2013) and Boes and Stillman (2013) study the effect of a policy change in New Zealand in 1999 that lowered its drinking age from 20 to 19. They both find that the policy increased alcohol-related hospitalizations using difference-in-differences approaches. Lindo et al. (2015, forthcoming) use a regression discontinuity design to examine the effects of the MLDA in New South Wales, Australia on hospitalizations (among other outcomes) and find it reduces admissions due to alcohol abuse and assault.[5]

Our study advances this literature in several key ways. First, to our knowledge we are the first study to directly examine the issue of alcohol control and morbidity in the United States using this MLDA-based quasi-experimental approach and high quality administrative data. While it is useful to understand related experiences of other countries such as Australia, New Zealand, and

1999, Carpenter 2004, and others), we are not aware of similar research that has examined nonfatal injury rates in this way.

[4] Note that we do not review here some related work in economics that uses quasi-experimental methods applied to survey (as opposed to administrative) data (e.g., Markowitz and Grossman 2000, 1998; Balsa et al. 2009). Similarly, some previous work linking movie violence and professional sports to violent outcomes that are likely related to the injuries we study here has suggested alcohol consumption as a possible mechanism (Dahl and DellaVigna 2009, Card and Dahl 2011).

[5] A series of recent studies in public health also use this design to study inpatient hospitalizations in Canada from 1997 to 2007 (Callaghan, Sanches, and Gatley 2013) and hospital-based treatment for alcohol-related conditions in Ontario from 2002 to 2007 (Callaghan et al. 2013). They find significant increases in alcohol use disorders and suicide events at the Canadian MLDA, as well as increases in injuries from motor vehicle accidents for males.

Canada, their age-specific mortality rates, youth drinking profiles, and alcohol control policies differ in important ways from those in the US. Focusing on the US setting is also important given active debates in the US about changing the drinking age (McCardell 2012, *The New York Times* 2015). Second, we also benefit from the fact that there is extensive evidence on the first stage relationship between the alcohol control policy we consider (MLDA in the US) and alcohol consumption.[6] This allows for more straightforward interpretation of our findings compared to other settings. Third, we examine both emergency department visits as well as hospital admissions; prior work in this area has focused mainly on hospital admissions. As referenced above, ED visits are an order of magnitude more common than inpatient admissions among young adults and are quite costly (over $1,500 in list charges per injury-related ED visit on average in our data). Understanding the determinants of ED admissions and what can be done to reduce them is also important in light of concerns over ED overcrowding (IOM 2004, Owens et al. 2010). Fourth, we comprehensively examine all causes of morbidity, not just injuries from motor vehicle accidents and alcohol overdoses—the main focus of prior work. This is important, as it allows us to assess whether the significant increases in alcohol-related causes of morbidity we find at the MLDA are simply outcomes that are being systematically recategorized at the MLDA

[6] Previous research shows that alcohol consumption increases sharply at age 21 in the US by 20-30 percent (Carpenter and Dobkin 2009, 2011; Crost and Guerrero 2012; SAMHSA 2009). That work also demonstrates that other observable characteristics such as employment, education, and demographics do not change discretely at the same threshold.

threshold. Finally, our very large samples of administrative ED and hospitalization data (over 3.7 million records) allow us to estimate how the effects of easier access to alcohol on morbidity vary by both cause and gender.

Our empirical approach follows much recent work in this area and leverages variation in access to alcohol induced by the MLDA (Carpenter and Dobkin 2009). We use a regression discontinuity design (RD) which takes advantage of the fact that access to alcohol and alcohol consumption increase discretely at age 21, the MLDA in the United States.[7] We use administrative records which include a near-census of emergency room visits and inpatient hospital stays from several large states. These records allow us to observe any medical event severe enough to require treatment in an emergency department or on an inpatient basis. The sample sizes are large enough and the records have sufficient medical and demographic information that by combining them with denominators from the census it is possible to precisely estimate age profiles for various types of health events. These age profiles allow us to determine how much illnesses and injury rates increase at exactly age 21 when people are no longer subject to the MLDA.

[7] This design has also been used recently to examine the effects of easier alcohol access on marijuana and tobacco consumption (Crost and Rees 2013, Crost and Guerrero 2012), academic outcomes of students at the United States Military Academy (Carrell, Hoekstra, and West 2011), and academic outcomes of students at the University of Oregon (Lindo, Swenson, and Waddell 2013).

We find that ED visits and hospital admissions both increase sharply at age 21 by 71.3 and 8.4 per 10,000 person-years, respectively. These increases are primarily due to accidents (including motor vehicle accidents), alcohol overdoses, and injuries inflicted by others. We also find that the effect of the MLDA on nonfatal injuries is larger for males than for females, and in many cases these gender differences are statistically significant. Overall our results are the first to comprehensively document the effects of easier alcohol access on morbidity in the United States using quasi-experimental methods and high quality administrative data and indicate that the costs of alcohol use by young adults are much larger than previously estimated (Bonnie and O'Connell 2004). As we discuss below, even when compared against the very large mortality costs of alcohol access, the estimated external costs of morbidity are substantial.

The remainder of the paper proceeds as follows. Section 2 describes our data sources and discusses our empirical methods. Section 3 presents the main results, and Section 4 offers a discussion and concludes.

2. Data and Methods

We use administrative data comprising a near-universe of visits to emergency rooms in three states (Arizona 2005-2009, New Jersey 2004-2010, and Wisconsin 2004-2010) and inpatient hospital admissions in four states (Arizona

1990-2010, New York 1993-2008, Texas 1999-2003, and Wisconsin 2004-2010).[8] These records include a detailed description of the cause of the ED visit or hospital stay and the treatment the patient received. In addition the records include month and year of both birth and start of treatment which allows us to estimate the patient's age in months.

We classify causes of ED visits and hospital admissions into three mutually exclusive categories: alcohol intoxication, injuries, and a residual category we denote as 'illness' (which strictly speaking is everything other than alcohol intoxication and injuries). In addition to being an outcome of independent interest, alcohol intoxication rates also proxy for very heavy alcohol consumption. Injuries are also likely to be directly affected by the MLDA given previous work on mortality; we separate injuries into three mutually exclusive categories: self-inflicted injuries (e.g., attempted suicides), injuries deliberately inflicted by others, and accidental injuries (including motor vehicle accidents, falls, etc.). A single ED visit or hospitalization can have more than one cause. To create the mutually exclusive categories we give alcohol intoxication, when it is coded as the primary cause of admission, precedence over injuries. We then code injuries

[8] These records come from the Healthcare Cost and Utilization Project (HCUP) State Inpatient Databases (SID) and State Emergency Department Databases (SEDD). Our choice of states derives in part from the fact that there are no discrete changes in insurance coverage at age 21 in the states we study. Data exist for other states as well, but in some states young adults "age out" of Medicaid eligibility at age 21, and indeed in those states there are discontinuous changes in the presence and sources of health insurance coverage at age 21. Since previous research has shown a direct causal effect of insurance coverage on medical care use (including emergency room visits and hospital admissions – see, for example, Anderson, Dobkin, and Gross 2012, 2014), we do not use data from those states.

with a precedence order of injured by other, self-inflicted injury, and accidental injury.[9] Any visit/admission that does not fall into one of these groups is categorized as due to illness.[10]

Throughout our main analyses, we exclude pregnancy-related hospital admissions, as in this age range (19-22) pregnancy represents over half of all female hospital admissions. For symmetry in presenting results, we also exclude the very small number of pregnancy-related ED visits. This has very little effect on our results (especially in the analyses of ED visits), which is not surprising given that alcohol use in the third trimester of pregnancy is rare.[11]

To create the age profiles we compute a count of the number of people that experience an ED visit or hospital stay at a particular age in months (such as between their 21st birthday and their 21st birthday + 1 month). We convert the counts of ED visits and hospital stays into rates per 10,000 person years by dividing them by age-specific population estimates from the census. We then plot these age profiles to assess visually whether there is a discrete change in morbidity rates at age 21.

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[9] There are very few records that have more than one of these three types of injuries coded.

[10] Each category includes the following ICD codes: Alcohol intoxication: primary ICD-9 first three digits '291' or '303' or first four digits '3050'; Injury inflicted by other: E code first two digits '96'; Injury self-inflicted: E code first two digits '95'; Accidental injury: any E code not starting with '95' or '96'. Any admission or visit that does not fit into one of the categories above is coded as due to illness unless there is a mention of alcohol intoxication on one of the secondary ICD codes in which case it is added to the alcohol intoxication category.

