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LEGAL ACCESS TO ALCOHOL AND CRIMINALITY

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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 lacking in 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 percent larger; still larger for the most socially costly crimes of assault and drunk driving. This suggests that deterring criminality through increased punishments would likely prove difficult.

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1 Introduction

Recent research has found evidence of significant social costs associated with legal access to alcohol (Carpenter, 2004; Carpenter and Dobkin, 2009; Carpenter and Dobkin, 2010; Anderson, Crost, and Rees, 2014). 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 percent of robberies and 21 percent 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, 2014). Unlike in California, legal handgun access 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.

Utilizing regression-discontinuity models, we find evidence that overall crime increases with the legal availability of alcohol. Our estimates 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.[1] 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. 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.

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,

[1]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.

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-percent larger among individuals who turn 21 without having already collected a criminal record. This is somewhat discouraging in terms of the effectiveness of traditional channels for reducing crime—deterrence, for example, as in Becker (1968), Lee and McCrary (2009), and Hansen (2015)—as these are the same individuals who likely exhibit the lowest *a priori* propensities toward criminality. Given that younger individuals exhibit more myopia and less self-control, this also highlights the potential costs of lowering the minimum legal drinking age (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 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. (We have also estimated Poisson count-data models in which we obtained near-identical estimates, both in magnitude and statistical significance.)

3 Results

We come to this problem with the benefit of existent 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 Figure 1) and facilitating inference by quantifying the discontinuity in overall rates of crime coincident

with age 21 (see Table 1). We find a 10.6 percent increase in overall crime when individuals obtain legal access to alcohol; an increase in the total number of monthly charges of 2,106.

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 A of Figure 2, 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 Panel B, we disaggregate crimes within the broad categories of Panel A—we separate violent crimes into four underlying contributors, for example (i.e., murder, rape, robbery, and assaults with and without a weapon). Disaggregating this way again reveals that the lesser of these crimes (i.e., assault) is responsive to legal access to alcohol, while robbery and rape are not. This also in keeping with the prior that alcohol plays less of a role in crimes for which premeditation is possibly more acute. Carpenter and Dobkin (2015) find small

but significant increases in robbery, of a magnitude that falls within the confidence interval around out estimates.

With our additional flexibility allowing for the plotting of the categories of assault, Column (1) 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"). Conditional on an assault, we again find that alcohol availability is seemingly mattering more where crimes are of less severity and potentially less premeditated.

In Column (3) of Figure 2, we plot drug-possession charges across the MLDA threshold for each drug type and for schedule I through IV drug crimes.[2] 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 3 demonstrate that the 8-percent decrease in marijuana is not statistically significant, the increase in cocaine possession is large and significant, and on the order of 28 percent. While point estimates for methamphetamine and heroin possession are also positive, that cocaine offenses are more responsive is also consistent with the patten of elasticities falling in crime severity.

When doing a similar unpacking of alcohol-related crimes in Column (4), 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 (41 percent, both) while reckless driving and mischief increase 31 and 15 percent, respectively. (These point estimates are

[2]Drugs and their immediate precursors are classified in Schedules I through V under the Federal Controlled Substances Act. See `http://arcweb.sos.state.or.us/pages/rules/oars_800/oar_855/855_080.html` for additional detail.

available in Panel A of Table 3.)

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 figures 3 and 4 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—the disaggregated figures in Panel B reveal 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.

Somewhat discouraging, possibly, is that the increase in overall criminality associated with MLDA is 50-percent larger among individuals who turn 21 without having already collected a criminal record. (See tables 4 and 5 for these and following point estimates.) These are the same individuals who exhibit the lowest *a priori* propensities toward criminality, which brings the effectiveness of traditional channels for reducing crime—deterrence, for example. Among the most socially costly crimes that increase at age 21, we see a ten-times larger increase in assaults at age 21 among first-time offenders (with estimated semi-elasticities of .134 among first-time offenders, compared to .011 among repeat offenders), and 65-percent larger increases in drunk driving among first-time offenders (with estimated semi-

elasticities of .416 and .252, respectively). Given that younger individuals exhibit heightened myopia and less self-control, this also highlights the potential costs of lowering minimum legal drinking ages.

