Journal of Health Economics 57 (2018) 277–289

Contents lists available at ScienceDirect

Journal of Health Economics

journal homepage: www.elsevier.com/locate/econbase

Legal access to alcohol and criminality☆

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ARTICLE INFO

Article history:
Received 10 May 2016
Received in revised form 29 July 2017
Accepted 1 August 2017
Available online 14 August 2017

JEL classification:
K4
I1
D8

Keywords:
Alcohol
Crime
Minimum legal drinking age

ABSTRACT

Previous research has found strong evidence that legal access to alcohol is associated with sizable increases in criminality. We revisit this relationship using the census of judicial records on criminal charges filed in Oregon Courts, the ability to separately track crimes involving firearms, and to track individuals over time. We find that crime increases at age 21, with increases mostly due to assaults that lack premeditation, and alcohol-related nuisance crimes. We find no evident increases in rape or robbery. Among those with no prior criminal records, increases in crime are 50% larger—still larger for the most socially costly crimes of assault and drunk driving.

1. Introduction

Recent research has found evidence of significant social costs associated with legal access to alcohol (Carpenter, 2004; Carpenter and Dobkin, 2009, 2010; Anderson et al., 2016). We revisit the relationship between alcohol and crime utilizing the universe of criminal charges filed in Oregon courts from 1990 through 2012. We follow an approach similar to Carpenter and Dobkin (2015), exploiting the discontinuous change in legal access to alcohol that occurs at age 21. Our estimates suggests that criminality increases sharply at age 21, and that the increase is driven by lesser assaults (e.g., those not involving weapons), including drunk driving and other alcohol-related crimes.

Carpenter and Dobkin (2015) also find that arrests increase substantially at age 21. However, individuals in California can also legally purchase handguns upon turning 21, which introduces a potential confounder that may bias estimates of the effect of alcohol that are identified off of the age-21 discontinuity. The sign and magnitude of any such bias is an empirical question, of course, determined by how often criminals use legally purchased handguns to commit crime and whether they are induced into acquiring firearms and/or criminality upon turning 21. The potential for bias is considerable, however, as handguns are used in 41% of robberies and 21% of assaults. Prior research on firearms also suggests that increases in the prevalence of guns can be associated with increases in robbery, suicide, and other types of violent crime (Duggan, 2001; Cook and Ludwig, 2006; Lang, 2016). Unlike in California, legal access to handguns in Oregon occurs at age 18, which leaves the prevalence of firearms smooth at age 21. With no such confounder, our analysis has the potential to contribute to corroborating earlier analysis.

Alcohol's disproportionate presence in crime and its possible causal link have been long debated (Lipsey et al., 2002). Long-term alcohol abuse is associated with neurobehavioral deficits, depression, and deteriorating interpersonal relationships (Oscar-Berman and Marinković, 2007), and while these consequences are consistent with increased criminality (Anderson et al., 2015; Raine et al., 1996), they might not be relevant for those initiating or increasing alcohol consumption with legal access to alcohol at age 21. That said, utilizing regression-discontinuity models, we offer evidence that overall crime increases with the legal availability of alcohol. Such would be consistent with recent experimental research among social drinkers—their ages were centered around the MLDA—which finds that alcohol consumption increases risk-

☆ We thank Michael Anderson, Jillian Carr, Jeff Duncan, and seminar participants at Purdue University for valuable comments.
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https://doi.org/10.1016/j.jhealeco.2017.08.001
0167-6296/

taking propensity, and reduces the cognitive ability to make choices in response to potential rewards or costs (George et al., 2005).[1]

Our estimates also support the majority of findings of Carpenter and Dobkin (2015), both in sign and magnitude, as we also find increases in assault and drunk driving at age 21. We find no evidence that weapons-related assaults increase, and no evidence that robbery or rape increase with the onset of legal access to alcohol.[2] In this instance, adopting a similar econometric approach, but using a different measure of crime (charges instead of arrests), exploiting crime-commission dates relative to turning 21 instead of arrest date relative to 21, and introducing new data to the literature (Oregon instead of California), we find strikingly similar estimates. This speaks to both the internal and external validity one could take away from the joint estimates of the two studies.[3]

In addition, our setting allows us to further identify potential mechanisms, as Oregon's assault statutes are coded specifically with regard to the presence of weapons. We find that increases in assaults are driven by lesser assaults—those not involving weapons—and no evidence that weapons-related assaults increase, and no evidence that robbery or rape increase with the onset of legal access to alcohol. With the universe of Oregon court charges, we are also able to link individuals over time, conditional on not exiting the state. This richness enables an examination of the heterogenous responses of individuals with varying criminal histories. This proves important, as we show that the increase in criminality around the advent of legal alcohol is 50% larger among individuals who turn 21 without having already collected a criminal record. We do not observe alcohol consumption, and thus cannot demonstrate first-stage responses to the minimum legal drinking age (MLDA). However, that we find significant increases in drunk driving among those with prior convictions suggests that even if the first stage of alcohol consumption is smaller among those with prior criminal convictions, these individuals still experience a sizable increase in alcohol consumption. Given that younger individuals exhibit more myopia and less self-control (Lee and McCrary, 2009), this also highlights the potential costs of lowering the MLDA. Furthermore, it provides evidence that legal access to alcohol is increasing crime through individuals who had not yet committed criminal acts, rather than increasing the criminality of those with a history of violating the law. In as much as this first-time exposure to the legal system could increase future criminality through negative criminogenic effects, these sorts of path-dependent outcomes also suggest that RD-based estimates would yield lower-bound estimates of the true, longer-run effect.

In Section 2 we describe our data and methods, which we follow with a discussion of results in Section 3. As part of this discussion, we exploit the panel structure of our data to inform our understanding of mechanisms and, in the end, motivate a richer understanding of behavior and the potential for policy to influence outcomes. In so doing, we also introduce new evidence regarding the nature of interactions between perpetrators and police around the onset of legal alcohol consumption. In Section 4 we offer concluding remarks.

2. Data and methods

For this study we utilize administrative records on the universe of charges filed in Oregon courts during the 1990–2012 window, maintained in the Oregon Judicial Information Network. These administrative data are similar to the arrest records used in Carpenter and Dobkin (2015). An important distinction, however, is that a dataset of charges both discards arrests for which prosecutors found insufficient grounds to warrant prosecution, and includes additional charges that often go unobserved in arrest records. For instance, resisting arrest or assaulting a police officer may not motivate the arrest (and hence can be absent in arrest records), but would be recorded in charges. Our main empirical models closely follow Carpenter and Dobkin (2015), utilizing regression discontinuity models that allow for quadratic age effects and a bandwidth of two years on each side of the age-21 threshold. Our regressions include all criminal charges brought on individuals between the ages of 19 and 23, which we will explore through bandwidth sensitivity analyses following the presentation of results. (We have also estimated Poisson count-data models in which we obtained near-identical estimates, both in magnitude and statistical significance.) Our economic specification is as follows:

$$Charges_a = \alpha + f(Age_a) + \beta 1(Age_a \geq 21) + f(Age_a)1(Age_a \geq 21) + \gamma_a + \epsilon_a \quad (1)$$

where $Charges_a$ represents the number of charges per 10,000-person-years for the one-month age cell a.[4] Age_a is the administrative record of the individual's age at the time of arrest. An age of 21 or older implies that, at that time of arrest, the individual would have been able to access alcohol legally. Thus, the parameter of interest in (1) is $\hat{\beta}$, which captures the discrete increase in the adverse event rate that occurs at age 21. If none of the other determinants of $Charges_a$ are changing discretely at 21 (and the polynomial $f(\cdot)$ does not underfit the age profile) then the discrete increase in adverse events picked up in the model by $\hat{\beta}$ can be interpreted as the effect of the MLDA.