[11] Results that include the pregnancy-related ED visits and hospital admissions are very similar and are available upon request.

To determine if an increase in the morbidity rate visible in a figure is statistically significant we use the morbidity event rates for the 24 months before and after the 21st birthday (i.e., from age 19 to 22) to estimate the following model:

$$(1) \qquad Y_a = \alpha_0 + \alpha_1 Z_a + \alpha_2 A_a + \alpha_3 A_a^2 + \alpha_4 Z_a A_a + \alpha_5 Z_a A_a^2 + \alpha_6 B_a + v_a.$$

In this model Y_a is the rate of ED visits or hospital stays per 10,000 person years for the one month age cell a, A_a is age re-centered at 21, and Z_a is an indicator variable that takes on a value of 1 for age greater than or equal to 21. The variable B_a is an indicator variable that takes on a value of 1 for the month in which the 21st birthday falls and 0 otherwise and is intended to absorb the pronounced celebration effects we observe for some outcomes such as ED visits due to alcohol intoxication.[12] The parameter of interest is α_1 which captures the discrete increase in the morbidity rate that occurs at age 21. If none of the other determinants of the ED visit and hospitalization rates are changing discretely at 21 and the second order polynomial is sufficiently flexible to absorb age-related changes, then the discrete increase in morbidity, α_1, can be interpreted as the effect of the MLDA.[13]

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[12] This also lets us avoid any attenuation bias that results from not knowing if a person with an inpatient stay or ED visit in the month that they turn 21 is over or under 21.

[13] In prior work we have documented that there are no other major discrete changes in factors that are likely to affect injury rates (Carpenter and Dobkin 2009, 2015).

3. Results

a. Descriptive Statistics

In Table 1 we present descriptive statistics for the ED visits and inpatient hospital stays in our sample. Among young adults in the age range we examine (19-22, inclusive), the rate of ED visits and inpatient hospital admissions is 3,951 and 341 per 10,000, respectively. The table reveals that the list charges for the ED visits and the hospital stays are substantial: average list charges for inpatient admissions are $15,567 while ED visits for this age group have average list charges of $1,593.[14] Finally, Table 1 shows that a significant portion of these costs are transferred directly others (i.e., not borne privately) through the mechanism of insurance.[15]

b. ED Visits

Figure 1 presents the overall age profile of non pregnancy-related ED visits per 10,000 person-years separately for two groups of causes: alcohol intoxication and injuries (combined) in the bottom series and the residual illness category in the top series. The lines on top of each series are fitted estimates from a regression of the ED visit rate on a second order polynomial in age interacted with a dummy variable for being over 21 and a dummy for the month in which the 21^{st} birthday falls (equation 1). Figure 1 shows a sharp increase in ED visits due

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[14] Insurers have negotiated discounts on the list prices. The median list prices are substantially lower as the distribution of list charges, particularly for hospitalizations, has a very long right tail.
[15] Moreover, hospitals are often unable to collect payment from uninsured individuals.

to alcohol intoxication and injuries at age 21. This increase is apparent even ignoring the very high rate of ED visits in the month in which people's 21st birthday falls.[16] In contrast, Figure 1 shows no visual evidence of an increase in ED visits for illness.

We further separate out the external cause categories in Figure 2. Because ED visits due to accidental injuries are much more common than intentional injuries (both self-inflicted and inflicted by others) and alcohol intoxication visits, we present the age profiles using different scales. The most striking series in Figure 2 is for alcohol intoxication (dark diamonds) which is scaled on the right axis and shows clear evidence of a sharp increase at the MLDA, as well as very strong evidence of celebration effects at all birthdays with particularly large effects on the 21st birthday. The other series that shows strong evidence of an increase at age 21 is for ED visits due to injuries deliberately inflicted by another person. Finally, the series for accidental injury-related ED visits shows some evidence of a small increase at the MLDA. Appendices 1 and 2 present the same age profiles as in Figure 2 separately for males and females, respectively. These appendices show clearly that the increase in ED visits for injuries deliberately inflicted by others at age 21 is driven entirely by males, while the increases in ED visits for accidental injuries and alcohol intoxication are observed for both

[16] An examination of the subsample of our data for which we have exact dates reveals the increase in the birth month is driven largely by people celebrating their 21st birthday. We account for these 'celebration effects' by including a dummy variable for the month which includes the 21st birthday, so as to estimate the more persistent effects of the MLDA.

genders. In all four figures comparisons of the fitted values from equation 1 with the corresponding rates suggests that the second order polynomial included in equation 1 is sufficiently flexible to fit the age profiles of the various outcomes.

We present the regression estimates corresponding to Figures 1 and 2 and Appendices 1 and 2 in Table 2. The dependent variable in the regression is the number of ED visits per 10,000 person-years occurring X months before or after the individual's 21st birthday, and we estimate equation 1 over the 48 months for people ages 19 to 22, inclusive.[17] The regressions are weighted to account for the very slight differences in the size of the underlying populations across age cells.[18] In the tables to save space we only present our estimate of α_1 from model 1 and its standard error along with the estimate of the constant. Because the age variable has been re-centered at 21, α_1 provides an estimate of the discrete increase in the morbidity rate at age 21 and the constant provides an estimate of the rate for people just under 21.

The results in the top, middle, and bottom panels of Table 2 correspond to results for the full sample, males, and females, respectively, and provide point estimates of the increases in morbidity rates visible in the age profiles. Specifically, we find that ED visits increase by a statistically significant 71.3 per

[17] This is the same as the bandwidth used in all the figures. For most outcomes the bandwidth selection procedure proposed in Imbens and Kalyanaraman (2012) yields optimal bandwidths between 1.5 and 2.5; Appendices 3 and 4 confirm our results are not very sensitive to bandwidth choice.

[18] We report heteroskedastic consistent standard errors.

10,000 at age 21. The ED visit rate for young adults just below the MLDA is 3,974, suggesting a 1.8 percent effect. In column 2 we confirm that, as we would expect, the increase in visits due to illness (i.e., not alcohol intoxication or injuries) is small and statistically insignificant. As seen in Figure 1, we confirm in column 3 of Table 2 that there is a statistically significant increase in ED visits due to injuries or alcohol intoxication at the MLDA of 57.8 visits per 10,000. When we break the result in column 3 out into its component causes, we find statistically significant increases at the MLDA for ED visits due to alcohol intoxication (31.8% effect), accidental injuries (2.7% effect), and injuries deliberately inflicted by others (11.3% effect). We find no evidence of statistically significant increases in self-inflicted injuries. As can be seen in Appendices 3 and 4 which present regression estimates and confidence intervals for a range of bandwidths, these estimates are robust to bandwidth choice.

Disaggregating the results by gender in the middle and bottom panels of Table 2 confirms that, as observed in Appendices 1 and 2, the increases in ED visits at the MLDA for accidental injuries and alcohol intoxication are economically and statistically significant for both males and females. In contrast, however, the increase in ED visits at age 21 for injuries deliberately inflicted by others is only statistically significant for males. In the bottom row of Table 2 we report the p-values of the gender differences in the estimated effects of the MLDA on ED visits confirming that the increase in deliberately inflicted injuries is larger for men.

c. Inpatient Hospital Admissions

We next examine morbidity outcomes severe enough to result in an overnight hospital stay. We present the age profiles of hospital admission rates in Figures 3 and 4. Figure 3 shows evidence of an increase in hospital admissions due to alcohol intoxication or injuries and no change in admissions due to illness. Figure 4 shows that the increase in Figure 3 is driven by admissions due to alcohol intoxication, unintentional injuries, and injuries deliberately inflicted by another person. Disaggregating the results by gender in Appendices 5 and 6 for males and females, respectively, shows stronger evidence of discontinuous increases in hospital admissions for alcohol intoxication, injuries inflicted by others and accidental injuries for males and very little evidence of any meaningful changes for females. Overall the patterns for hospital admissions are qualitatively similar to those for ED visits.

