The Drinking Age, Alcohol Consumption, and Crime

Christopher Carpenter and Carlos Dobkin[1]

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Abstract

We use the exogenous variation in alcohol consumption induced by the Minimum Legal Drinking Age (MLDA) and data from California to determine how much an increase in drinking increases criminal behavior. We find that individuals just over age 21 are 32 percent more likely to report having consumed alcohol in the previous month and drink on 70 percent more days than those just under age 21. This greater alcohol consumption results in a 6 percent increase in arrests, which is largely attributable to robberies, assaults, DUI, drunkenness, and disorderly conduct. These results imply an elasticity of about .08, which suggests that crime will increase significantly if proposals to reduce the legal drinking age are adopted.

[1] Carpenter: The Paul Merage School of Business, University of California Irvine, kittc@uci.edu. Dobkin: Department of Economics, University of California Santa Cruz, cdobkin@ucsc.edu. We are grateful to Shaun Devlin, Detective Sergeant of the UCI Police Department, for very helpful conversations. We also thank seminar participants at UC Riverside and the All-UC Labor Conference for many useful comments, and we thank Brandon Traudt and the staff at the UCLA Center for Health Policy Research for assistance with the CHIS data. Some of the results in this paper are based on confidential data. Readers can contact the authors for information on obtaining access. We acknowledge grant support from NIH/NIAAA #RO1 AA017302-01. The usual caveats apply.

Introduction

The high social costs of crime have motivated substantial research with the goal of understanding its causal determinants. Recent work has focused on incarceration (e.g., Hjalmarsson 2005), police enforcement (e.g., DiTella and Schargrodsky 2004 and Levitt 1997), unemployment (e.g., Raphael and Winter-Ebmer 2001), and education (e.g., Lochner and Moretti 2004). There is also a growing body of research that suggests that alcohol consumption may increase criminal behavior, particularly among young adults. High crime rates and high drinking rates among individuals age 18-24 are certainly consistent with the possibility of a link between alcohol and crime, as are several descriptive facts about those who have been arrested and convicted for crimes. Over 60 percent of incarcerated individuals surveyed by the Bureau of Justice Statistics admitted that they had been drinking before they committed a crime (BJS 1998). And millions of violent crimes are perpetrated each year by people whose victims report being certain that the perpetrator had been drinking alcohol (BJS 1998). A 2004 report by the Institute of Medicine notes that criminal activity constitutes the largest social cost of under-age alcohol use ($29 billion annually), even greater than the social costs of motor vehicle accidents ($19 billion) (Bonnie and O'Connell 2004).

Of course, the key difficulty facing social science researchers trying to determine whether drinking alcohol causes the commission of crimes is the fact that some individuals are particularly likely to engage in many risky behaviors, especially those in late adolescence and early adulthood. This raises the concern that a substantial part of the correlation between alcohol consumption and crime may be due to unobserved heterogeneity. Young adults with a taste for risk, for example, may choose both to consume alcohol and to commit crimes. In fact, previous research has not reached a consensus on whether and to what extent drinking alcohol leads to the commission of various types of crime.

Estimating the relationship between alcohol consumption and crime is important for several reasons. First, alcohol consumption can be manipulated by public policy. Stricter enforcement of minimum purchase age laws, for example, would increase the difficulty of obtaining alcohol and presumably result in reduced drinking participation

and intensity.[2] Given that about half of 18-20-year-olds report recent alcohol consumption, there is notable latitude for policy interventions to reduce alcohol consumption among the under-age population. Second, young adults in their late teens and early 20s are responsible for a disproportionate share of all criminal activity. As such, understanding the causal determinants of criminal behavior among individuals in this age group and developing appropriate policies could result in meaningful reductions in total crime. Finally, understanding the effect of alcohol consumption on crime is important for current policy debates regarding the minimum drinking age. Florida, Wisconsin, Vermont, and Missouri have all considered proposals to reduce their drinking age in recent years (Johnson 2007).[3]

In this paper we provide direct evidence of the effect of alcohol consumption on crime by exploiting the sharp differences in alcohol availability caused by the minimum legal drinking age (MLDA). Specifically, in the U.S. it is illegal for youths under age 21 to purchase or consume alcohol.[4] This means that the difficulty of obtaining alcohol falls discretely after an individual turns 21. There are virtually no other institutional features in the U.S. that change discretely at age 21.[5] This allows us to implement a regression discontinuity design (RDD) in which alcohol consumption and crime outcomes for individuals who just turned 21 are compared with those same outcomes for individuals who are just about to turn 21. The intuition behind the RDD is straightforward: since all observed and unobserved determinants of drinking and criminal behavior other than alcohol access are very likely to trend smoothly across the age-21 threshold, any increase in drinking and criminal behavior at age 21 can be attributed to the easier access to alcohol. This makes it possible to avoid the omitted variables bias that plagues conventional estimates of the association between alcohol consumption and crime.

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[2] Throughout this paper we use the terms "minimum purchase age" and "minimum drinking age" interchangeably, although they are, strictly speaking, different concepts.

[3] Our research design yields the local average treatment effect (LATE) for people sensitive to government intervention, which makes it ideal for predicting the likely effects of these proposals.

[4] We do not review here the history of the U.S. minimum legal drinking age, which has been set at age 21 in most states for at least twenty years. There are minor exceptions to this rule in certain states (e.g., if a parent is present), but we ignore these since there is no such exception in the state of California, which is the focus of our study.

[5] There are only a few exceptions, which we revisit below. For example, in California there are some Native American casinos that prohibit playing table games (e.g., blackjack) for individuals under 21. We do not address this directly because it is a fairly minor restriction that is less likely to be enforced than the MLDA.

We use confidential versions of the 2001, 2003, and 2005 California Health Interview Survey (CHIS) to estimate the impact of the MLDA on alcohol consumption. The CHIS provides self-reported drinking measures for young adults along with detailed information on the age of each respondent.[6] This allows us to estimate the magnitude of the discrete increase in alcohol consumption at age 21, and this increase in drinking makes it possible for us to plausibly identify the causal effect of alcohol consumption on crime. We also use the CHIS data to show that other observed correlates of alcohol consumption do, in fact, trend smoothly across the age-21 threshold. For crime outcomes we use administrative data on arrests from California's Monthly Arrest and Citation Register (MACR) for 2000 through 2006, a census of arrests in the state.[7] These confidential arrest files contain information on an individual's exact age in days at the time of arrest and the most serious offense he/she is charged with. Working with a census of arrests from a large state makes it possible to separately examine all the major types of crime.