If none of the other determinants of $Charges_a$ are changing discretely at 21 (and the polynomial $f(\cdot)$ does not underfit the age profile) then the discrete increase in adverse events picked up in the model by $\hat{\beta}$ can be interpreted as the effect of the MLDA. However, research in criminology on age gradients and sentencing suggests that the likelihood of incarceration is relatively flat at age 21, while sentence length increases (Doerner and Demuth, 2010; Steffensmeier et al., 1998). This would suggest that if treatment of crime is changing at age 21, it would be changing in a way that biases our estimates towards zero. Bushway and Piehl (2007) note that most of the criminology literature does not simultaneously examine age and prior criminality. When they examine sentencing patterns, they find that punishment severity declines with age once controlling for prior convictions, albeit in a smooth fashion. Regardless of the connection between age and prior criminality, there is little evidence in the criminology literature of a jump in sanctions coincident with age 21. It is also possible the lawyers might treat individuals differently depending on their age relative to 21. However, when we examine final dispositions for charges, we find that case dismissals are essentially unchanged at age 21. The point estimate (0.005) is small relative to an average dismissal probability of 0.40, and although we fail to reject the null that the point estimate is zero, the stand error is precise enough (0.004) that we can

[1] Likewise, research focusing specifically on younger drivers suggests that alcohol decreases executive function and increases errors (Raine et al., 1996).

[2] This is the only outcome for which our findings differ from the Carpenter and Dobkin (2015) results. However, as their point estimates are within the confidence interval around ours, one should not infer that the two results are inconsistent.

[3] This reproduction is noteworthy, as recent attempts to replicate studies in economics have often failed. Indeed, Camerer et al. (2016) found only 11 of 18 experimental studies published in *The American Economic Review* and *The Quarterly Journal of Economics* could be reproduced, while Chang and Li (2015) found only 29 of 59 of econometric studies that that used observational data could be replicated, even with the authors' original data and coding.

[4] In accounting for rates of criminality, we use state population estimates available through the Surveillance, Epidemiology, and End Results (SEER) Program of the National Cancer Institute.

Table 1
Summary statistics, by category.

Panel A: Overall crime rates per 10,000				
Total	Violent	Property	Alcohol	Drug
1797.3	180.4	361.1	183.2	134.2
Panel B: Violent crimes rates per 10,000				
Murder	Robbery	Rape	Assault Weapon	Assault No weapon
7.2	29.8	20.4	22.3	100.8
Panel C: Property crimes rates per 10,000				
Burglary	Theft	Trespassing	Mischief	Vehicle theft
50.9	137.2	45.6	127.4	39.9
Panel D: Alcohol crimes rates per 10,000				
DUI	Reckless Driving	Disorderly	Providing alc. to minors	Drinking alc. in a park
112.7	35.5	19.6	13.3	2.1
Panel E: Drug crimes rates per 10,000				
Schedule I-IV	Marijuana	Cocaine	Heroin	Meth
9.6	7.6	3.5	5.6	14.2

Notes: This table contains summary statistics for criminal charges per 10,000 person-years, by category, for the ages just below the 21 cutoff, in the interval [20.5, 21).

Table 2
The effect of attaining MLDA on incidence of criminal charge rates, by category.

	All crime (1)	Violent crime (2)	Alcohol crime (3)	Property crime (4)	Drug crime (5)
$1(Age \geq 21)$	220.8***	9.824	112.6***	43.29***	4.296
	(29.55)	(6.270)	(12.42)	(9.060)	(3.449)
Semi-elasticity	0.118	0.054	0.517	0.104	0.032

Notes: This table contains estimates of the change in charge rates per 10,000 associated with legal access to alcohol at age 21. All models are estimated assuming a quadratic polynomial and bandwidth of two years. Estimated standard errors (robust) are reported in parentheses.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

be confident that dismissal probabilities are not changing in any economically meaningful way in either direction.

3. Results

We begin with a presentation of summary statistics, in Table 1. Generally, more-severe crimes are less common. However, for alcohol-related crimes, drunk driving is both the most common and the most-severe crime. We come to this problem, however, with the benefit of existing literature having established the potential for important heterogeneity across types of crime. Thus, we will proceed quickly to models that allow for heterogeneity across crime type, after demonstrating a systematic increase in overall crime coincident with the minimum legal drinking age of 21 (see Panel A of Fig. 1) and facilitating inference by quantifying the discontinuity in overall rates of crime coincident with age 21 (see Table 2). We find an 11.8% increase in overall crime when individuals obtain legal access to alcohol; an increase in the total number of monthly charges of 2221 per 10,000 person years.[5]

[5] We estimate semi-elasticities using the predicted value of the outcome. This naturally creates semi-elasticities which are a mid-point semi-elasticity of using either the predicted values of the outcome to the left or right of the discontinuity to scale the estimated coefficient.

3.1. Crime-specific responses to alcohol availability

In this section, we explore the potential changes in rates of specific crimes around the MLDA of 21. We largely affirm that the empirical regularities in Carpenter and Dobkin (2015) are not likely suffering from the confounding effects of handgun availability that are inseparable from the effects of MLDA in California data.

In Panel B of Fig. 1 we separately allow for MLDA-induced discontinuities in crime across various broad categories: violent crime, property crime, drug crime, and alcohol-related crime. Doing so makes evident that increases in crime coincident with age 21 are only in property and alcohol-related nuisance crimes. Rates of violent crime do not increase with alcohol availability—which is not surprising, to the extent one anticipates that there are fewer individuals at the margin of committing violent crimes. This pattern is evident more generally, actually; any measured responsiveness at higher levels of aggregation is typically being driven by the "less-serious" crimes within that aggregate.

In Fig. 2 we further disaggregate broad measure of criminality into contributory crimes. Among violent crimes, this disaggregation again reveals that the lesser of crimes (i.e., assault) is responsive to legal access to alcohol, while the more serious crimes (e.g., robbery, rape) are not. This is also in keeping with the expectation that alcohol plays less of a role in crimes for which premeditation is possibly more acute. Carpenter and Dobkin (2015) find small but significant