We present the regression estimates corresponding to Figures 3 and 4 and Appendices 5 and 6 in Table 3. The dependent variable in the regression is the number of hospital admissions per 10,000 person-years occurring X months before or after the individual's 21st birthday, and the empirical approach is the same as the one implemented in the analysis of the emergency department stays.[19] The results in Table 3 confirm the visual patterns in Figures 3 and 4. Specifically in the top panel of Table 3, corresponding to Figure 3, we estimate that hospital admissions for alcohol intoxication or injury increase significantly by 6.4 admissions per 10,000 person-years at the MLDA, and there is no statistically significant increase in admissions coded as due to illness. When we separately consider the causes of injury-related admissions, the rightmost panels of Table 3 reveal that accidental injuries drive the majority of the discontinuous increase in injury-related hospital admissions. Disaggregating the results by gender in the middle and bottom rows of Table 3 reveals that the MLDA has a much larger effect on the injury rates of men. The estimated increase at 21 for alcohol intoxication, accidental injury, and injuries deliberately inflicted by another person are all sizable and statistically significant. In contrast, we find no significant increases in hospital admissions at the MLDA for women for any cause. The bottom row of Table 3 confirms that the gender differences in the effects of the MLDA on inpatient admissions due to accidental injury and alcohol intoxication (i.e., larger for men) are statistically significant.

d. Representativeness of the Analysis Sample

[19] As with emergency department visits, we find that for most outcomes the bandwidth selection procedure proposed in Imbens and Kalyanaraman (2012) yields optimal bandwidths between 1.5 and 2.5; Appendices 7 and 8 confirm our results on inpatient hospital stays are not very sensitive to bandwidth.

The sample of states included in the emergency department and inpatient analyses, though geographically diverse, only includes 7 percent and 18 percent of the United States population, respectively. Although the regression discontinuity design returns internally valid estimates of the effect of the MLDA on emergency department visits and hospitalizations, one may be concerned that these estimates do not generalize well to the rest of the country.

We evaluate this issue by determining how similar young adults from the states in the analysis samples are to young adults in the US population. The most important dimensions are alcohol consumption patterns and access to and utilization of the health care system. The first is important because it is the primary mechanism through which the law acts; the second is important because we only observe people that receive medical treatment, and the probability of treatment is sensitive to barriers to care such as lack of health insurance (Anderson, Dobkin, and Gross 2012, 2014). We also examine baseline health and demographic characteristics. We do this using the sample of young adults surveyed between 2004 and 2010 as part of the Centers for Disease Control's Behavioral Risk Factor Surveillance System (BRFSS). The BRFSS is a large telephone survey that is designed to be representative at the state level and that asks questions about alcohol use, health insurance, access to health care, health outcomes, and demographics.[20]

In Table 4 we separately report the characteristics for the entire United States, young adults living in states included in the ED visits analysis (Arizona, New Jersey, and Wisconsin), and young adults residing in states included in the inpatient hospital stays analysis (Arizona, New York, Texas, and Wisconsin). For each characteristic we also report the p-value of the difference in means between the relevant subsample and the US as a whole.

Comparing the first and second columns of Table 2 reveals that young adults from states included in the analysis of emergency department admissions have slightly higher alcohol consumption levels than the overall population before they turn 21 and a larger increase after turning 21.[21] In addition, they are more likely to have health insurance and a regular doctor. The higher levels of alcohol consumption and greater access to the health system in the emergency department sample suggest that estimates of the effect of the MLDA on ED visits in our sample are likely to be slightly larger than those we would expect overall for young adults in the United States.

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[20] For the access to care outcomes, we use questions from BRFSS that ask respondents whether they have any kind of health care coverage; whether they have one person they think of as their personal doctor or health care provider; and whether there was a time in the past 12 months when they needed to see a doctor but could not due to cost.
[21] We cannot precisely estimate the change at age 21 in alcohol consumption due to small samples of young adults from our study states.

Comparing the first and fourth columns of Table 4 shows that young adults in states included in the inpatient sample have slightly higher alcohol consumption rates than the overall population before turning 21 and a slightly smaller increase after turning 21. They also have lower rates of health insurance coverage, and there is some evidence that they are more likely to have foregone care due to cost. These differences suggest that estimates of the effect of the MLDA on inpatient hospital stays in our sample are likely to be slightly smaller than what we would find were it possible to directly estimate the effect for all young adults.[22]

4. Discussion and Conclusion

Previous research using quasi-experimental designs to examine the adverse health consequences of alcohol use has focused primarily on mortality due in large part to the lack of high quality data on morbidity rates. We fill this gap in the literature by examining a near-census of ED visits and inpatient hospital admissions from several large states. We leverage the discontinuous change in alcohol availability associated with turning 21 in the United States to estimate the effect of easier alcohol access on morbidity. We find statistically significant increases in ED visits and inpatient hospital admissions at the MLDA

[22] There is also a substantial difference in the percent Hispanic due to the inclusion of Texas in the inpatient sample. The estimates of the effect of the MLDA on the hospitalization rate without the inclusion of Texas are very similar to the results with Texas and are available on request.

of about 71.3 and 8.4 per 10,000 person-years, respectively. These effects are clearly visible in the age profiles and are driven primarily by alcohol overdoses, injuries inflicted by others, and accidental injuries. To our knowledge these are the first quasi-experimental estimates of the impact of the MLDA on emergency department visits and the first estimates of its effects on nonfatal injuries in the United States.

Of course, our study is not without limitations. One concern is that young adults who are under the drinking age may choose not to go to the hospital when they are injured for fear that people (e.g., parents, police) might find out they were drinking alcohol illegally. Given the severity of the injuries we study (particularly for inpatient stays), such behavior seems unlikely, though if present it could create a discontinuous increase at the MLDA in the number of people seeking treatment even in the absence of a true increase in the morbidity rate. In that case we would expect a compositional change that would result in a sharp decline in list charges at the MLDA (Conover and Scrimgeour 2013). Appendix 9 reports the estimated discontinuities at the MLDA in list charges for various categories of ED visits and inpatient stays. We find a small, statistically significant reduction in list charges for alcohol-related ED visits at the MLDA of about 6 percent but no significant change in list charges overall for ED visits and no significant change in list charges for alcohol-related inpatient stays. Taken together and in the context of the extremely large and statistically significant increase in morbidity rates for alcohol-related ED visits, these results are suggestive evidence of a small degree of systematic avoidance behavior for ED visits but not for inpatient stays.

Another limitation of our study is that it does not provide a direct estimate of the effect of lowering the drinking age. There are a couple of reasons for caution in using the estimates of the sharp change in injury rates at the MLDA to predict the effect of lowering the drinking age. The first is that a higher drinking age may not significantly lower total lifetime alcohol consumption but instead might simply shift the timing of drinking to after individuals pass the MLDA threshold. If this is the case, and if the risk associated with drinking does not change much with age, a higher MLDA would primarily increase the age at which people are injured by a few years rather than lower the lifetime probability of an injury due to alcohol consumption. A careful examination of the age profiles of self-reported drinking from surveys in both Canada and the United States shows evidence of a gradual increase in drinking frequency in the two years before people are allowed to drink legally, followed by a sharp increase in alcohol consumption at the MLDA that persists above the pre-MLDA trajectory for several years (Carpenter et al. 2014 forthcoming, Carpenter and Dobkin 2009, 2015). This pattern is inconsistent with the MLDA just changing the timing of alcohol consumption and suggests that a higher drinking age substantially reduces lifetime alcohol consumption rather than just displacing it.

A second reason the estimates from the paper might not give good predictions of the effect of lowering the MLDA is that it is possible that people learn to drink safely very quickly. If this is the case and the rate at which people learn to drink safely does not vary much with age, a higher drinking age will primarily delay rather than prevent injuries and deaths. If this is occurring for a particular type of injury we would expect to see an age profile with a large increase in the injury rate among people just over the MLDA who are in the 'learning to drink safely' phase that fairly quickly returns to the rate we would expect based on projections from the pre MLDA age profile. An examination of Figure 3 reveals what appears to be a persistent increase in the hospitalization rate after the MLDA suggesting that people are not rapidly learning to avoid the type of drinking and behavior that results in serious injury. On the other hand the age profiles in Figure 2 suggest that the probability of accidental and deliberately inflicted injuries that result in Emergency Department visits return to baseline fairly quickly. This suggests that predictions of the effect of lowering the MLDA based on the regression discontinuity estimates should be about right for the inpatient admissions but will be too high for the Emergency Department visits.[23]

[23] An additional concern is that the effect size may depend on the age at which the MLDA is set if factors such as environment or impulse control change with age, as psychological and brain research suggests (Steinberg 2004, 2007). A comparison of the effect of the MLDA on mortality rates in the US with its effect in Canada suggest that these factors, if large, are canceling each other out. In the US at age 21 there is a 9 percent increase in the overall death rate and a 15 percent increase in motor vehicle fatalities. In contrast in Canada the increase at the MLDA (18 or 19, depending on the province) is 6 percent overall and 17 percent for motor vehicle fatalities. Neither of these differences is statistically significant.