We find strong evidence that alcohol access has important effects on alcohol consumption and criminal activity. Past-month drinking participation and intensity both exhibit large discrete increases at age 21 for young adults in the CHIS. Specifically, we estimate that the proportion of people who reported drinking in the previous month is about 16.7 percentage points greater at age 21 – in other words, approximately 33 percent higher than the drinking participation rate of people just under 21. Drinking intensity also increases sharply at age 21: we find that the proportion of days in the previous month on which an individual reports drinking alcohol is about 70 percent higher than the proportion of drinking days for people just under 21. Importantly, we find that the other determinants of alcohol consumption we observe in our data (e.g. employment, demographics) trend smoothly across the age-21 threshold, strongly suggesting that the discrete jumps in alcohol consumption we observe are due to the MLDA.

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[6] All three surveys record the exact date the interview was conducted. The 2001 survey includes each interviewee's date of birth, so we can compute a person's exact age at the time of the interview. For the 2003 and 2005 surveys we observe only month and year of birth so we can compute each interviewee's age in months on the day he/she was surveyed.

[7] Like previous research, we focus on arrests because crimes are not reported by age of offender. Though we have data from 1979 to 2006, we focus on arrest data from 2000-2006 so as to closely match the period for which we have data on alcohol consumption.

When we examine the age profile of arrested individuals, we find that the increase in alcohol consumption documented above results in a statistically significant 6 percent increase in total arrests. When we disaggregate by crime type, we find large and robust effects for violent crime arrests: these arrests jump sharply by about 6 percent at age 21, and the increase is statistically significant. The rise in violent crime arrests is largely due to an increase in assaults and robberies. We also find very large increases in arrests for driving under the influence, drunkenness, and nuisance crimes. In contrast to previous research, we find no evidence of a change in the arrest rates for either property crime or drug possession or sale, which suggests that alcohol consumption, on the margin we observe, plays little role as a causal factor in these types of crime.

Taken together, our results provide the strongest and most comprehensive evidence to date that restricting access to alcohol can significantly reduce crime. Unlike previous researchers, we are also able to directly quantify the relationship between alcohol consumption and crime. Specifically, we estimate that a 10 percent decrease in the proportion of recent drinking days reduces the probability of being arrested by about 1 percent.

The remainder of the paper proceeds as follows. Section 1 provides a brief review of the relevant literature that has used quasi-experiments to evaluate the effect of alcohol access on crime. Section 2 describes the California Health Interview Survey data and the California MACR and discusses our empirical methods. Section 3 presents the main results and Section 4 offers a discussion and concludes.

1. Literature Review

The substantive question of whether alcohol use leads to the commission of crime is complicated by the possibility that unobserved characteristics that contribute to alcohol consumption may also drive criminal activity (e.g., preferences for risk). As such, credible designs for answering this question must identify plausibly exogenous sources of variation in alcohol consumption. Most previous research has considered state changes

in alcohol policies in a quasi-experimental setting.[8] Cook and Moore (1993), for example, use within state variation in beer taxes and state/year panel data on crime rates and find a significant negative relationship between a state's beer tax and rates of rape and robbery, but no significant relationship between the beer tax and rates of assault or homicide after accounting for state and year fixed effects. Conlin et al. (2005) examine local prohibitions on alcohol established by county liquor referenda in Texas and find that, after accounting for county fixed effects and county-specific linear time trends, "wet" status is associated with *lower* drug arrests and drug deaths in Texas. Carpenter (2005, 2007) uses variation in the timing of the adoption of strict age-targeted drunk driving laws which generated sharp differences in heavy alcohol consumption (i.e., "binge" drinking) among 18-20-year-old males. Using detailed information on the age structure of arrests within police agencies over the period 1988-1997, Carpenter finds that so-called zero-tolerance laws also significantly reduced arrests for property crime and nuisance crime among young men age 18-20.

Surprisingly, only one study has focused on differences in alcohol access associated with the minimum drinking age to identify the effect of alcohol consumption on crime. Joksch and Jones (1993) studied variations in alcohol availability created by increases in states' Minimum Legal Drinking Age (MLDA) in the 1980s and found that higher MLDAs were associated with a lower incidence of vandalism and disorderly conduct (on the order of 10 percent). Combined with evidence that the MLDA predictably affected alcohol use among the affected cohorts, this is some of the most compelling evidence that alcohol use causes to the commission of these "nuisance" crimes. Importantly, Joksch and Jones (1993) do not find that a higher drinking age led to fewer assaults or other violent crimes; the reductions in crime attributable to the drinking age were concentrated only among the less-serious "nuisance" crimes.

[8] We do not review here a large literature that has examined the relationship between alcohol access and alcohol consumption, including a growing body of studies that have examined the effects of state minimum drinking age changes on age-specific alcohol consumption. With very few exceptions, research has found that state drinking age changes were predictably associated with age-specific changes in alcohol consumption (e.g., when states raised their drinking age to 21, drinking among 18-20-year-olds fell). Examples of these studies include Dee 1999; Cook and Moore 2001; and Carpenter et al. 2007. Our results using the CHIS provide new and complementary evidence on the relationship between drinking age and alcohol consumption for young adults in California.

A fair read of the literature, then, returns no consensus on the causal effect of alcohol availability and alcohol consumption on the commission of crime among studies that have taken seriously the endogeneity of alcohol consumption. Our study contributes to and advances this literature in several key respects. First, the regression discontinuity design we implement is less likely to suffer from omitted variables bias than the research designs used in the prior literature. Public policy experiments with state drunk driving laws or county and federal alcohol prohibitions, for example, may reflect unobserved population characteristics correlated with both policy preferences and the outcomes of interest. In contrast, our design identifies the effect of alcohol availability through sharp comparisons of outcomes for individuals just on either side of the age-21 threshold using a long-standing policy.[9] Second, our study analyzes data for a broader range of crimes than any previous study. Most previous research considers only one major crime type.[10] In contrast, we are able to provide credible and statistically precise evidence on the relationship between alcohol access and crime for all major types of crime. Finally, our research is the first to quantify the underlying structural relationship between alcohol consumption and crime. Specifically, we provide an implied instrumental variables estimate of the effect of alcohol access on crime by combining the first-stage estimates from CHIS with the reduced-form arrest results. Most previous work estimates only the reduced-form associations between alcohol control policies and crime/arrest outcomes.