To the extent alcohol availability induces individuals into criminality, rather than increasing the criminality of those with established histories of violating the law, we should anticipate higher social costs association with access as much as this first-time exposure to the legal system could increase future criminality through negative criminogenic effects, with any such path-dependence implying 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 increase at age 21 is 10 times larger among first-time offenders—and drunk driving—where the increase is 65-percent larger among first-time offenders. Given that younger individuals exhibit heightened myopia and less self-control, 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 or charge individuals for these crimes. Yet, with any inducement into criminality policy is informed by considering any changes to the nature of police interactions at this margin.

In Figure 5, 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.[3] Point estimates also imply an 18-percent (but insignificant) increase in assaults on police officers. There is no apparent increase in the presentation of false information. We find other interactions with police, such as presenting them with false information, are essentially unchanged. This suggests that police are uniformly increasing all charges for individuals when they reach the age of 21.

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 1990-2012. 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

[3]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.

criminality through charges rather than through arrests.

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-percent larger increases in criminality at age 21 among individuals with no criminal record. This is somewhat discouraging, in terms of the effectiveness of traditional channels for reducing crime, as these are individuals who likely exhibit the lowest *a priori* propensities toward criminality. This includes the most socially costly crimes that increase at age 21, including assault—where the increase at age 21 is 10 times larger among first-time offenders—and drunk driving—where the increase is 65-percent larger among first-time offenders. Given that younger individuals exhibit heightened myopia and less self-control, this also highlights the potential costs of lowering minimum legal drinking ages.

Furthermore, because the evidence suggests that the MLDA is increasing crime through individuals who otherwise do not have a history of criminality, it could be the case that the RD based estimates of the MLDA may be understating the effects of legal access on crime. This type of bias towards zero could result if exposure to the legal system and time in jail or prison encourages future criminality through the negative criminogenic effects of incarceration. As such, RD-based estimates could be viewed as a lower bound for the effect of legal access to alcohol on criminality.

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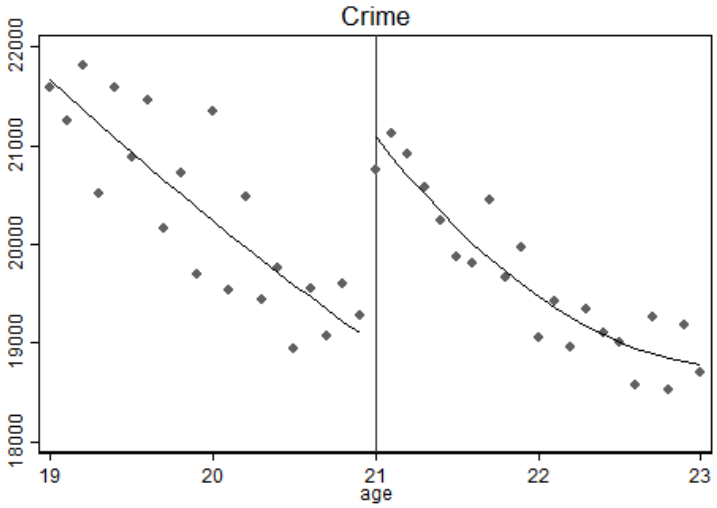
5 Figures and Tables

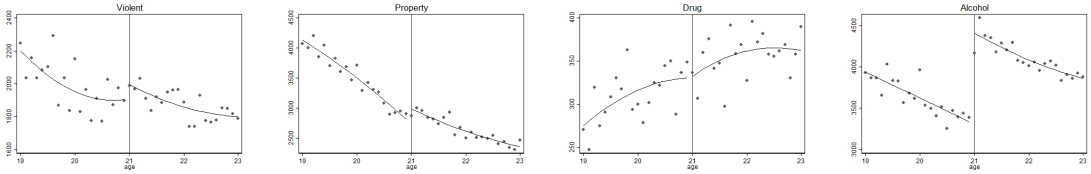
Figure 1: Crime across the MLDA threshold