Despite these limitations, our results are important for several reasons. First, our finding that there is a significant discontinuous increase at age 21 in the likelihood of needing treatment for an injury inflicted by another person provides one of the literature's most direct pieces of evidence that allowing individuals to purchase and consume alcohol increases the probability they will become the victim of violence – an idea that has received considerable anecdotal support but relatively little empirical validation using a quasi-experimental approach. Of course, it is possible that the party that ends up in the hospital is also the party that instigated the violent event.

More importantly, these results are the first to comprehensively evaluate the effect of the MLDA on all the major causes of morbidity in the US. We estimate that the external cost of the increase in injures is substantial even relative to the external cost of the increase in mortality documented in the literature. Consider that prior work found total deaths increase by about 0.8 per 10,000 person-years at the MLDA largely due to an increase in deaths from motor vehicle accidents, suicide, and alcohol intoxication (Carpenter and Dobkin 2009). Given the research design the increase in deaths is entirely among people that have been drinking, and those costs – while tragic – should not be considered externalities. For the suicides and alcohol overdoses the person drinking is very unlikely to have killed anyone else. However, estimates from the Fatal Accident Reporting System suggest that for every 21 year old that dies while driving drunk, in expectation an additional 0.24 people are killed that are in another car, on foot, or riding a bicycle.[24] Given that the increase in the death rate from motor vehicle accidents at age 21 is 0.365 per 10,000 person years we estimate that there is an increase of 0.088 (0.365*.24) deaths per 10,000 person years at age 21 that reflect external costs.

Though the cost of a non-fatal health event is much smaller than the values commonly used in policy-related valuations of a death, they are much more common, and the majority of the direct health cost of nonfatal injuries is transferred to other people through the mechanism of insurance. We estimate that ED visits increase by 71.3 per 10,000 person years and hospital admissions increase by about 8.4 per 10,000 person years at the MLDA. Given commonly used estimates of the value of a statistical life from Viscusi and Aldy (2003) (approximately $7.94 million measured in US dollars in 2005, which is in the middle of the sample period for our morbidity data) and appropriately deflated average costs of an injury-related hospital admission ($7,000) or ED visit ($500), our estimates imply that the non-fatal injuries due to the MLDA impose an externality cost on society that is approximately 1/7th to 1/8th the size of the

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[24] To calculate the relevant proportion of total MLDA deaths that should properly be counted as external, we examined the 3,006 accidents in the Fatal Accident Reporting System (FARS) involving a drunk 21 year old where alcohol involvement is determined by either a BAC of .08 or greater (the legal limit in the US) or by a police officer reporting that the driver had been drinking. These accidents resulted in 2,029 deaths of drunk 21 year old drivers. These accidents also resulted in the deaths of 484 sober drivers, their passengers, pedestrians or cyclists. These calculations suggest that on average for each age-21 drunk driver who dies in a fatal car accident an additional .239 (484/2029) people die who are not in the drunk driver's car.

associated MLDA-related external cost of mortality.[25] Moreover, the costs of ED visits and hospitalizations likely significantly understate the total costs of the events because alcohol-related injuries may require expensive rehabilitation and in many cases—at least in the short term—reduce productivity. Taken together, our results suggest that the literature's disproportionate focus on mortality misses a significant part of the benefits of stricter alcohol control and thus the case for government intervention in this setting is stronger than previously understood.

[25] To see this, note that the average list charges for an inpatient hospital stay and ED visit in our data for injuries are approximately $21,000 and $1,500, respectively (Table 1). We deflate each of these by one third using the 2006 average statewide cost-to-charge ratios for the states in our sample (Federal Register 2006). While we recognize that the fraction of list charges that is actually recovered by hospitals varies greatly by payer, the point of the current exercise is to gauge relative orders of magnitude. As such, reasonable alternative choices about how to deflate list charges are unlikely to change the basic argument. Given these cost estimates, the external mortality costs are approximately 0.088 deaths * $7.94 million = $698,720 per 10,000 person years. Calculating for ED visits yields: 71.3 * $500 = $35,650 per 10,000 person years; calculating for inpatient stays yields: 8.4 * $7,000 = $58,800 per 10,000 person years. Thus, we estimate that the costs of ED visits and inpatient stays as a proportion of external mortality costs are: [($35,650 + $58,800) / $698,720] = 0.135, or between 1/7th and 1/8th.

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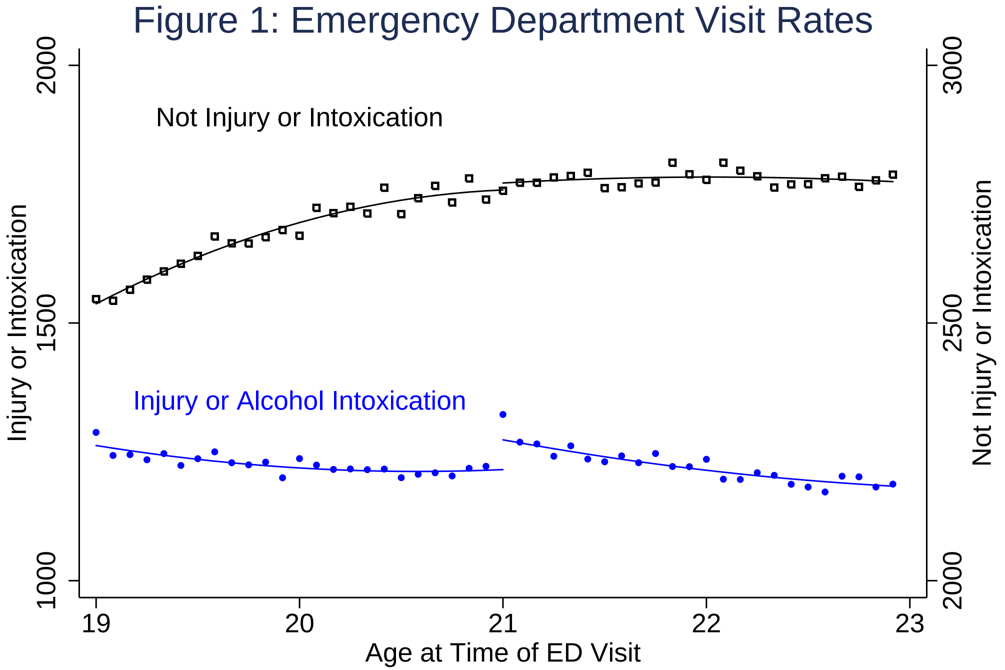
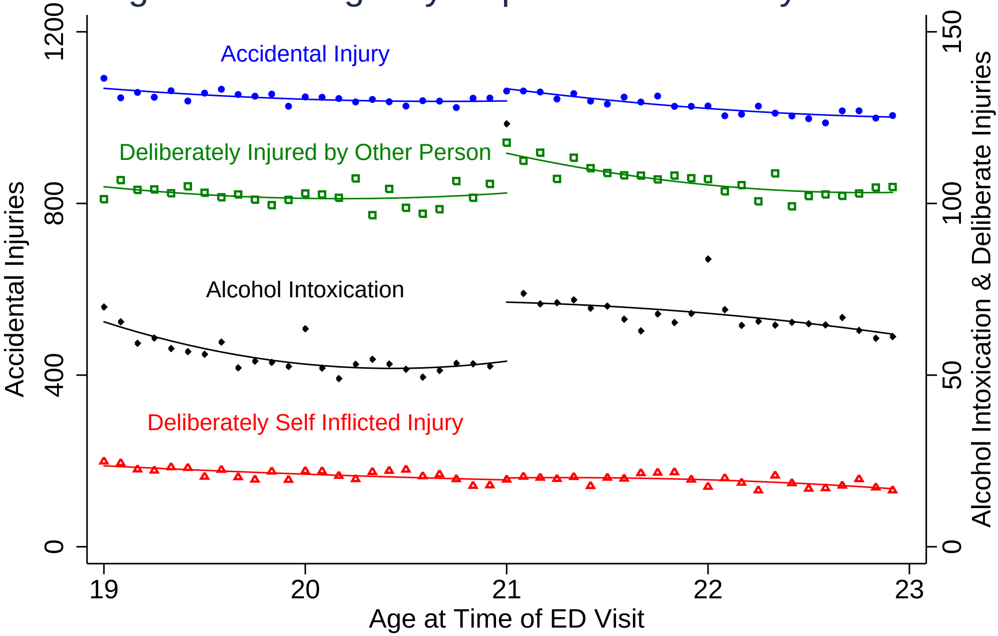