2. Data and Methods

We use both survey and administrative data on outcomes for young adults around the legal drinking age threshold to identify the effect of alcohol access on alcohol consumption and crime. Our main survey data on alcohol consumption come from the 2001, 2003, and 2005 waves of the California Health Interview Survey (CHIS). These data are from telephone-based random-digit dialing surveys that ask a randomly chosen adult in approximately 40,000 households in each year a set of questions about demographic characteristics, health outcomes, and health behaviors – including alcohol

[9] California did not lower its drinking age from 21 in the 1970s or 1980s, unlike many other states.
[10] For example, Cook and Moore (1993) focus on violent crimes, Joksch and Jones (1999) consider nuisance crimes, and Conlin et al. (2005) focus on drug crimes.

consumption.[11] The drinking questions were included as part of the core questionnaire for the adult survey in each wave. We have obtained access to a restricted-use version of these data, which includes each interviewee's month and year of birth and exact interview date. We use that information to compute the respondent's age in months at the time of interview. We focus on adults age 19-22, inclusive.

Each CHIS wave asks a variety of questions about alcohol consumption. Individuals are first asked if they consumed any alcoholic beverages in the past month, and we create an indicator variable for past-month drinking participation. Past-month drinkers are then asked about the frequency and intensity of their alcohol consumption; and we use their answers to create additional outcome variables. First, individuals are asked on how many days they drank alcohol over the previous month, and we use this information to create an outcome measuring the proportion of past-month days on which the respondent drank alcohol. Individuals are also asked how many alcoholic beverages they consumed per day on the days they drank. We consider this measure of drinks per day to examine changes in drinking intensity at age 21. Finally, individuals are asked how many times in the past 30 days they consumed 5 or more drinks at one sitting. This outcome is commonly referred to as "heavy" or "binge" drinking in the literature, and we create an indicator variable reflecting any heavy drinking in the previous month.[12] We also create an outcome variable that reflects the proportion of days in the past month on

[11] For further technical details on the CHIS sampling methods, see Cervantes et al. (2006). Two concerns about sampling in large household surveys of this type (including CHIS) are relevant for our research. First, the CHIS sampling frame excludes individuals living in "group quarters," including college dormitories (though college students living in off-campus housing would not be excluded). Unfortunately, we do not have a direct way to identify whether someone was currently attending school at the time of interview, though we find no significant discontinuity in the likelihood of living with one's parents at age 21. Second, the increasing use of cellular phones is a concern since cell phone numbers were not included in the sample. Notably, the problem of exclusive cell phone use is a very recent phenomenon: in 2000, only 1.9 percent of households did not have a landline telephone in California. By 2005, exclusive cell phone use had reached over 11 percent. In results not reported but available upon request we found very similar estimated discontinuities in alcohol consumption when we restricted attention to data from the 2001 wave, which was before the large increase in exclusive cell phone use. Note that there is no reason to believe that these sampling issues change discretely at age 21 (e.g., cell phone service providers do not use age 21 as a relevant threshold). Finally, we note that neither of these potential sampling issues is relevant for the crime analysis since we observe the universe of statewide arrests.

[12] Note that the reference window for the drinking questions is the past month, and for some individuals who very recently turned 21, this window includes a period when the individual was age 20 and thus not old enough to legally purchase alcohol. To adjust for this problem in all of the consumption models we include a dummy variable for individuals surveyed in the first month after they turn 21. This variable is not a significant predictor of alcohol consumption, and our results are robust to the inclusion of this variable. These patterns suggest that the reporting window is not a serious problem for our analysis.

which the individual engaged in binge drinking behavior.[13] In addition to standard alcohol consumption variables, in the models below we control for demographic characteristics (race, sex) and behavioral outcomes (educational attainment, marital status, employment, health insurance). In all specifications we include the CHIS sample weights to make the results representative of the state of California.

Our main data on crime outcomes are from California's Monthly Arrest and Citation Register (MACR) for the period 2000-2006. These administrative data are a census of arrests in the state and provide us with information on the arrestee's date of birth and date of arrest, which we use to compute exact age in days. These data also provide information on the arrestee's race, sex, and the type of crime for which the individual was arrested. We follow standard conventions to separately consider arrests for violent crimes, property crimes, drug-related crimes, and crimes that are explicitly alcohol-related, such as drunkenness.[14] We again focus on young adults age 19-22.

Suppose that an increase in alcohol consumption is hypothesized to increase the probability that a person will commit a crime. Then an appropriate causal model is

$$(1) \qquad y_i = X_i\alpha + f(a) + C_i\delta + u_i ,$$

where y_i is the probability individual i will commit a crime, X_i is a vector of covariates for individual i, C_i is a measure of alcohol consumption by individual i, f(a) is the structural age profile, u_i is an idiosyncratic error, and δ is the parameter of interest. Suppose in addition that alcohol consumption is determined by a simple (first-stage) model of the form:

$$(2) \qquad C_i = X_i \beta^C + g^C(a) + D_a \pi^C + v^C_i ,$$

[13] Strictly speaking, the binge drinking question asks how many "times" the interviewee engaged in binge drinking in the previous month, not how many "days." We assume, however, that "times" is equivalent to "days" in calculating the proportion of past-month binge drinking days variable. Unfortunately, the CHIS does not measure lifetime drinking participation. This information would have been useful for assessing the extent to which the legal drinking age affects people's first ever exposure to alcohol. In other work examining national survey data we have found that very few people have their first experience with alcohol at age 21 (Carpenter and Dobkin 2007).

[14] Recall that we rely on arrest-report data instead of crime-report data (since the exact age of the offender's age at the time the crime was committed is not reported). There is only one crime type recorded per arrest. In cases where an arrest is for multiple infractions (e.g., murder and robbery), only the most severe crime (as measured by potential punishment severity) is reported on the arrest record. This means, for example, that an arrest for a combined murder/robbery is only listed as a murder arrest. We return to this issue below.

where $g^C(a)$ is the structural age profile of alcohol consumption, D_a is an indicator for being 21 or older, and the parameter π^C measures any discrete change in alcohol consumption that occurs precisely at age 21. Equations (1) and (2) imply a reduced-form model like equation (3):

$$(3) \qquad y_i = X_i \beta^Y + g^Y(a) + D_a \pi^y + v^Y_i \ ,$$

in which the reduced-form effect of reaching age 21 on outcome y is $\pi^y = \pi^C \times \delta$. The causal effect of alcohol consumption on crime, δ, can be estimated by forming the ratio of the estimated discontinuity in crime at age 21 to the estimated discontinuity in alcohol consumption.