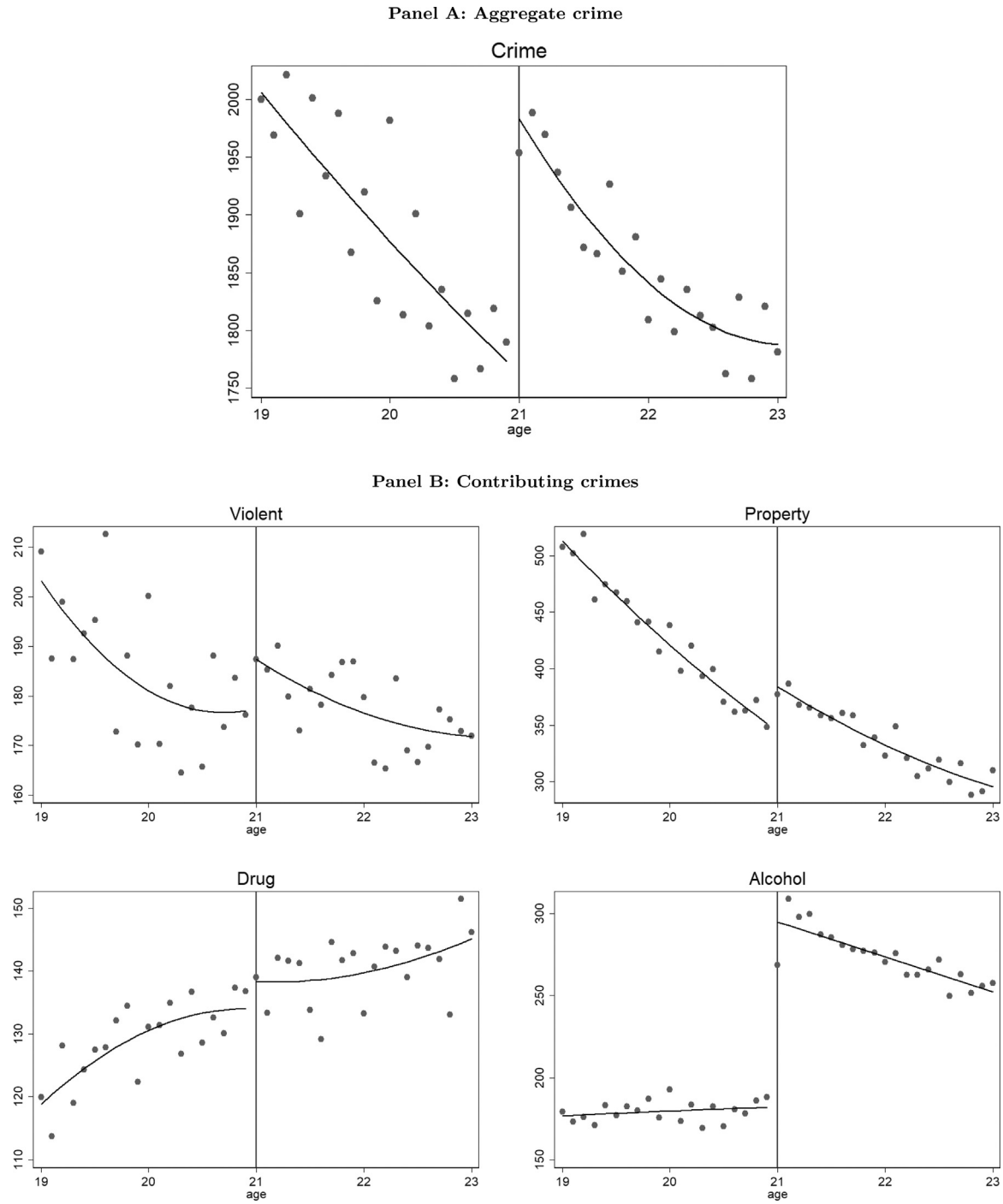

Fig. 1. Crime across the MLDA threshold. *Notes:* Charges per 10,000 person-years filed in Oregon courts in 1990 through 2012 based on the Oregon Judicial Information Network.

increases in robbery, of a magnitude that falls within the confidence interval around our estimates.[6]

With our additional flexibility allowing for the plotting of the categories of assault, Fig. 2 also reveals that the broader increases in overall rates of assault appear to be exclusive to those for which there are no weapons involved in the commission of the crime (i.e., in Oregon, "Assault 3" and "Assault 4"). The associated econometric results regarding violent crimes are reported in Table 3, which suggestive of a 21% increase in Assault 3 and an 8% increase in Assault 4. Conditional on an assault, we find that alcohol availability is seemingly mattering more where crimes are of less severity and potentially less premeditated.

Likewise for property crimes, criminal mischief is most responsive in the absolute sense, while it is the 2nd most common crime

[6] For the rarer crimes, we are less likely to find statistically significant evidence of responses even using the universe of charges over 22 years. For instance, the estimated standard error on murder would suggest would have only found statistical significance if murders had increased by 43.1%. From ex-ante perspective, the minimal detectable effect (an effect where we would expect find significance at the 95% level 80% of the time) is a 61.6% increase. Given the standard error around the point estimate on robbery, ex post we would have found a statistically significant estimate if robberies had increased by 21.5%, and ex ante the minimal detectable effect is 30.8%.

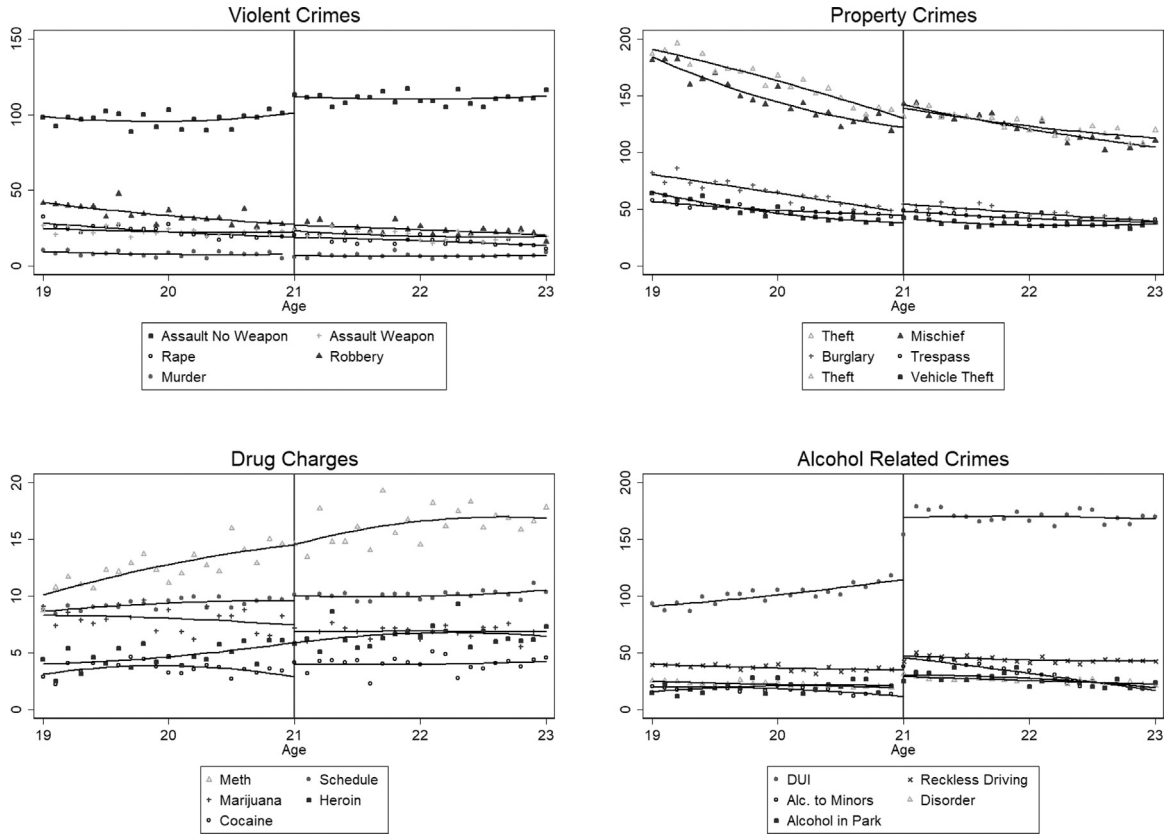

Fig. 2. Crime across the MLDA threshold, by category of crime. *Notes:* Charges within the universe of charges filed in Oregon courts in 1990 through 2012.

Table 3
The effect of attaining MLDA on incidence of violent crimes.

	Murder (1)	Assault (2)	Robbery (3)	Rape (4)
$1(Age \geq 21)$	−0.995	11.95***	−0.881	−0.251
	(1.561)	(3.546)	(3.154)	(1.850)
Semi-elasticity	−0.142	0.095	−0.032	−0.013
	Assault 1 (1)	Assault 2 (2)	Assault 3 (3)	Assault 4 (4)
$1(Age \geq 21)$	−0.0973	1.272	3.989**	6.788***
	(0.583)	(1.637)	(1.705)	(2.232)
Semi-elasticity	−0.019	0.079	0.210	0.081

Notes: This table contains estimates of the change in charges associated with legal access to alcohol at age 21. All models are estimated assuming a quadratic polynomial and bandwidth of two years. Estimated standard errors (robust) are reported in parentheses.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

behind theft. Theft also shows a notable increase, while the other categories of crime, trespassing, burglary, and vehicle theft show more muted responses. However when the increases are translated to percentages in Table 4 most property crimes show similar increases.