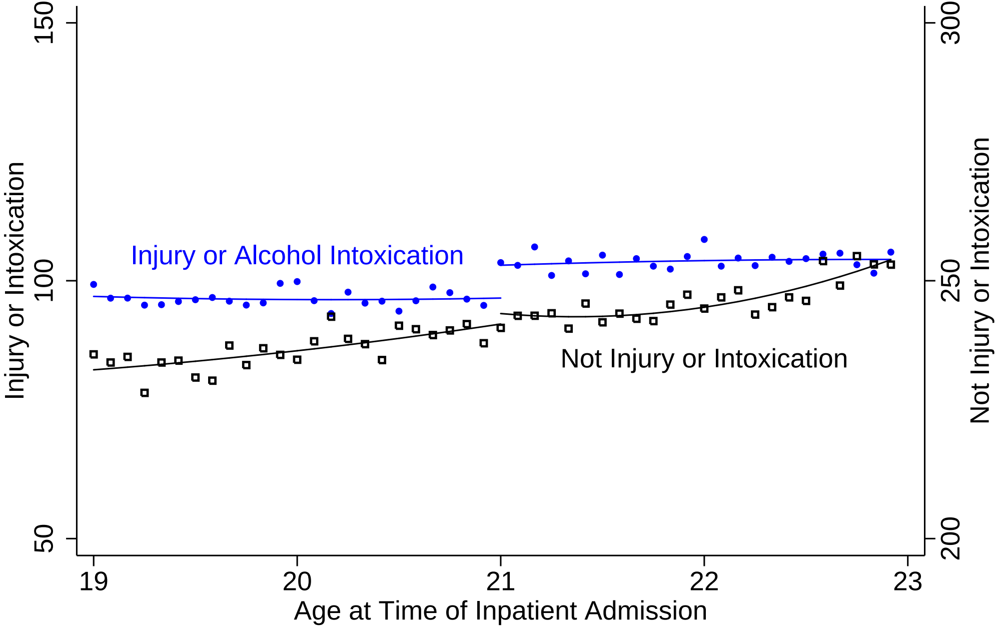
Note: The points are ED visit rates per 10,000 and the fitted lines are from a second order quadractic polynomial in age estimated seperately on either side of the threshold.

Figure 2: Emergency Department Visits by Cause



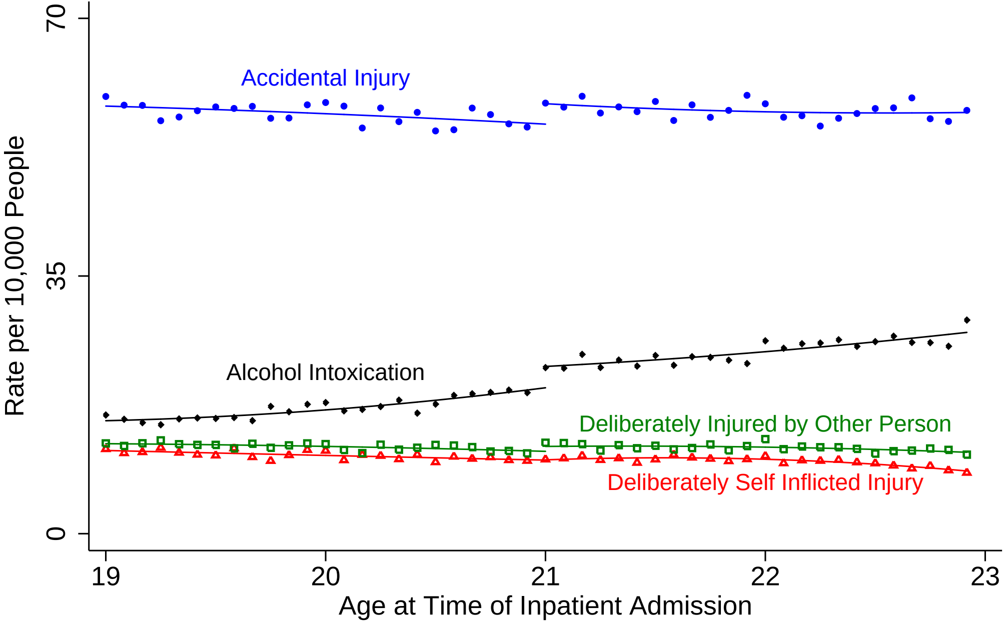
Note: The points are ED visit rates per 10,000 and the fitted lines are from a second order quadractic polynomial in age estimated seperately on either side of the threshold.

Figure 3: Inpatient Admission Rates



Note: The points are inpatient admission rates per 10,000 and the fitted lines are from a second order quadractic polynomial in age estimated seperately on either side of the threshold.

Figure 4: Inpatient Admissions by Cause



Note: The points are inpatient admission rates per 10,000 and the fitted lines are from a second order quadractic polynomial in age estimated seperately on either side of the threshold.

Table 1: Descriptive Statistics for Emergency Department and Inpatient Samples

	Emergency Department Visits				Inpatient Hospital Admissions			
Group	All Visits	Illness	Injury	Alcohol	All Visits	Illness	Injury	Alcohol
Percent Female	57.0	64.0	41.6	37.7	48.4	54.5	34.4	30.5
List Charges	1,593	1,597	1,561	2,033	15,567	14,053	21,367	10,839
Insurance Type								
Private	39.8	38.8	42.5	37.3	39.4	40.0	41.2	25.6
Medicaid	19.6	22.4	13.5	11.3	33.3	34.6	26.4	44.6
Self Pay	32.8	33.3	30.7	47.0	16.8	15.3	20.2	20.6
Other	7.8	5.6	13.2	4.4	10.5	10.1	12.2	9.1
Rate per 10K	3,951	2,727	1,162	63	341	241	79	21
Number of Records	2,729,392	1,883,492	802,577	43,323	1,040,875	735,549	241,702	63,624

Notes: The Emergency Department sample includes a near census of visits for the following states Arizona (2005-2009), New Jersey (2004-2010) and Wisconsin (2004-2010). The Inpatient Sample includes a near census from the following states Arizona (1990-2009), New York (1993-2009), Texas (1999-2003) and Wisconsin (2009-2010)

Table 2: Emergency Department Visits

	All Visits	Illness	Injury or Alcohol	Alcohol	Accidental Injury	Self Inflicted Injury	Injury Inflicted by Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All							
Over 21	71.3	13.5	57.8	17.2	28.4	0.6	11.6
	(17.6)	(14.9)	(8.9)	(2.3)	(8.3)	(1.2)	(2.8)
Constant	3,973.8	2,758.2	1,215.6	54.1	1,039.0	19.5	103.1
	(16.1)	(13.8)	(6.6)	(1.3)	(7.1)	(1.1)	(2.1)
Observations	48	48	48	48	48	48	48
R-squared	0.927	0.961	0.845	0.914	0.781	0.602	0.697
Male							
Over 21	78.2	15.0	63.2	16.0	24.9	2.1	20.2
	(21.3)	(13.3)	(13.3)	(4.0)	(12.1)	(1.2)	(4.3)
Constant	3,314.8	1,921.1	1,393.7	69.4	1,178.4	17.0	129.0
	(13.5)	(6.9)	(8.9)	(2.3)	(8.2)	(1.0)	(3.4)
Observations	48	48	48	48	48	48	48
R-squared	0.917	0.963	0.793	0.891	0.749	0.347	0.642
Female							
Over 21	64.1	12.0	52.0	18.5	32.1	-1.1	2.5
	(31.3)	(31.7)	(13.1)	(2.1)	(12.2)	(1.7)	(2.3)
Constant	4,670.2	3,642.9	1,027.3	37.8	891.7	22.1	75.7
	(29.5)	(27.6)	(8.8)	(1.6)	(8.5)	(1.5)	(1.8)
Observations	48	48	48	48	48	48	48
R-squared	0.792	0.863	0.726	0.902	0.578	0.636	0.287
P-value of Gender Difference	0.7106	0.9325	0.5501	0.5798	0.6751	0.1373	0.0005

Notes: See notes from Table 1. The dependent variable is Emergency Department visits per 10000 persons. An observation is the admission rate for a one month age range. The regressions include a second order polynomial fully interacted with an indicator variable for over 21 and a dummy for the month the 21st birthday falls in. The regressions include people age 19 to 22 inclusive which is equivalent to a bandwidth of two years. For most outcomes the bandwidth selection proposed in Imbens and Kalyanaraman 2012 gives optimal bandwidths between 1.5 and 2.5. Estimates are weighted by population and the heteroskedastic consistent standard errors are presented in parentheses.