3. Results

3.1 Alcohol Consumption - CHIS

In this section we present regression discontinuity evidence on the effects of MLDA laws on alcohol consumption using the 2001-2005 CHIS data. These results complement our previous work, which used data from the National Health Interview Survey and found evidence of large increases in alcohol consumption nationally at age 21 (Carpenter and Dobkin 2007). In Figure 1 we present the age profiles of the proportion of people who reported engaging in drinking and binge drinking over the past month. Over these monthly averages we have superimposed the fitted lines from regressions on the underlying micro data.[15] The figure reveals that about 50 percent of 20-year-olds in the CHIS reported having consumed alcohol in the previous 30 days and that there was a sharp and noticeable increase of about 20 percentage points at age 21. In the leftmost panel (first two columns) of Table 1 we present the corresponding regression estimates of the increase in the proportion of people who reported consuming any alcohol in the past month. The specification in the first column of the panel does not include any covariates other than the quadratic polynomial in age interacted with the dummy for being over 21. This regression reveals that the increase we observe in the figure is about 16 percentage points and is statistically significant.[16] In the second specification we include the

[15] The regression is a quadratic polynomial in age fully interacted with a dummy for over 21. An examination of the figure suggests that the quadratic polynomial fits the data well.

[16] Since the age variable in the regression is the number of months until (or since) the person's 21st birthday at the time of the interview, the "Over 21" dummy gives the estimate of the discrete increase in the

demographic and behavioral covariates available to us in the CHIS, which include: dummy variables for male, black, Hispanic, Asian/Pacific Islander, married, less than a high school diploma, at least some college education, currently employed, and currently with health insurance. We also include a variable indicating the respondent was interviewed in the month just after turning age 21 in the second specification. Though these covariates are strongly correlated with alcohol consumption, their inclusion has very little impact on the estimated size of the discontinuous jump at age 21.[17] In Appendix B we present evidence that for all the outcomes we examine, more flexible models such as a cubic polynomial or a local linear regression give very similar results to the quadratic specifications

Figure 1 also plots the age profile of the proportion of people who reported binge drinking at least once in the past month.[18] The regression line superimposed on these outcomes provides some evidence of an increase in binge drinking at age 21, though the jump is smaller than for past-month drinking participation. We present the corresponding regression results in the second panel of Table 1. The first specification confirms that the increase we saw in Figure 1 is about 8.9 percentage points and is marginally significant at the 10 percent level. The inclusion of covariates in the second specification has little effect on the estimate.

In Figure 2 we present the age profiles of two measures of drinking intensity: the proportion of days in the past month on which the respondent reported drinking or binge drinking. The figure reveals a large increase at age 21 in the proportion of days people reported drinking in the past month. In the third panel of Table 1 we present the corresponding regression estimates, which confirm that the increase we observe in the figure is about 5 percentage points and is statistically significant. This is a very large increase in drinking intensity given that people just under 21 reported drinking on 7 to 8 percent of days. In the second specification we observe that the inclusion of covariates slightly increases the magnitude of the jump at age 21. The model with covariates

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outcome that occurs at age 21. The alcohol consumption regressions are weighted to reflect the California population. The weights were not used in the creation of the figures because they significantly increase the variance as some of the individuals in the survey have very high weights.

[17] Below, we directly show that these covariates are smoothly distributed across the age-21 discontinuity.

[18] Note that past-month abstainers are included in our analyses of all alcohol consumption outcomes, except where noted. This is appropriate because we are interested in the effects of age-based restrictions on population outcomes (alcohol consumption and crime).

suggests there is an approximately 70 percent increase in the number of days on which people drink. The figure shows less compelling evidence of an increase in binge drinking. Consistent with this, the corresponding regression estimates in the fourth panel of Table 1 are statistically insignificant at conventional levels and smaller than the results for the proportion of days drinking any alcohol.

Given the imprecision of the two estimates of the change in binge drinking, we take additional steps to determine whether drinking intensity changes at age 21. In Figure 3 we present the age profile of the number of drinks individuals reported consuming on the days they drank any alcohol in the previous month. Specifically, Figure 3 plots the average reported drinks per day among the sample of past-month drinkers. This outcome shows some evidence of a decline in drinking intensity at age 21 among drinkers, which is probably due to the change in the composition of drinkers documented in Figure 1. The corresponding regression estimates in the rightmost panel of Table 1 confirm that the reduction in the number of drinks per day among drinkers is sizable (an estimated 22 percent less, or .66 fewer drinks per day on days on which they drank) though only marginally significant. We present further evidence on this question in Figure 4, which plots a histogram of reported drinks per day for 20- and 21-year-olds. Figure 4 shows that – apart from the 15-16 percentage point difference in the likelihood of being a past-month drinker documented in Figure 1 – the distribution of average drinks per day shifts slightly to the left after people turn 21. The evidence in Figures 3 and 4 and the rightmost panel of Table 1 suggests that the most important change in drinking habits after turning 21 is that people drink on more days (i.e., there is an increase in drinking exposure). The average drinking intensity declines slightly, so the increase in arrests we document below is probably not the result of a disproportionate increase in heavy drinking by young adults.[19]

Finally, before examining the increase in crime that results from the greater alcohol consumption documented above, we directly test the hypothesis that there are other changes occurring at age 21 that could confound our analysis. That the regression estimates are robust to the inclusion of covariates (i.e., moving from specification 1 to 2

[19] In prior work we show that only a very small portion of the population has a first experience with alcohol shortly after turning 21 (Carpenter and Dobkin 2007).

in Table 1) suggests that the observable characteristics are smoothly distributed across the discontinuity but it is worth testing this directly. In Table 2 we document that demographic characteristics and employment status, insurance status, and educational attainment all evolve smoothly through the age-21 threshold.[20] Specifically, we estimate models similar to specification 1 in Table 1 but replace the measures of alcohol consumption with the demographic and behavioral characteristics to see if these characteristics exhibit sharp jumps at age 21. We find no evidence of a practically or statistically significant discrete change at age 21 in any of these characteristics. While this is not surprising for the demographic characteristics, the null findings for employment and health insurance are comforting. That these observable characteristics are smoothly distributed across the discontinuity suggests that the unobservable characteristics are also distributed smoothly across the discontinuity and reduces our concerns about the possibility of omitted variables bias.