Prior research has found evidence that other drugs such as marijuana or cocaine are substitutes for alcohol (Crost and Guerrero, 2012; Deza, 2015). Both Crost and Guerrero (2012) and Deza (2015) examine reported past drug consumption in the months immediately before and after turning 21. Kenkel et al. (2001) find alcohol may be a complement to other drugs in the long-run, through gateway effects.[7] In Fig. 2 we also plot drug-possession charges across the MLDA threshold for each drug type and for schedule I through IV drug crimes.[8] This reveals two stark patterns—the potential substitution away from marijuana upon turning 21 and increasing rates of cocaine-related crime. While subsequent econometric results in Table 4 demonstrate that the 8% decrease in marijuana is not statistically significant, the increase in cocaine possession is large and significant, and on the order of 29%. While point estimates for methamphetamine and heroin possession are also positive, that

[7] See http://arcweb.sos.state.or.us/pages/rules/oars_800/oar_855/855_080.html for additional detail.

[8] Drugs and their immediate precursors are classified in Schedules I through V under the Federal Controlled Substances Act.

Table 4
The effect of attaining MLDA on incidence of non-violent crime.

Panel A: Alcohol-related crimes

	DUI (1)	Reckless driving (2)	Disorderly conduct (3)	Alcohol in parks (4)	Selling to minors (5)
$1(Age \geq 21)$	54.68***	12.63***	10.02***	0.928**	34.31***
	(7.568)	(2.577)	(1.346)	(0.410)	(3.567)
Semi-elasticity	0.429	0.313	0.426	0.441	1.562

Panel B: Property crimes

	Burglary (1)	Theft (2)	Vehicle theft (3)	Trespass (4)	Mischief (5)
$1(Age \geq 21)$	7.448***	8.821*	3.704**	3.534**	19.78***
	(2.108)	(5.155)	(1.740)	(1.337)	(5.276)
Semi-elasticity	0.139	0.063	0.089	0.077	0.149

Panel C: Drug-related crimes

	Heroin (1)	Meth (2)	Cocaine (3)	Marijuana (4)	Schedule 1–4 (5)
$1(Age \geq 21)$	−0.0446	0.0264	1.091**	−0.567	0.473**
	(0.838)	(1.062)	(0.460)	(0.812)	(0.208)
Semi-elasticity	−0.001	0.001	0.288	−0.076	0.048

Notes: This table contains estimates of the change in charges associated with legal access to alcohol at age 21. All models are estimated assuming a quadratic polynomial and bandwidth of two years. Estimated standard errors (robust) are reported in parentheses.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

cocaine offenses are more responsive around the MLDA threshold is also consistent with the general pattern—estimated elasticities are smaller for more-severe crimes. Given prior research that suggests that individuals consume fewer higher hard drugs upon 21, the increased rate of cocaine offenses could be driven increased detection. Indeed, 75% of the charges for cocaine possession coincide with another charge. Thus the increase in cocaine charges could likely originate as secondary charge after the commission of another crime such as an assault or drunk driving.

In Fig. 2 we also report a similar disaggregation of specific alcohol-related crimes, where we see that DUI's, reckless driving, disorderly conduct, alcohol possession in parks, and selling to minors all increase with legal access to alcohol. These increases are also considerable, with DUI and disorderly conduct exhibiting large increases at age 21 (43%, both) while reckless driving and mischief increase 31% and 15%, respectively. (These point estimates are available in Panel A of Table 4.)

3.2. Repeat offenders

Our data afford the ability to link individuals over time, which proves important and introduces new results to the literature related to the effect of legal access to alcohol across criminal history. In Figs. 4 and 5 we stratify by whether the individual is a repeat or first-time offender. While broad categories of crime reveal similar patterns across offender type—significant increases in criminality again appearing in alcohol-related crimes, among both first and repeat offenders—disaggregating again into subcategories (in Figs. 3 and 6) reveals interesting distinctions across criminal history. For example, the significant increase in DUI and reckless driving that is coincident with MLDA are driven by first-time offenders, as is the possession of alcohol in public parks. Disorderly conduct and selling to minors move similarly for both first-time and repeat offenders.

The increase in overall criminality associated with MLDA is also 50% larger among individuals who turn 21 with having already collected a criminal record, thereby exhibiting the lowest *a priori* propensities toward criminality. (See Tables 5 and 6 for these and subsequent point estimates.) Among the most-socially costly crimes that increase at age 21, we see a large increase in assaults at age 21 among first-time offenders contrasted with a small decrease among repeat offenders (with estimated semi-elasticities of 0.151 among first-time offenders, compared to −0.037 among repeat offenders), and 127% larger increase in drunk driving among first-time offenders (with estimated semi-elasticities of 0.434 and 0.191, respectively). Given that younger individuals exhibit heightened myopia and less self-control (Lee and McCrary, 2009), this also highlights the potential costs of lowering minimum legal drinking ages. We note that these differences could also be driven by legal access to alcohol inducing a smaller change in actual access to alcohol for individuals with prior criminal histories. However, among this group we do find that legal access to alcohol is still associated with substantial increases in drunk driving, albeit not as large as the increases present for first-time offenders. While it's possible—perhaps it is even likely—that the first stage of alcohol consumption is smaller among those with prior criminal convictions, the DUI response is consistent with those individuals still experiencing a sizable increase in alcohol consumption.

In Table 7 we break down alcohol-related crimes by prior offense status, demonstrating the broad pattern of responsiveness across various subcategories, both in first-time offenders and among those with priori criminal records, though elasticities tend to be higher among first-time offenders.

To the extent alcohol availability induces individuals into criminality, rather than increasing the criminality of those with established histories of violating the law, we may anticipate higher social costs association with access, as this first-time exposure to the legal system could increase future criminality through negative criminogenic effects. Moreover, any such path-dependence implies that RD-based estimates would be lower-bound estimates of the long-run increase in crime associated with legal access.

This includes the most socially costly crimes that increase at age 21, including assault, where the estimated increase at age 21 actually switches signs and is 5-times larger among first-time offenders, and drunk driving, where the increase is 127% larger among first-time offenders. Given that younger individuals exhibit heightened