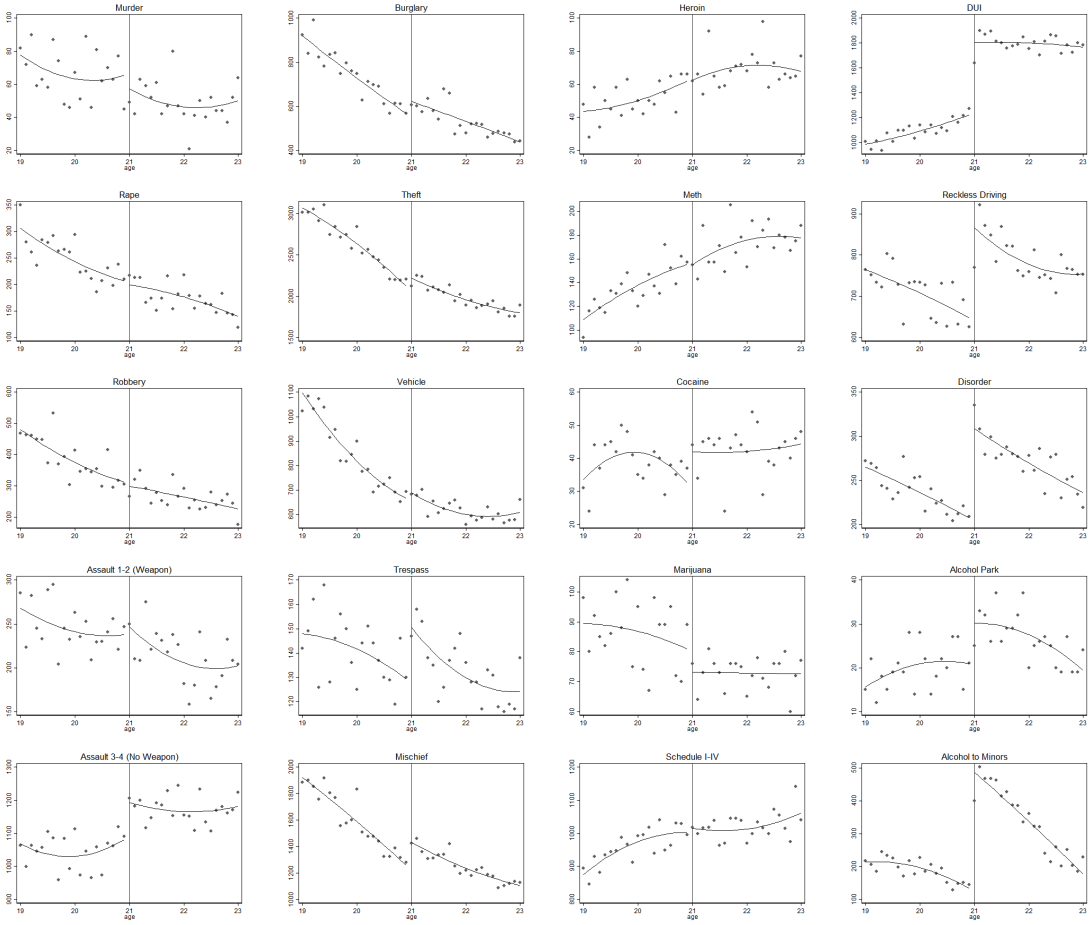
Notes: Charges within the universe of charges filed in Oregon courts in 1990 through 2012.