3.2 Crime – California MACR

In this section we evaluate how the increase in alcohol consumption documented above affects arrest rates. We begin with Figure 5, which presents the age profile of arrests rates in California for the 2000 to 2006 period by major crime type.[21] Using the main left axis scale, we present results for alcohol-related arrests, arrests for illegal drug possession or sale, violent crime, property crime, and arrests for city/county ordinance violations such as public urination and possession of alcohol in public parks.[22] Figure 5

[20] The corresponding age profiles are available upon request.

[21] For all of the arrest figures we have dropped observations for birthdays and the day immediately after to account for the "birthday" effects in arrests (i.e., the greater likelihood that people will be arrested for alcohol-related violations on their birthday or the day after – see Appendix A). To avoid confounding these birthday effects with the effect of easier access to alcohol, we omit the birthday observations from the figures. In the regressions, we account for birthday effects by including a dummy variable for each birthday observation and the day after.

[22] We also examined non-rape sex crimes such as lewd/lascivious behavior, but these data were too noisy to be meaningful, as arrests for such crimes are rare. We follow the reporting standards of the Federal Bureau of Investigation (FBI) to define violent crimes as including: murder, manslaughter, rape, robbery, and assault. We define property crimes to include: burglary, larceny, motor vehicle theft, stolen property, and vandalism. We define alcohol-related arrests to include: driving under the influence (DUI), liquor law violations, drunkenness, disorderly conduct, and vagrancy. We define drug-related arrests to include violations pertaining to the possession and/or sale of marijuana, narcotics, and other dangerous drugs. "County ordinance violations" include arrests for violations of city, county, and municipal laws. These generally include arrests for activities that are not well defined as violations of the penal code or business and professions code but that localities decide are illegal. Although we do not observe more detailed

provides strong visual evidence of discontinuous increases in arrests for violent crimes and alcohol-related offenses, while there is little visual evidence of a change in arrest rates for property crime or for the possession or sale of illegal drugs. Arrests for county ordinance violations also exhibit a noticeable increase at the age-21 threshold.[23]

We present estimates of the discontinuous increase in these crime types in Table 3. The dependent variable in the regression (and all subsequent regressions) is the log of the counts of arrests occurring X days before or after an individual's 21st birthday, and we estimate the model over the 1,455 days between ages 19 and 22, inclusive. The coefficient of interest is the Over 21 indicator (the estimate of π^y from equation 3) and can, for small changes, be interpreted as the percentage change in arrests at age 21. The estimates in the first column under each outcome are from models that fit a quadratic polynomial to the age profile of arrests. In the second column under each outcome we add dummy variables for birthdays and the day after to account for birthday effects.[24] In Appendix C we document that more flexible models, cubic polynomial and local linear regression, give very similar results. We find a 6 percent increase in overall arrests. This estimate is statistically significant and insensitive to model specification.[25] Combined with our estimate of the effect of alcohol access at age 21 on the proportion of days on which people drink, we estimate an elasticity of about 0.08, suggesting that a 10 percent increase in the proportion of days on which people drink increases arrests by almost 1 percent. With respect to the control variables in Table 3, we find that arrest rates are dropping with age for most crimes and that there are pronounced "birthday effects". However, inclusion of the birthday dummies only slightly reduces the estimated size of each age-21 discontinuity.

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information about the exact nature of the violations, these crimes generally include violations such as social host liability violations, public urination, and possession of alcohol at beaches and public parks, among others.

[23] There are a small number of arrests that do not fit into any of these categories. These arrests show a modest increase at age 21.

[24] In Appendix A we plot the counts of arrests in the days around a person's 20th, 21st, and 22nd birthday. This figure reveals that there is a strong birthday effect on the birthday itself and the day immediately after.

[25] In results not reported but available upon request we also tested a model that added a cubic in age and its interaction with the Over 21 indicator for the arrest outcomes. We found that the coefficients on the cubic term were generally statistically insignificant; throughout, we refer to the specification in Column 2 of each panel (with the quadratic in age and birthday dummies) as our preferred specification. The figures also suggest that the model with the quadratic in age is sufficiently flexible to fit the age profile of arrests.

The remaining entries in Table 3 follow the same format and present results for the other major crime categories. The results in the second panel of Table 3 confirm that violent crime arrests exhibit a significant discontinuity at age 21 of 5.6-6.6 percent. We obtain a fairly precise zero for arrests for property crime and for drug possession and sale. Alcohol-related arrests in the fourth panel exhibit a statistically significant 12-14 percent increase at age 21. Arrests for city and county ordinance violations exhibit a remarkably large increase of over 40 percent at age 21.[26]

In Figures 6-8 we present results for specific crimes within the broad crime type categories, including: violent (Figure 6), property (Figure 7), and alcohol-related (Figure 8).[27] The associated regression estimates follow the format of previous tables and are presented in Tables 4-6, respectively. For violent crime arrests in Figure 6 we find visual evidence of increases at age 21 for aggravated assaults and other assaults, while there is somewhat noisier evidence of an increase in arrests for robbery.[28] The other violent crime arrests are noisy and/or relatively stable across the age-21 threshold. The

[26] We also found that "other" arrests (including, for example, arrests for failure to appear in court) exhibited a statistically significant discontinuous increase at age 21 of about 3.4 percent (not reported but available upon request).

[27] We do not report drug-related arrests in a disaggregated fashion because there were no economically or statistically significant discontinuities in those arrests at age 21.