Table 3: Inpatient Hospital Admissions

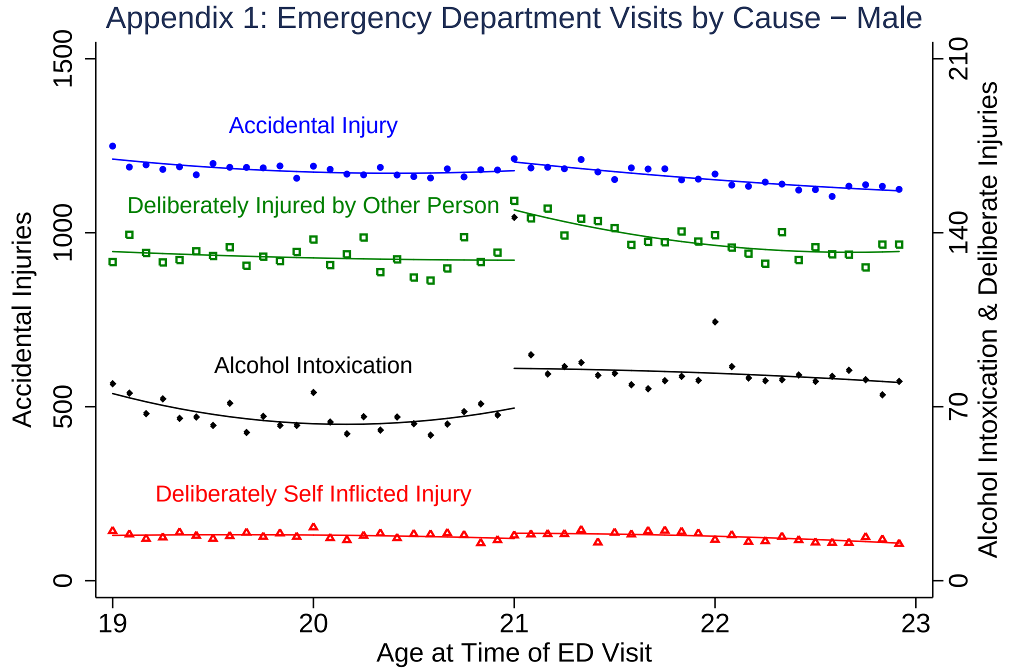
	All Visits	Illness	Injury or Alcohol	Alcohol	Accidental Injury	Self Inflicted Injury	Injury Inflicted by Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All							
Over 21	8.4	2.0	6.4	2.9	2.8	0.0	0.7
	(2.7)	(1.8)	(1.8)	(0.7)	(0.9)	(0.3)	(0.4)
Constant	338.3	241.6	96.6	19.8	55.6	10.0	11.2
	(2.2)	(1.6)	(1.0)	(0.3)	(0.6)	(0.2)	(0.2)
Observations	48	48	48	48	48	48	48
R-squared	0.923	0.865	0.838	0.970	0.261	0.756	0.422
Male							
Over 21	13.3	3.1	10.3	4.8	3.9	0.4	1.2
	(3.1)	(2.0)	(2.1)	(1.1)	(0.9)	(0.4)	(0.6)
Constant	331.0	211.0	120.0	23.9	69.3	7.5	19.3
	(2.0)	(1.8)	(1.2)	(0.6)	(0.7)	(0.3)	(0.3)
Observations	48	48	48	48	48	48	48
R-squared	0.892	0.721	0.861	0.966	0.331	0.334	0.463
Female							
Over 21	2.4	1.1	1.3	0.4	1.0	-0.3	0.2
	(3.4)	(2.7)	(2.1)	(0.8)	(1.4)	(0.4)	(0.4)
Constant	330.2	265.7	64.5	11.4	39.4	11.5	2.2
	(3.0)	(2.1)	(1.5)	(0.6)	(0.9)	(0.3)	(0.3)
Observations	48	48	48	48	48	48	48
R-squared	0.871	0.868	0.432	0.898	0.207	0.777	0.138
P-value of Gender Difference	0.0197	0.5677	0.0032	0.0016	0.0829	0.2721	0.1594

Notes: See notes from Table 1. The dependent variable is hospital admissions per 10000 persons. An observation is the admission rate for a one month age range. The regressions include a second order polynomial fully interacted with an indicator variable for over 21 and a dummy for the month the 21st birthday falls in. The regressions include people age 19 to 22 inclusive which is equivalent to a bandwidth of two years. For most outcomes the bandwidth selection proposed in Imbens and Kalyanaraman 2012 gives optimal bandwidths between 1.5 and 2.5. Estimates are weighted by population and the heteroskedastic consistent standard errors are presented in parentheses.

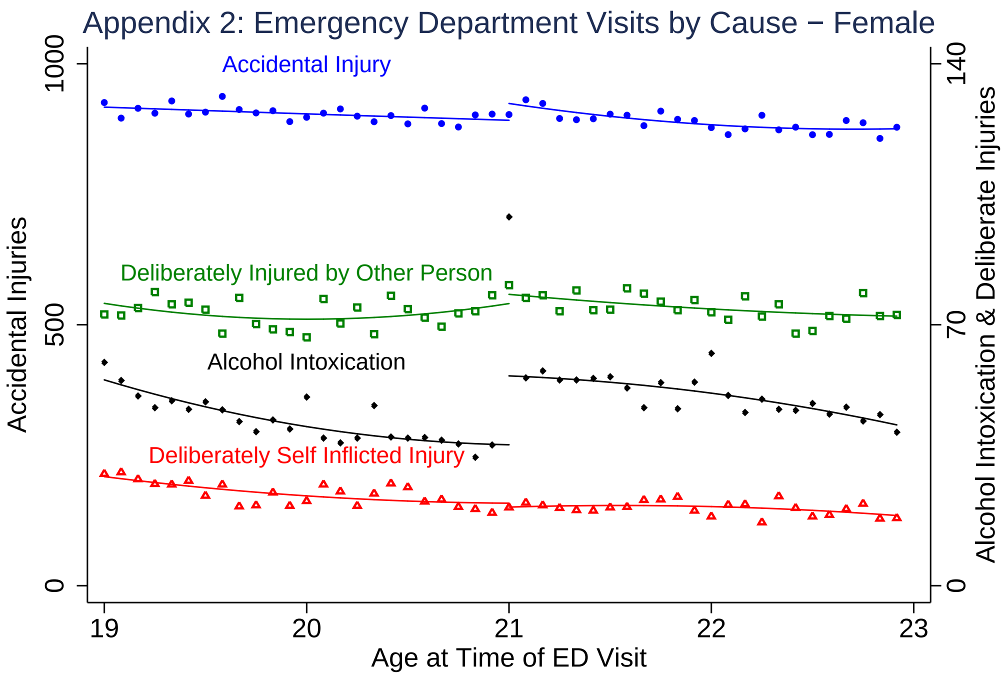
Table 4: Characteristics of 19-22 Year Olds in States Included in the Study