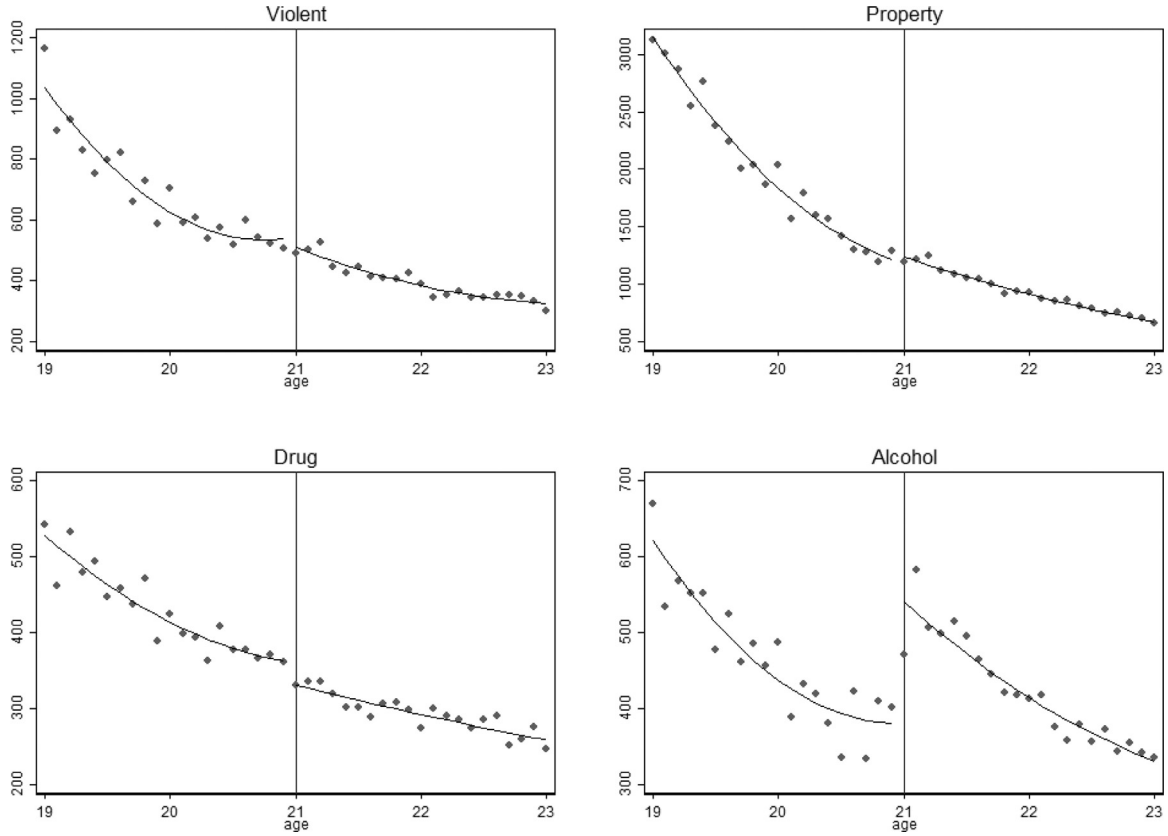

Fig. 3. Crime across the MLDA threshold, by category, repeat offenders only. *Notes:* Charges within the universe of charges filed in Oregon courts in 1990 through 2012.

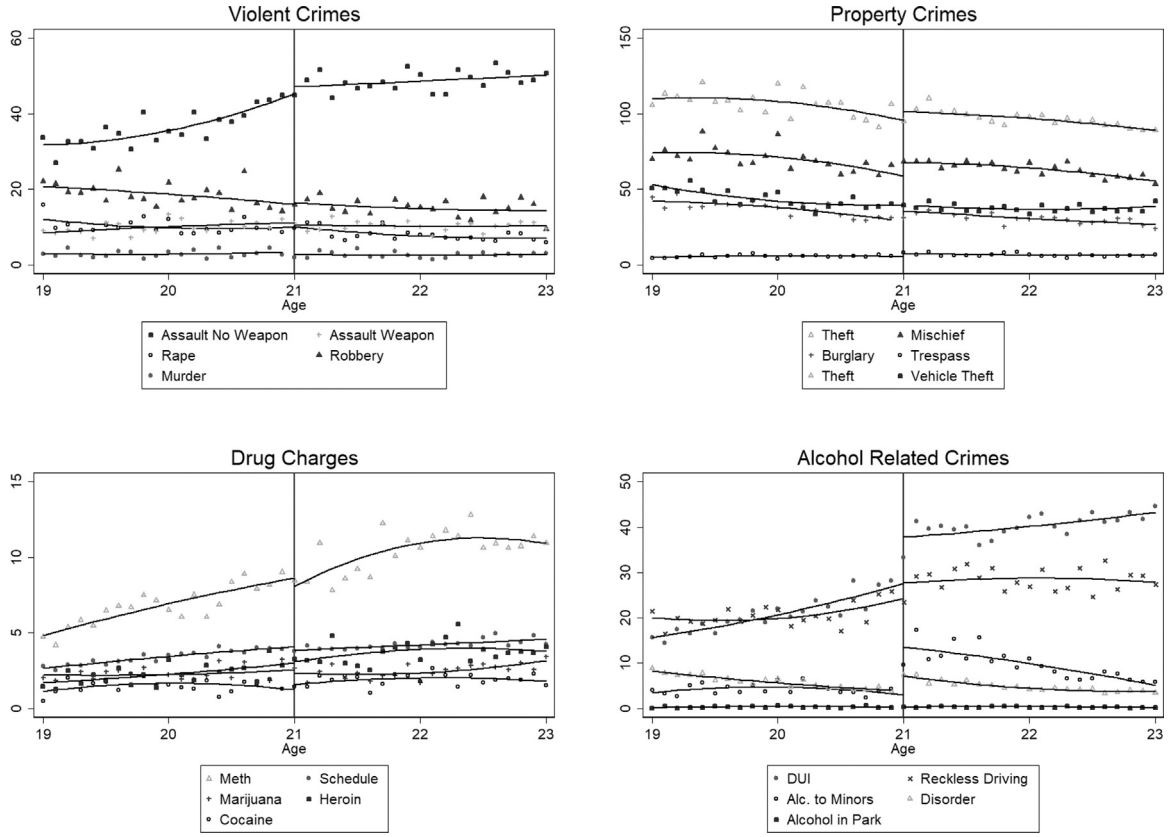

Fig. 4. Crime across the MLDA threshold, by subcategory, repeat offenders only. *Notes:* Charges within the universe of charges filed in Oregon courts in 1990 through 2012.

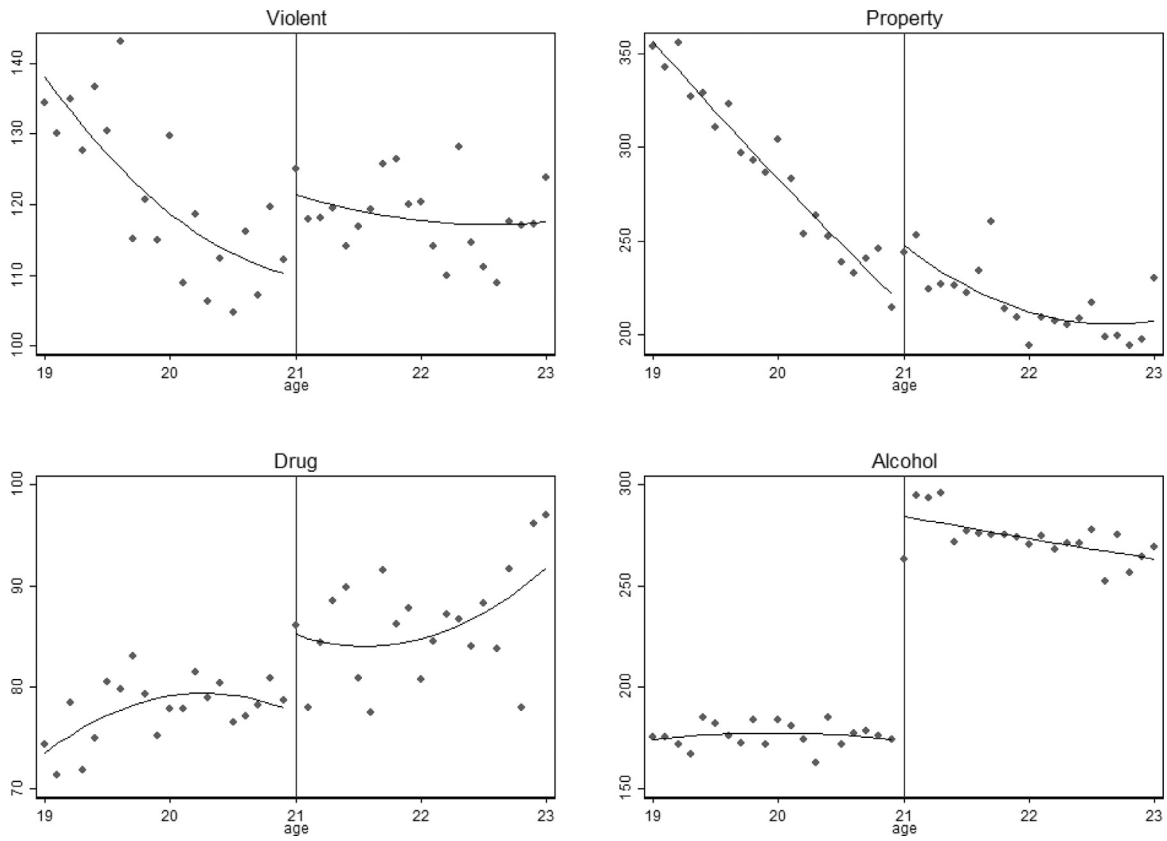

Fig. 5. Crime across the MLDA threshold, by category, first-time offenders only. *Notes:* Charges within the universe of charges filed in Oregon courts in 1990 through 2012.