[28] The definition of aggravated assault varies by jurisdiction but is usually differentiated from simple or "other" assault by characteristics such as: the offender's intent, the extent of injury to the victim, and whether a deadly weapon was used. A potentially problematic institutional feature for our analysis is that California's minimum purchase age for handguns is also 21 – the only other substantive institutional feature of the state that also changes discretely at the MLDA threshold. Unlike access to alcohol, however, there are several other requirements that individuals over age 21 must meet in order to buy a handgun. These include: satisfying a 10 day waiting period, completing a handgun safety class, and completing a safe handling demonstration. Despite these requirements, a natural question is: could our findings on aggravated assaults be attributable to discontinuities in handgun ownership as opposed to alcohol consumption? (Note that handgun ownership cannot plausibly be responsible for the other crime types we consider.) Fortunately, the 2001 wave of the CHIS included several questions about gun ownership, including handgun ownership. We estimated models of handgun ownership that mirrored our alcohol consumption models and found no evidence that handgun ownership or the number of handguns owned by young adults increase discretely at age 21. The estimated discontinuity in the baseline model with a quadratic in relative age was -.077 with a standard error of .050. Adding covariates and a cubic in age did not materially change the estimated discontinuity (i.e. it remained sizably *negative*). The estimated discontinuity in the presence of any guns in the home (including rifles) was also negative and statistically insignificant. Importantly, we continued to find evidence of significant discrete increases in drinking participation and intensity even when we restricted attention to the 2001 CHIS wave (which was the only one to include the handgun questions). The estimated discontinuity in past month drinking in 2001, for example, was .243 with a standard error of .087. All of these results are available upon request. Our findings that assaults increase sharply at the drinking age provide evidence consistent with recent research by Dahl and Della Vigna (2008), who find that increases in audiences for violent movies result in fewer same-day assaults. They speculate that reduced alcohol consumption is one of the mechanisms.

associated regression evidence for violent crime arrests in Table 4 confirm the visual results from Figure 6: we estimate a marginally significant increase in arrests for robbery of 6.5 percent, and we find statistically precise increases in aggravated and other assaults at age 21 on the order of 4.5 and 7.4 percent, respectively.[29] As can be seen in Appendix D the majority of arrests for assault and the majority of the increase in arrests from age 20 to age 21 are occurring at people's residences. This suggests that the increase in assaults we documented above is not due to a discrete increase in drinking in public places (such as bars) where the probability of an arrest conditional on committing an assault may be higher. For the other violent crime types we find smaller and statistically insignificant estimates of the discontinuity at age 21. Because violent crimes – particularly murder, manslaughter, and rape – are relatively uncommon, we are limited somewhat by statistical precision when we disaggregate by specific violent crime type.[30]

Figure 7 presents the age profile for specific property crimes. Recall that the overall age-21 effect for property crime arrests was smaller than for either violent crimes or alcohol-related crimes and was statistically indistinguishable from zero; as such, the lack of visual evidence in Figure 7 of increases in the rates of specific property crime types is not particularly surprising. We find some visual evidence of increases for motor vehicle theft, though these outcomes are fairly noisy. This is also borne out in the regression evidence in Table 5: although the magnitude of the age-21 discontinuity is sizable for motor vehicle theft (4.8 percent) and stolen property (3.2 percent), they are not statistically significant.[31]

Figure 8 disaggregates arrests for alcohol-related crimes. The visual evidence here is particularly compelling and reinforces the evidence from the survey that granting access to alcohol at age 21 is associated with sharp increases in alcohol consumption.

[29] An analysis of arrest records for the entire 1979-2007 period show a discontinuity in assaults of 7.1 percent with a SE of 1.6.

[30] In results not reported but available upon request, we also estimated models using the entire years of arrest data available to us (1979-2007). Recall that because our first-stage estimates only span the range 2001-2005, we focus on 2000-2006 arrest estimates for comparability. In the longer sample, however, we found stronger evidence for robbery. The point estimate was 0.071 and the standard error was 0.016. The other estimated discontinuities (for rape, murder, and manslaughter) are all statistically insignificant in the full sample.

[31] Focusing attention on the longer period 1979-2006 did not change the finding that the estimated discontinuities for property crime were much smaller than the associated violent crime discontinuities at the relevant age threshold.

We find that arrests for driving under the influence increased across the age distribution and jumped very substantially at age 21. A similarly stark jump appears for drunkenness and drunk protective custody arrests, while arrests for liquor law violations fell to just a fraction of the age-20 mean at the age-21 threshold.[32] We also find evidence of a discontinuous increase at age 21 for disorderly conduct arrests, while there is no evidence of an increase at age 21 in arrests for vagrancy. The associated regression evidence in Table 6 confirms what we see in Figure 8. We find significant increases in arrests at age 21 in our preferred specification for disorderly conduct, drunkenness, and driving under the influence, though we find no important change in arrests for vagrancy.[33]

Table 6 also reveals evidence of a large and statistically precise decline in arrests for liquor law violations, consistent with Figure 8. These changes in liquor law violations – including (but not limited to) "minor in possession" arrests – merit further discussion because they raise the possibility that the observed positive discontinuities in other alcohol-related arrests are, in part, reporting artifacts. That is, because a 20.9-year-old who consumes alcohol can be charged with illegal underage alcohol consumption but a 21-year-old cannot be similarly charged (since she is "of age"). The change in the statute under which a person is arrested might create the appearance of an increase in arrests for certain types of crimes even in the absence of any change in behavior. However, there are several pieces of evidence that suggest that this is only a minor problem. First, offenses are coded in order of decreasing severity. Because liquor law violations are among the least severe offenses, none of our results for any of the major crime types (violent, property, or drug crimes) are affected by this reporting issue. In fact, the reallocation of liquor law offenses for individuals age 21 and over can only affect DUI, disorderly conduct, and county ordinance violations. The discontinuity in drunkenness arrests is unaffected by this issue because in the coding scheme used in the MACR the drunkenness code dominates the liquor law violations code. Second, note that the estimate of the overall increase in alcohol related crimes is if anything biased slightly

[32] Liquor law violations refer to ordinances prohibiting the manufacture, sale, transport, furnishing, and possession of intoxicating liquor. The possession of alcohol by minors is included here. Note that "of age" individuals who supply alcohol to younger individuals can also be arrested for liquor law violations; this is why arrests for liquor law violations among 21 and 22 year olds are not exactly zero.

[33] The crime of vagrancy generally refers to the act of wandering around from place to place without any means of support. In some jurisdictions, it is similar to the violation of loitering.

down by this issue. Overall, then, there is clear evidence that easier access to alcohol has real behavioral effects on alcohol-related criminal behavior and arrests.

4. Discussion and Conclusion

We used the discontinuous change in alcohol availability associated with turning 21 in the U.S. to identify the effect of alcohol access on alcohol consumption and arrests for all the major types of crimes. The RD design we implemented provides straightforward, intuitive, and visually assessable estimates of the effects of easier alcohol access on drinking and crime. Unlike previous research using changes in alcohol control policies, the RD design is not likely to be biased by either unobserved individual or population preferences. We first documented that alcohol consumption jumps sharply at age 21 among young adults in California: individuals who had just turned 21 reported drinking on about 70 percent more days than individuals who were just about to turn 21. We found that this increase in alcohol consumption resulted in an increase in many different types of crime, including alcohol-related crimes and violent crimes. Contrary to previous research, we did not find evidence of a significant discontinuity in arrests at age 21 for property or drug-related crimes. Combining our estimates of the age-21 discontinuity in alcohol consumption (drinking days) and crime (total arrests), we estimate an overall elasticity of about 0.08, suggesting that a 10 percent increase in drinking days increases the probability of arrest by almost 1 percent.