	All States	ED Sample		Inpatient Sample	
	Mean	Mean	P-value	Mean	P-value
	(1)	(2)	(3)	(4)	(5)
Reported Drinking Age 19-20					
Drank in Last 30 Days	41.9	45.2	0.097	44.1	0.124
Days Drinking in Last 30	2.4	2.7	0.126	2.6	0.151
Days Binge Drinking in Last 30	0.9	1.2	0.087	1.1	0.101
Reported Drinking Age 21-22					
Drank in Last 30 Days	63.2	68.0	0.017	60.2	0.022
Days Drinking in Last 30	4.3	4.8	0.078	4.0	0.037
Days Binge Drinking in Last 30	1.4	1.8	0.015	1.4	0.633
Access to Health Care					
Health Insurance	68.4	72.0	0.005	64.9	0.000
Has a Doctor	62.2	67.4	0.000	61.3	0.385
Did Not Get Care Due to Cost	18.7	17.1	0.119	20.0	0.098
Demographics					
White	57.1	61.4	0.002	49.2	0.000
Black	10.9	7.0	0.000	9.0	0.001
Hispanic	22.2	20.4	0.151	31.1	0.000
Some High School	10.0	8.1	0.075	11.8	0.007
High School Graduate or GED	36.9	35.8	0.387	36.5	0.641
Some College	50.6	53.8	0.025	48.5	0.034
Working	50.4	48.7	0.233	48.2	0.027
Observations	56,014	2,784		4,195	

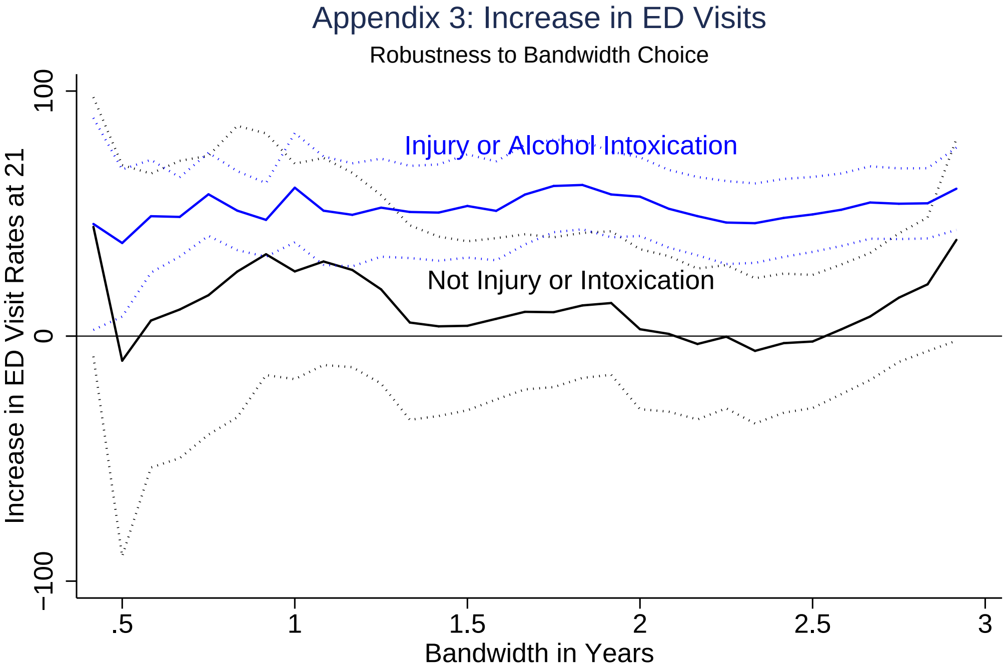
Notes: The descriptive statistics are from the Behavioral Risk Factor Surveillance System 2004 - 2010 and are for 19 to 22 year olds. The Emergency Department Sample includes Arizona, New Jersey and Wisconsin. The Inpatient Sample includes Arizona, New York, Texas and Wisconsin. The P-values are from a comparison of 19 to 22 year olds in the subsample with those not in the subsample. Focusing on just 19-20 year olds or 21-22 year olds splits the sample approximately in half.



Note: The points are ED visit rates per 10,000 and the fitted lines are from a second order quadractic polynomial in age estimated seperately on either side of the threshold.



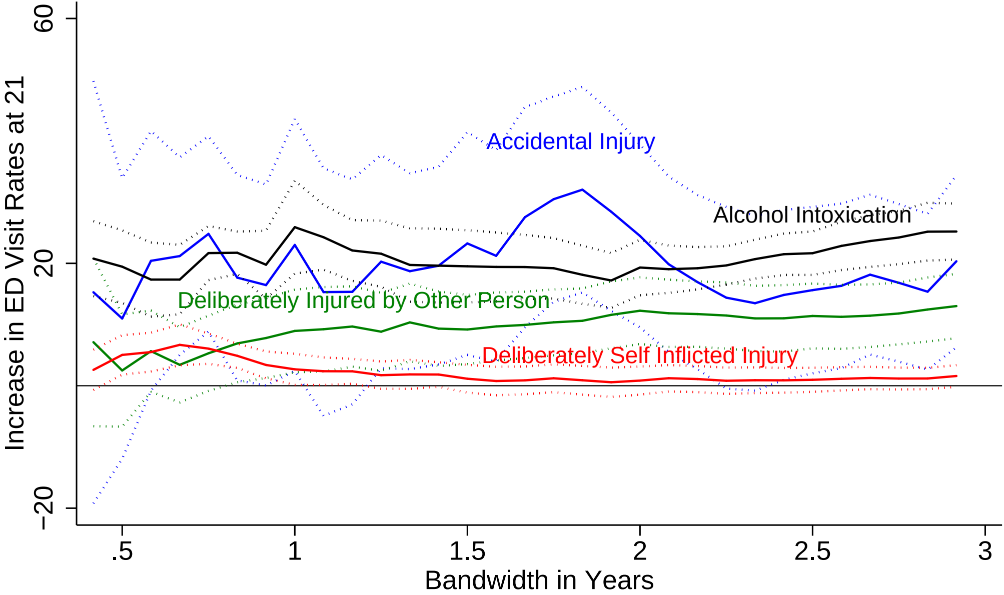
Note: The points are ED visit rates per 10,000 and the fitted lines are from a second order quadractic polynomial in age estimated seperately on either side of the threshold.



Note: The estimates of the increase at age 21 are from a second order quadractic polynomial fully interacted with an indicator variable for being over 21. The heavy line is the point estimate; the dotted line is the 95% confidence interval.