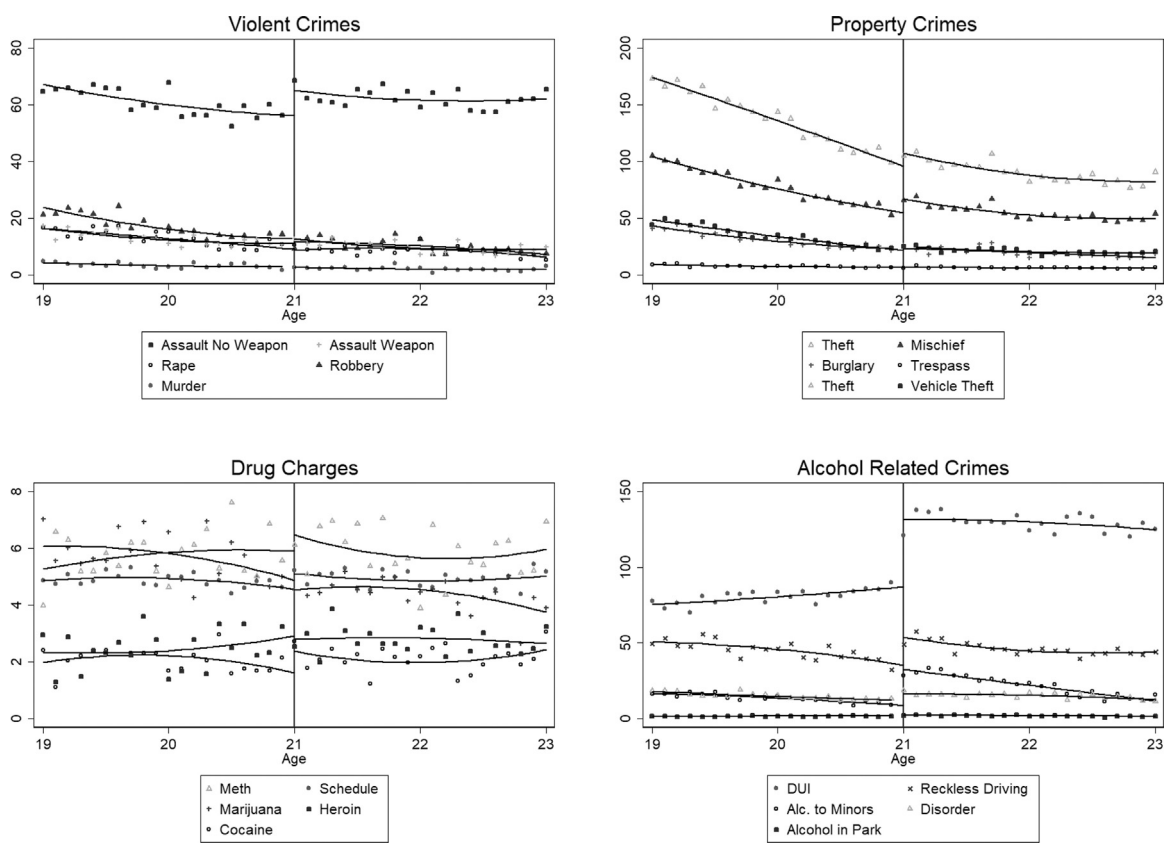

Fig. 6. Crime across the MLDA threshold, by subcategory, first-time offenders only. *Notes:* Charges within the universe of charges filed in Oregon courts in 1990 through 2012.

Table 5
The effect of attaining MDLA, by prior-offense status.

Panel A: Repeat offenders	All crime (1)	Violent crime (2)	Alcohol crime (3)	Property crime (4)	Drug crime (5)
1($Age \geq 21$)	143.1	−37.47	159.0***	24.38	−28.69***
	(133.0)	(38.19)	(41.83)	(66.20)	(10.39)
Semi-elasticity	0.028	−0.077	0.368	0.017	−0.084
Panel B: First-time offenders	All crime (1)	Violent crime (2)	Alcohol crime (3)	Property crime (4)	Drug crime (5)
1($Age \geq 21$)	140.6***	9.191**	91.54***	29.16***	6.203**
	(17.18)	(4.525)	(8.380)	(8.639)	(2.473)
Semi-elasticity	0.140	0.097	0.513	0.133	0.094

Notes: This table contains estimates of the change in charges associated with legal access to alcohol at age 21. All models are estimated assuming a quadratic polynomial and bandwidth of two years. Estimated standard errors (robust) are reported in parentheses.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

Table 6
The effect of attaining MDLA on alcohol-related criminality, by prior-offense status.

Panel A: Repeat offenders	Assault (1)	DUI and reckless (2)	Burglary and mischief (3)	Disorderly conduct (4)	Alcohol in parks (5)
1($Age \geq 21$)	−11.93	63.73**	33.11***	46.85*	0.902
	(16.88)	(31.23)	(5.269)	(26.96)	(1.472)
Semi-elasticity	−0.037	0.191	0.616	0.081	0.278
Panel B: First-time offenders					
1($Age \geq 21$)	13.24***	76.31***	4.691***	17.54***	1.039**
	(3.779)	(9.064)	(1.412)	(5.910)	(0.437)
Semi-elasticity	0.151	0.434	0.264	0.167	0.551

Notes: This table contains estimates of the change in charges associated with legal access to alcohol at age 21. All models are estimated assuming a quadratic polynomial and bandwidth of two years. Estimated standard errors (robust) are reported in parentheses.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

Table 7
The effect of attaining MLDA on interactions with police officers.

	Resist arrest (1)	Assault officer (2)	False info (3)	Interfere with officer (4)
1($Age \geq 21$)	3.136***	0.897	−3.778**	2.148*
	(0.944)	(0.550)	(1.404)	(1.270)
Semi-elasticity	0.220	0.197	−0.295	0.262

Notes: This table contains estimates of the change in charges associated with legal access to alcohol at age 21. All models are estimated assuming a quadratic polynomial and bandwidth of two years. Estimated standard errors (robust) are reported in parentheses.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

myopia and less self-control (Lee and McCrary, 2009), this also highlights the potential costs of lowering minimum legal drinking ages.

3.3. Police officer interactions upon arrest

In the above analysis, we are implicitly attributing the increase in criminality at age 21 to the presence of alcohol, and not to some coincident increase in the proclivity of police to arrest individuals for these crimes. Yet, with any inducement into criminality, policy is informed by considering any changes to the nature of police interactions. In Fig. 7, we plot rates of resisting arrest, officer assault, and the giving of false information. We find significant increases in resisting arrest coincident with the age-21 cutoff, suggesting that with the increased availability of alcohol, perpetrator/officer exchanges are changing in the direction we might anticipate if alcohol influences perpetrators' self control.[9] Point estimates also imply an 18% (but statistically insignificant) increase in assaults on police officers. There is no apparent increase in the presentation of false information, and we find that other interactions with police, such as presenting them with false information, are essentially unchanged. This suggests that police are not uniformly increasing all charges for individuals when they reach the age of 21. It is also possible the lawyers might tread individuals differently depending on their

[9] While we are unable to rule out such patterns being driven by officer behavior, alcohol is arguably likewise implicated and we are inclined to interpret these patterns as evidence of alcohol consumption increasing with MLDA attainment.