There are a number of issues that need to be considered when interpreting the estimates. The first issue has to due with the possibility that alcohol consumption in the CHIS is underreported by individuals under age 21 owing to desirability bias (since consuming alcohol is illegal for people under 21). Such a discontinuous change in reporting bias would cause our estimates of the increase in alcohol consumption that occurs at age 21 to be biased upwards (with a corresponding *downward* bias to our implied instrumental variables estimate of δ). Despite this potential concern, several empirical patterns we observe indicate that our estimates of the discontinuities in alcohol consumption at age 21 are not severely biased. For example, we find very large discrete increases in a variety of alcohol-related crimes (e.g., driving under the influence,

drunkenness) at age 21. These findings using administrative data provide strong evidence that the changes in alcohol consumption we observe are real (since the alcohol-related arrest results are not subject to the same concerns about desirability bias). Also, as we show in Figure 9, though both drunkenness arrests and the proportion of days on which people drink rise sharply at age 21, their ratio does not change discretely at age 21. This is inconsistent with the existence of substantial desirability bias in the reports of alcohol consumption. Finally, in the age range we studied there were no individuals who refused to answer the screening question about past-month alcohol consumption or who indicated that they did not know whether they had consumed alcohol in the previous month. If endogenous underreporting were a serious problem, we might expect information on alcohol consumption to be missing for under-age individuals.

A second interpretation issue concerning our results is that because our data are for arrests and not crimes it is possible that part of the increase in arrests we document is due to an increase in the probability of arrest conditional on committing a crime (e.g., alcohol consumption may make for "sloppier" criminals who leave fingerprints, etc.). However, the fact that we find effects for some types of crimes but not others suggests this is a minor problem, since it is difficult to imagine that alcohol would make people more careless when committing assault (where we find large discontinuities at age 21) but not more careless when committing larceny (where we find no effects). Moreover, Appendix D shows that changes in drinking venue at age 21 – which could also affect the probability of arrest conditional on crime commission – are not large enough to materially affect our estimated discontinuities in assault arrests.

Third, it is possible that the MLDA laws cause people to delay their drinking. This does not bias the IV estimates, but if there is substantial delay it suggests that it will be hard to use these estimates to predict the effect on crime of a reduction in the drinking age. One way to assess this problem is to note that if there were substantial delay we would expect to see a pronounced increase at age 21 in drinking and alcohol-related crime that drops off fairly rapidly. We do not see much evidence of this in Figures 1, 2, and 5. Instead, these figures show evidence of a persistent change in drinking behavior and arrest rates, both of which are inconsistent with a change in timing as the only explanation.

A final issue is that learning may substantially change the relationship between alcohol consumption and criminal behavior, effectively biasing our IV estimate upward. This is unlikely to be a serious concern since very few people have their first experience with alcohol at 21 (Carpenter and Dobkin 2007). Moreover, Figure 9 – which shows the age profile of the ratio between two kinds of alcohol-related arrests (DUI and drunkenness) and the fraction of days that people reported drinking in the past month (all three of which exhibited sharp discontinuities at age 21) – provides more direct evidence on the learning hypothesis.[34] If learning is important, then we would expect the age profile of the ratio of these arrests to the proportion of past-month drinking days to be rapidly downward sloping with an upward jump at age 21 when the proportion of inexperienced drinkers abruptly increases. Instead, we find that the age profiles of these ratios are only slightly downward sloping across the studied age range and do not increase at age 21. The lack of an abrupt increase in the ratios at age 21 suggests that any learning that occurs is a function of age rather than experience with alcohol. This suggests that a higher minimum drinking age results in substantially less drinking and criminal activity than would occur with a lower drinking age.

Overall, our results offer the most comprehensive evidence to date that MLDA laws substantially reduce alcohol consumption and that alcohol consumption causes the commission of nuisance and violent crime. These results have direct implications for policy: they imply that stricter enforcement of existing minimum drinking ages (and presumably other alcohol control policies that increase the costs of drinking) would reduce alcohol consumption and subsequently the commission of crime. Given that under-age drinking rates in the U.S. remain very high, and given the fact that the age profiles of crime and arrests peak around the ages we study, our results suggest wide latitude at effecting meaningful crime reduction. Finally, our research suggests that recent state proposals to reduce the minimum drinking age would have large societal costs in terms of increased nuisance and violent crime activity.

[34] A drunkenness arrest occurs when an individual is extremely intoxicated such that mental and physical faculties are seriously impaired. Also note that drunkenness arrests are not susceptible to concerns about changes in reporting behaviors at age 21, since drunkenness is a more serious offense (in terms of potential punishment severity) than a standard liquor law violation. This means that there are not mechanical differences in the decision rules regarding coding of these arrests at any point across our age range (i.e., the arrest effects are truly behavioral, not reporting artifacts).

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Table 1: Drinking and Binge Drinking in the Past Month

	Drank Last Month		Binged Last Month		Proportion of Days Drank		Proportion of Days Binged		Number of Drinks Cond. on Drinking	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Over 21	0.1624	0.1675	0.0884	0.0874	0.0496	0.0566	0.0119	0.0146	-0.7215	-0.6620
	[0.0542]***	[0.0541]***	[0.0473]*	[0.0492]*	[0.0168]***	[0.0169]***	[0.0091]	[0.0094]	[0.3975]*	[0.3944]*
Age	0.0005	0.0021	-0.0044	-0.0017	-0.0001	0.0010	-0.0009	-0.0002	0.0351	0.0349
	[0.0074]	[0.0073]	[0.0063]	[0.0062]	[0.0018]	[0.0018]	[0.0011]	[0.0011]	[0.0610]	[0.0592]
Age Sq	-0.0002	-0.0001	-0.0003	-0.0002	0.0000	0.0000	-0.0001	0.0000	0.0016	0.0012
	[0.0003]	[0.0003]	[0.0003]	[0.0002]	[0.0001]	[0.0001]	[0.0000]	[0.0000]	[0.0023]	[0.0022]
Age*Over21	-0.0021	-0.0051	0.0022	-0.0008	-0.0021	-0.0050	0.0003	-0.0010	-0.0054	0.0030
	[0.0104]	[0.0108]	[0.0091]	[0.0100]	[0.0031]	[0.0034]	[0.0018]	[0.0020]	[0.0747]	[0.0800]
Age Sq*Over 21	0.0002	0.0002	0.0003	0.0002	0.0002	0.0002	0.0001	0.0001	-0.0023	-0.0023
	[0.0004]	[0.0004]	[0.0004]	[0.0004]	[0.0001]	[0.0001]	[0.0001]	[0.0001]	[0.0030]	[0.0029]
Constant	0.5011	0.4556	0.2355	0.2313	0.0809	0.0711	0.0288	0.0381	3.6595	2.9529
	[0.0393]***	[0.0574]***	[0.0323]***	[0.0474]***	[0.0100]***	[0.0170]***	[0.0061]***	[0.0119]***	[0.3359]***	[0.4288]***
Covariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Obs	5560	5560	5560	5560	5560	5560	5560	5560	3197	3197
R-squared	0.04	0.10	0.01	0.08	0.03	0.11	0.00	0.07	0.01	0.12