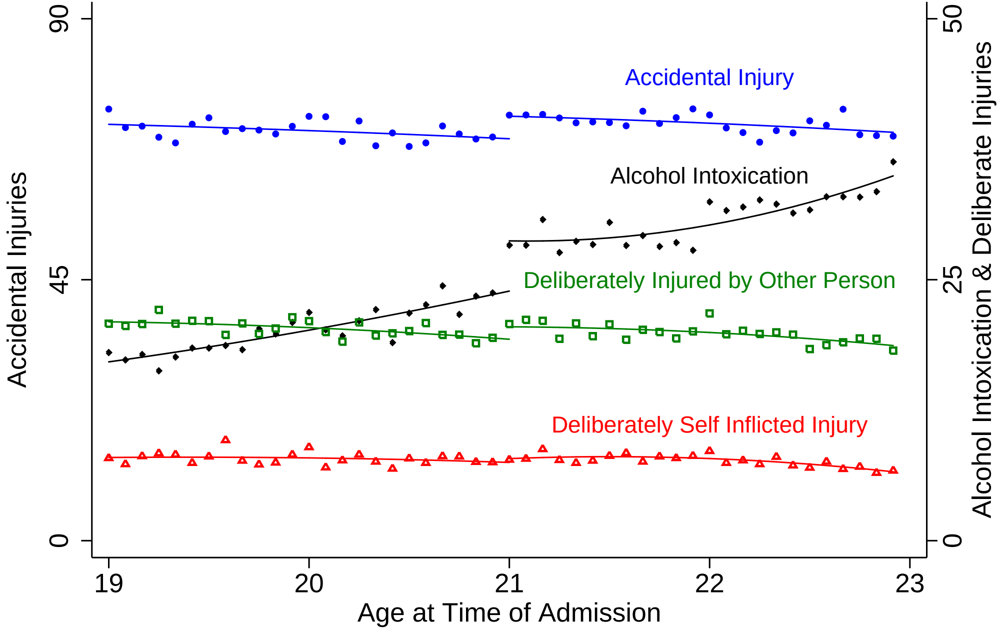
Appendix 4: Increase in ED Visits

Robustness to Bandwidth Choice



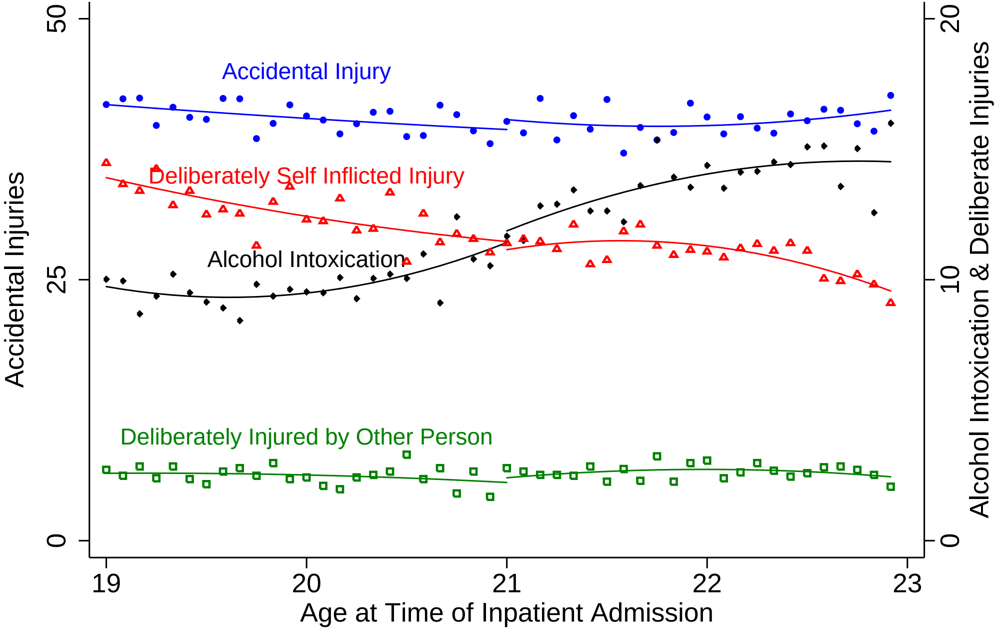
Note: The estimates of the increase at age 21 are from a second order quadractic polynomial fully interacted with an indicator variable for being over 21. The heavy line is the point estimate; the dotted line is the 95% confidence interval.

Appendix 5: Inpatient Admissions by Cause − Male



Note: The points are inpatient admission rates per 10,000 and the fitted lines are from a second order quadractic polynomial in age estimated seperately on either side of the threshold.

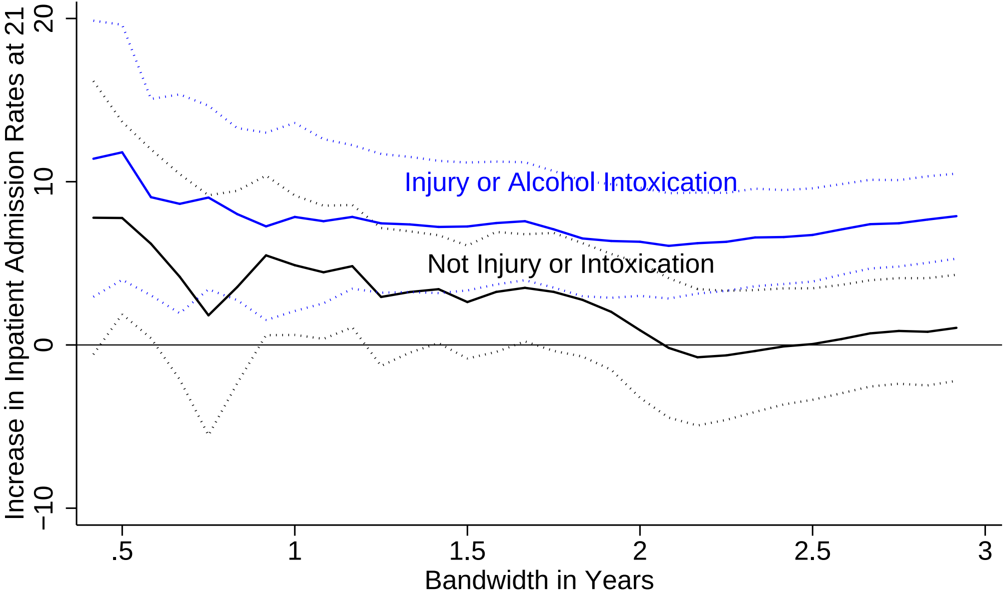
Appendix 6: Inpatient Admissions by Cause − Female



Note: The points are inpatient admission rates per 10,000 and the fitted lines are from a second order quadractic polynomial in age estimated seperately on either side of the threshold.

Appendix 7: Increase in Inpatient Admissions

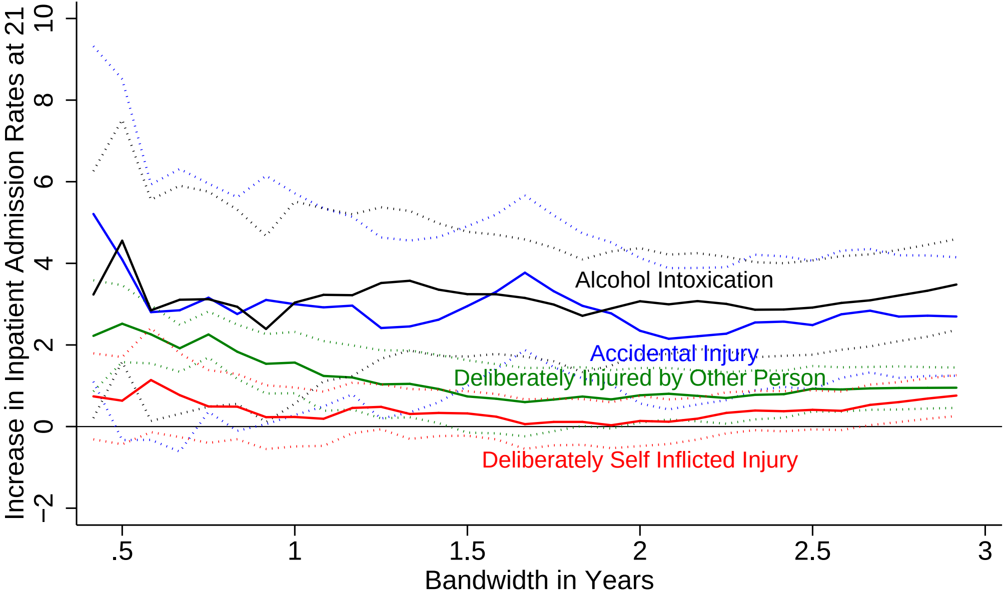
Robustness to Bandwidth Choice



Note: The estimates of the increase at age 21 are from a second order quadractic polynomial fully interacted with an indicator variable for being over 21. The heavy line is the point estimate; the dotted line is the 95% confidence interval.

Appendix 8: Increase in Inpatient Admissions

Robustness to Bandwidth Choice



Note: The estimates of the increase at age 21 are from a second order quadractic polynomial fully interacted with an indicator variable for being over 21. The heavy line is the point estimate; the dotted line is the 95% confidence interval.

Appendix 9: Estimated Effect of the MLDA on List Charges

	ED visits (1)	Inpatient stays (2)
All	10.2	187.5
	(8.8)	(190.4)
Illness	-3.2	104.0
	(9.2)	(140.0)
Injury or alcohol	39.6	165.7
	(16.6)	(671.3)
Alcohol	-132.3	-374.4
	(67.1)	(534.7)
Accidental injury	21.3	662.3
	(17.1)	(1036.5)
Self-inflicted injury	197.5	321.5
	(94.6)	(771.5)
Injury inflicted by other	95.9	-404.8
	(56.0)	(947.3)

Notes: See notes from Table 1. The dependent variable is average list charges for the ED and inpatient stays samples, respectively. An observation is the average list charge for a one month age range. The regressions include a second order polynomial fully interacted with an indicator variable for over 21 and a dummy for the month the 21st birthday falls in. The regressions include people age 19 to22 inclusive. Estimates are weighted by population and the heteroskedastic consistent standard errors are presented in parentheses.