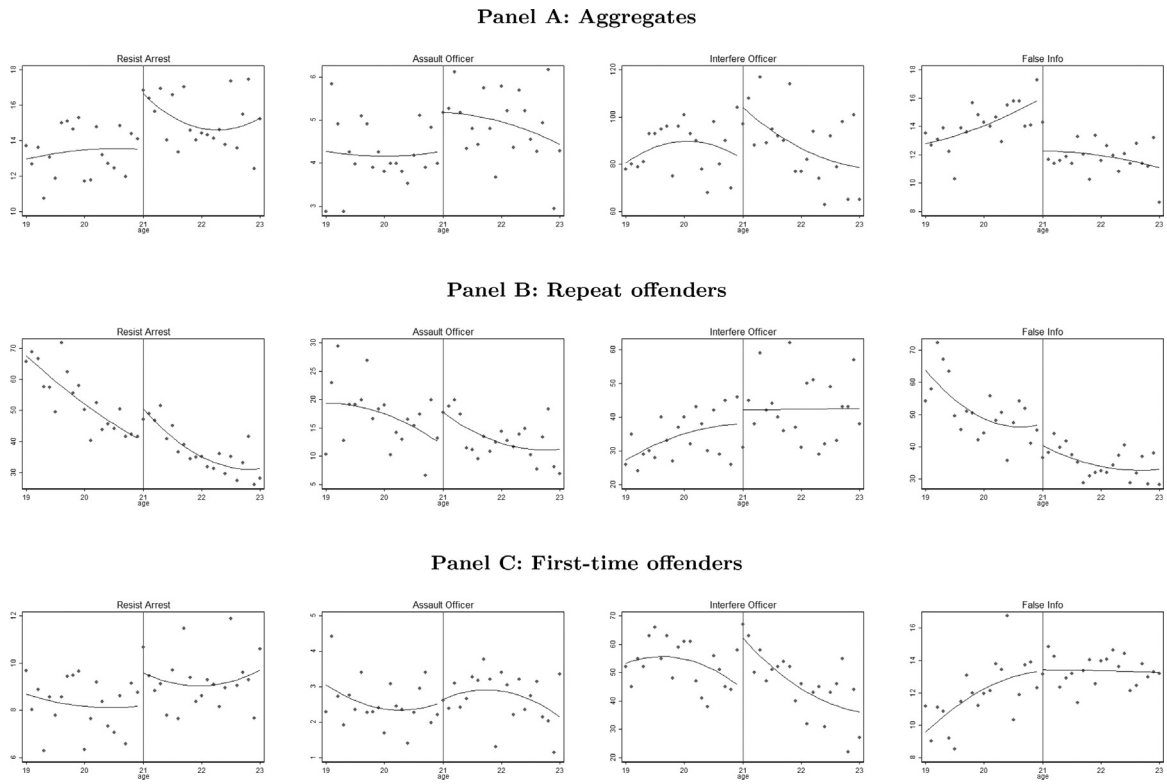

Fig. 7. Officer-interaction crime across the MLDA threshold. *Notes:* All charges filed in Oregon courts in 1990 through 2012.

Table 8
The effect of attaining MDLA on recidivism.

	All crime (1)	Violent crime (2)	Alcohol crime (3)	Property crime (4)	Drug crime (5)
$1(Age \geq 21)$	0.010**	−0.001	0.012***	0.001	−0.004**
	(0.004)	(0.001)	(0.002)	(0.003)	(0.001)
Semi-elasticity	0.052	−0.051	0.410	0.009	−0.238

Notes: This table contains estimates of the change in charges associated with legal access to alcohol at age 21. All models are estimated assuming a quadratic polynomial and bandwidth of two years. Estimated standard errors (robust) are reported in parentheses.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

age relative to 21. However, when we examine final dispositions for charges, we find that case dismissals are essentially unchanged at age 21. The point is estimate is small (0.005) relative to an average dismissal probability of 0.40, and although we fail to reject the null that the point estimate is zero, the stand error is precise enough (0.004) that we can be confident that dismissal probabilities are not changing in any economically meaningful way in either direction.

3.4. Robustness

Underlying theoretical support for a regression discontinuity design is based on the assumption that the expected conditional means on the left and right side of the cutoff are the same, absent the treatment. While this assumption considers the behavior of the data-generating process near the cutoff, in practice, researchers face a underlying trade-off of bias and variance concerning how large to make their bandwidth. In Fig. 8 we explore the sensitivity of the estimated semi-elasticities to bandwidth selection, examining the resulting point estimate and confidence interval for bandwidths between 6 months and 36 months. Overall the point estimates and standard errors are quite stable.

Before concluding, we examine how annualized recidivism-risk changes in Table 8 and Fig. 9. This in part addresses whether the repeat-offender population close to the legal-drinking threshold has greater criminogenic tendencies than the average individual with a prior criminal history. Comparing recidivism across crime measurements is often complicated, because many agencies use different definitions for recidivism (e.g., violations of probation, new convictions, rearrests). The combination of all of these types of recidivism usually results in a 1-year recidivism risk close to 30% based on national estimates (Castillo et al., 2004). We find an overall annualized risk of facing new charges of roughly 24% close to the age 21 threshold. Given that our administrative records include only criminal charges and not violations (which typically are roughly equal to rearrests in their contribution to recidivism measures) this suggests that the repeat-offender population close to age 21 has greater criminogenic tendencies than average individuals with prior criminal histories. We also find the highest recidivism rates among property crimes (5% recidivate in that category in a given year), yet in that crime category we find no increases in recidivism associated with MLDA. On the other hand, recidivism in alcohol-related crimes is only around 2.5% in a given year, but we find increases of 1 percentage point with legal access to alcohol. This