Figure 2: Crime across the MLDA threshold, by category

Panel A: Aggregates



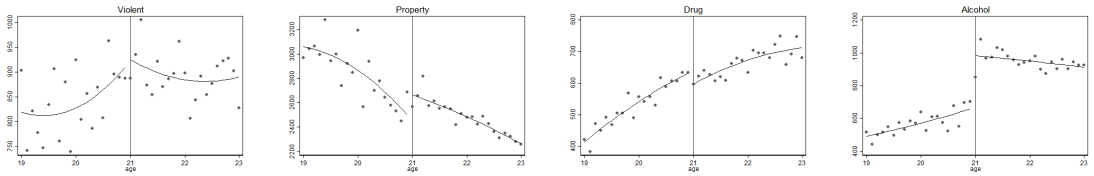
Panel B: Contributing crimes



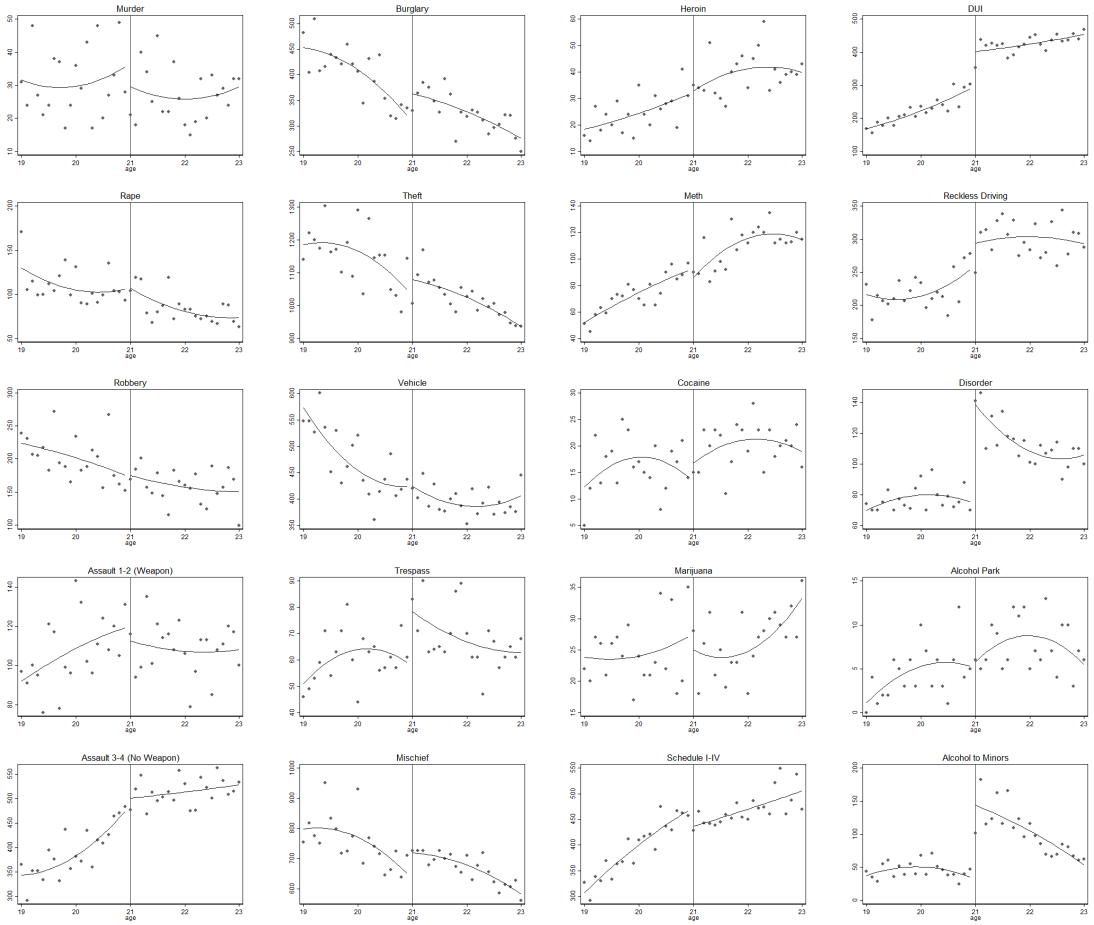
Notes: Charges within the universe of charges filed in Oregon courts in 1990 through 2012.