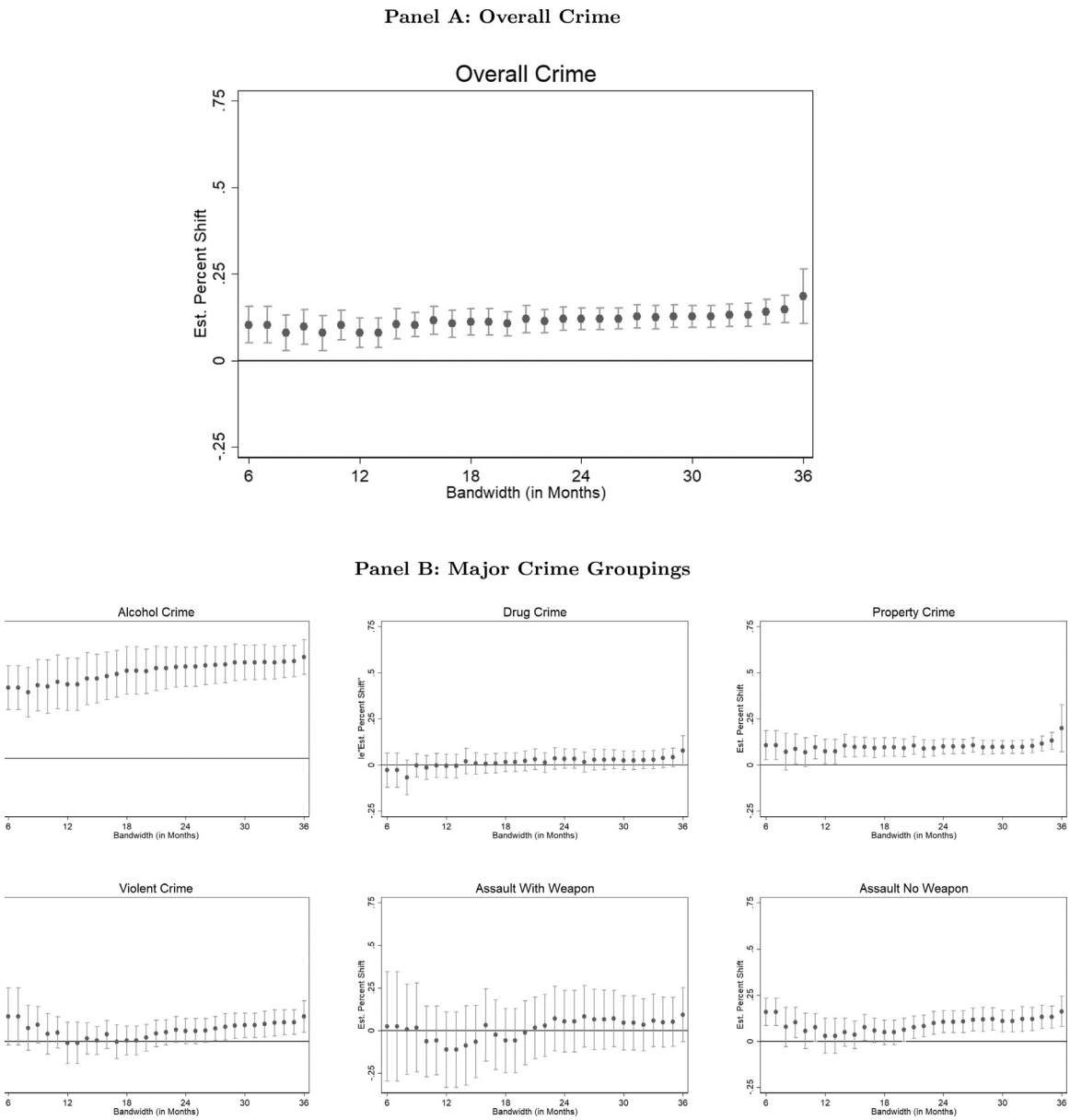

Fig. 8. Bandwidth sensitivity of main results. *Notes:* Charges filed in Oregon courts in 1990 through 2012.

suggests that changes in recidivism rates are not high enough by themselves to drive all of our findings.

4. Conclusion

In this paper, we revisit the relationship between access to alcohol and criminality utilizing data from the universe of criminal charges filed in Oregon from 1990 to 2012. We find strikingly similar estimates to Carpenter and Dobkin (2015), which lends to the external credibility of their findings. Our findings also suggest that a potential source of bias in California-based estimates—legal access to handguns—is not significantly confounding previous results. The confirmation of the estimated policy response of criminality associated with legal access to alcohol across two different states using different measures of crime (arrests instead of charges) is notable.

However, minor differences do emerge. We find no evidence that robberies increased, while we did find evidence that certain types of property related crimes, such as burglary, larceny or trespassing increased at age 21 as well. This could potentially due to measuring criminality through charges rather than through arrests.[10]

The universe of Oregon court charges also allows us to link individuals over time. This richness thus enables the consideration of heterogenous responses by prior criminal histories. Doing so reveals 50% larger increases in criminality at age 21 among individuals with no criminal record. This includes the most socially costly crimes that increase at age 21, including assault (where the increase at age 21 is 10-times larger and switches sign among first-time offenders) and drunk driving (where the increase is 65% larger among first-time offenders). Again, given that younger individuals exhibit heightened myopia and less self-control (Lee and McCrary, 2009), this also highlights the potential costs of lowering minimum legal drinking age as discussed in Carpenter and Dobkin (2010). Indeed, the increased crime rates for drunk and reckless driving,

[10] It is also more difficult to find statistically significant evidence of shifts some rarer crimes.

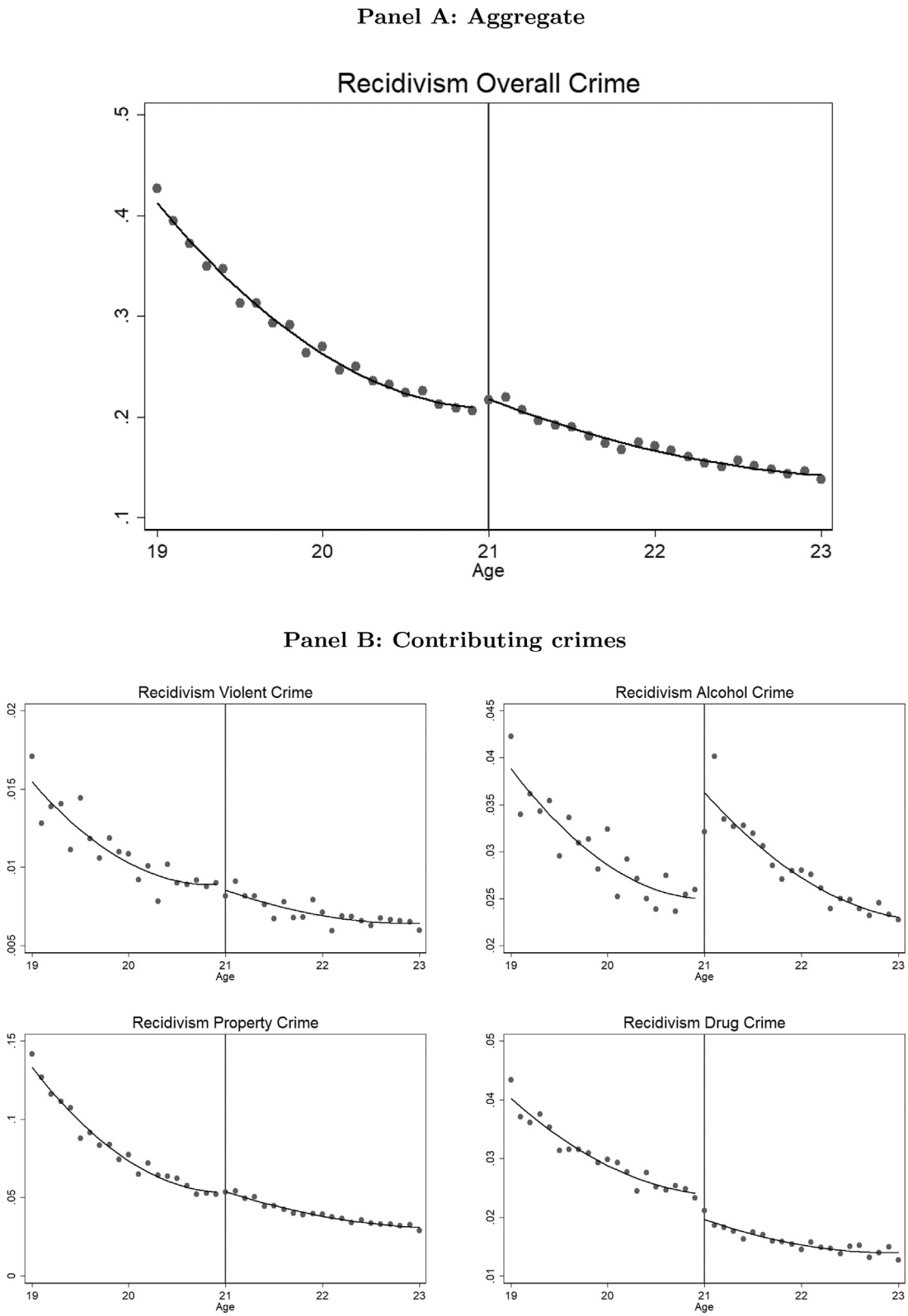

Fig. 9. Annualized recidivism across the MLDA threshold, by category, repeat offenders only. *Notes:* Annualized recidivism rates based on the universe of charges filed in Oregon courts in 1990 through 2012.

assaults, and property crimes at age 21 could be exacerbated by the age crime curve given that many of these crimes are decreasing as age increases in ages below 21 and above 18. Furthermore, the increased rate of drunk driving at age 21 could be potentially reduced if harsher punishments for drunk driving at age 21 were imposed (Hansen, 2015), or through increased enforcement (Lindo et al., 2015; Hansen and DeAngelo, 2014).

While our findings suggest a compelling relationship between alcohol and crime, the implications of MLDA policy could be larger that we identify, as our estimates pertain to alcohol's effect on *crim-*

inality and do not fully capture any direct implications there may be for victimization. If that is the case, our estimates would understate the social costs of alcohol consumption attributable to increased crime.

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