Figure 3: Crime across the MLDA threshold, by category, repeat offenders only

Panel A: Aggregates

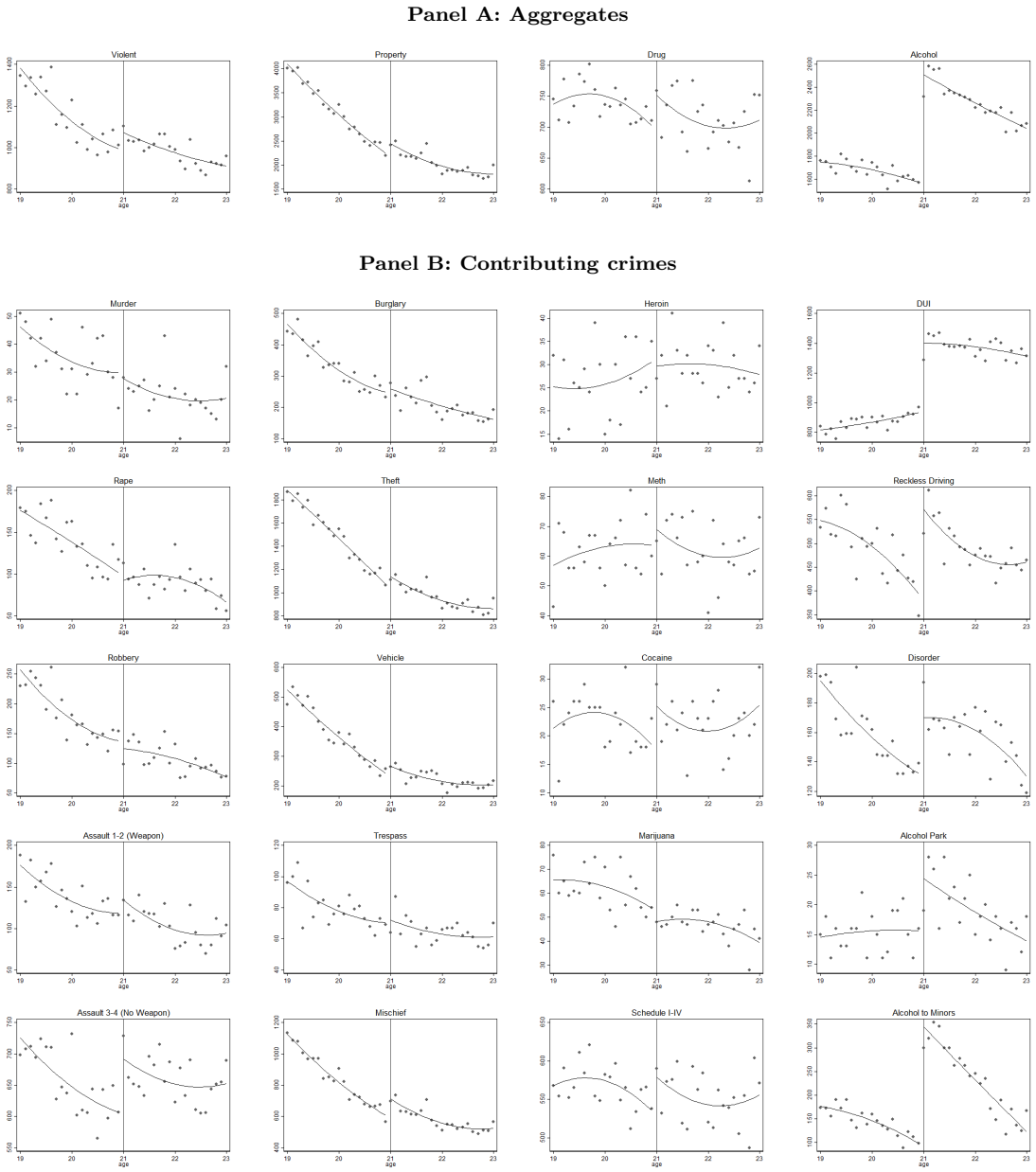


Panel B: Contributing crimes



Notes: Charges against repeat offenders within the universe of charges filed in Oregon courts in 1990 through 2012.

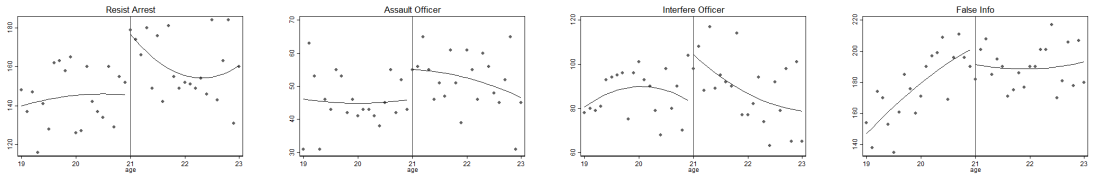
Figure 4: Crime across the MLDA threshold, by category, first-time offenders only



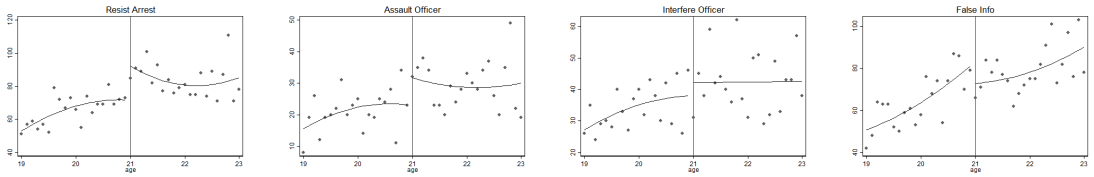
Notes: Charges against first-time offenders within the universe of charges filed in Oregon courts in 1990 through 2011.

Figure 5: Officer-interaction crime across the MLDA threshold

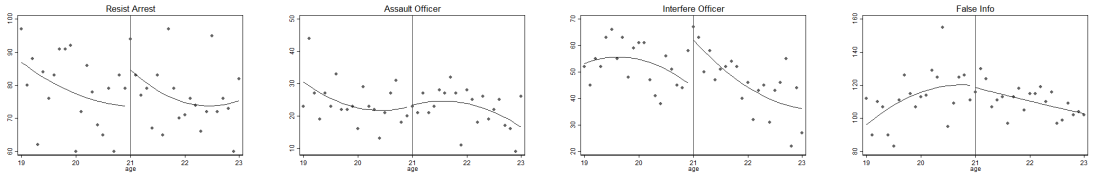
Panel A: Aggregates



Panel B: Repeat offenders



Panel C: First-time offenders



Notes: Charges within the universe of charges filed in Oregon courts in 1990 through 2012.

Table 1: The effect of attaining MLDA on incidence of crime, by category

	All crime (1)	Violent crime (2)	Alcohol crime (3)	Property crime (4)	Drug crime (5)
Age 21	2106.7***	87.36	1269.9***	496.9***	15.24
	(318.0)	(67.35)	(151.5)	(129.7)	(29.60)
Semi-elasticity	.106	.045	.478	.098	.012

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

Table 2: The effect of attaining MLDA on incidence of violent crimes

	Murder (1)	Assault (2)	Robbery (3)	Rape (4)
Age 21	-9.339	110.6***	-8.775	-5.151
	(13.56)	(38.58)	(34.78)	(19.81)
Semi-elasticity	-.167	.083	-.028	-.026

	Assault 1 (1)	Assault 2 (2)	Assault 3 (3)	Assault 4 (4)
Age 21	-2.254	10.68	39.71**	62.48**
	(6.207)	(17.20)	(18.29)	(23.99)
Semi-elasticity	-.041	.062	.195	.070

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

Table 3: The effect of attaining MLDA on incidence of non-violent crime

Panel A: Alcohol-related crimes

	DUI	Reckless driving	Disorderly conduct	Alcohol in parks	Selling to minors
	(1)	(2)	(3)	(4)	(5)
Age 21	567.3***	225.1***	104.2***	9.555**	363.7***
	(81.35)	(52.25)	(14.44)	(4.391)	(38.25)
Semi-elasticity	.417	.302	.414	.425	1.562

Panel B: Property crimes

	Burglary	Theft	Vehicle theft	Trespass	Mischief
	(1)	(2)	(3)	(4)	(5)
Age 21	70.35***	159.0**	33.81	22.77**	211.0***
	(24.73)	(75.86)	(31.38)	(9.209)	(46.34)
Semi-elasticity	.116	.071	.048	.167	.153

Panel C: Drug-related crimes

	Heroin	Meth	Cocaine	Marijuana	Schedule 1-4
	(1)	(2)	(3)	(4)	(5)
Age 21	-1.108	-1.452	11.32**	-6.939	13.42
	(8.932)	(11.36)	(4.954)	(8.727)	(22.46)
Semi-elasticity	-.019	-.010	.280	-.088	.014

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

Table 4: 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)
Age 21	734.1***	1.503	315.1***	213.3	-42.34***
	(220.4)	(45.62)	(75.26)	(127.3)	(15.14)
Semi-elasticity	.082	.002	.441	.081	-.072

Panel B: First-time offenders

	All crime (1)	Violent crime (2)	Alcohol crime (3)	Property crime (4)	Drug crime (5)
Age 21	1372.6***	85.85*	954.7***	283.6***	57.58**
	(185.8)	(48.62)	(89.90)	(92.50)	(26.54)
Semi-elasticity	.126	.082	.495	.118	.079

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

Table 5: The effect of attaining MDLA on alcohol-related criminaity, by prior-offense status

Panel A: Repeat offenders

	Assault	DUI and reckless	Burglary and mischief	Disorderly conduct	Alcohol in parks
	(1)	(2)	(3)	(4)	(5)
Age 21	6.288	138.5**	64.19***	143.6***	0.717
	(23.70)	(55.59)	(8.912)	(50.64)	(2.373)
Semi-elasticity	.011	.252	.670	.135	.145

Panel B: First-time offenders

	Assault	DUI and reckless	Burglary and mischief	Disorderly conduct	Alcohol in parks
	(1)	(2)	(3)	(4)	(5)
Age 21	104.3***	653.9***	40.04***	137.8**	8.838**
	(33.89)	(80.32)	(12.26)	(53.21)	(3.880)
Semi-elasticity	.134	.416	.255	.150	.527

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

Table 6: The effect of attaining MLDA on interactions with police officers

	Resist arrest	Assault officer	False info	Interfere w officer
	(1)	(2)	(3)	(4)
Age 21	31.50***	8.941	-11.52	22.27
	(10.16)	(5.896)	(11.43)	(13.61)
Semi-elasticity	.207	.185	-.063	.255

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