Notes: The dependent variable is a dummy for drinking or binge drinking. All regressions include a quadratic in age fully interacted with a dummy for being over 21. Only people 19 to 22 years old at the time of the interview are included in the regression. Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1% Covariates include dummies for male, black, Latino, API, interview in month of birth, year of interview, month of interview, no HS diploma, some college, married, employed, and has health insurance. People who report that they did not drink in the last month are dropped from the regressions examining the number of drinks people typically report consuming in one day.

Table 2: Age Profile of Potential Confounders

	Married	Employed	No HS Diploma	HS Graduate	Some College	Health Insurance	White	Male
Over 21	-0.0065	0.0037	0.0176	-0.0334	0.0158	0.0058	-0.0085	0.0520
	[0.0256]	[0.0520]	[0.0381]	[0.0539]	[0.0540]	[0.0488]	[0.0503]	[0.0545]
Age	-0.0012	-0.0035	0.0057	-0.0002	-0.0057	-0.0002	-0.0074	-0.0038
	[0.0032]	[0.0071]	[0.0052]	[0.0073]	[0.0070]	[0.0065]	[0.0070]	[0.0074]
Age Sq	-0.0001	-0.0004	0.0002	0.0004	-0.0006	0.0001	-0.0003	-0.0001
	[0.0001]	[0.0003]	[0.0002]	[0.0003]	[0.0003]**	[0.0003]	[0.0003]	[0.0003]
Age*Over21	0.0075	0.0062	-0.0117	-0.0065	0.0182	-0.0063	0.0150	-0.0118
	[0.0054]	[0.0100]	[0.0073]	[0.0102]	[0.0102]*	[0.0093]	[0.0097]	[0.0104]
Age Sq*Over 21	0.0001	0.0003	0.0000	-0.0003	0.0003	0.0001	0.0000	0.0008
	[0.0002]	[0.0004]	[0.0003]	[0.0004]	[0.0004]	[0.0004]	[0.0004]	[0.0004]*
Constant	0.0688	0.6507	0.1539	0.4285	0.4137	0.7138	0.3582	0.5055
	[0.0172]***	[0.0369]***	[0.0277]***	[0.0385]***	[0.0385]***	[0.0353]***	[0.0366]***	[0.0392]***
Obs	5560	5560	5560	5560	5560	5560	5560	5560
R-squared	0.03	0.01	0.00	0.04	0.04	0.00	0.00	0.00

Notes: See notes to Table 1. The dependent variable for each regression is at the top of the column.

Table 3: Age Profile of Arrests in California, 2000-2006

	All Crimes		Violent		Property		Alcohol Related		Drug Possession or Sale		Violation of City or County Ordinance	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Over 21	0.068	0.060	0.066	0.056	0.009	0.007	0.141	0.123	0.008	0.004	0.415	0.407
	[0.007]***	[0.004]***	[0.014]***	[0.012]***	[0.013]	[0.013]	[0.016]***	[0.010]***	[0.010]	[0.010]	[0.027]***	[0.026]***
Age	-0.073	-0.070	-0.019	-0.017	-0.158	-0.156	-0.052	-0.044	-0.049	-0.046	-0.028	-0.020
	[0.008]***	[0.008]***	[0.021]	[0.021]	[0.021]***	[0.021]***	[0.018]***	[0.017]***	[0.017]***	[0.017]***	[0.049]	[0.049]
Age Squared	-0.024	-0.022	-0.009	-0.008	0.009	0.010	-0.049	-0.045	-0.014	-0.012	0.004	0.008
	[0.004]***	[0.004]***	[0.011]	[0.011]	[0.010]	[0.010]	[0.009]***	[0.008]***	[0.008]*	[0.008]	[0.024]	[0.024]
Age*Over 21	-0.016	-0.007	-0.023	-0.009	0.024	0.025	-0.046	-0.025	-0.026	-0.024	-0.185	-0.185
	[0.016]	[0.011]	[0.032]	[0.029]	[0.029]	[0.029]	[0.036]	[0.024]	[0.024]	[0.024]	[0.062]***	[0.062]***
Age Sq*Ov 21	0.035	0.028	0.007	-0.001	-0.004	-0.007	0.081	0.066	0.018	0.015	0.012	0.005
	[0.007]***	[0.005]***	[0.015]	[0.014]	[0.014]	[0.014]	[0.016]***	[0.012]***	[0.012]	[0.012]	[0.031]	[0.030]
20th Birthday		0.214		0.229		0.095		0.386		0.142		0.559
		[0.002]***		[0.005]***		[0.004]***		[0.004]***		[0.004]***		[0.010]***
20th Birthday + 1		0.133		-0.056		0.122		0.328		0.116		0.226
		[0.002]***		[0.005]***		[0.004]***		[0.004]***		[0.004]***		[0.010]***
21st Birthday		0.360		0.336		0.207		0.721		0.163		0.376
		[0.003]***		[0.008]***		[0.008]***		[0.007]***		[0.007]***		[0.015]***
21st Birthday +1		0.334		0.433		-0.063		0.806		0.107		0.129
		[0.003]***		[0.008]***		[0.008]***		[0.007]***		[0.007]***		[0.015]***
22nd Birthday		0.157		0.096		0.039		0.392		0.054		-0.059
		[0.002]***		[0.004]***		[0.005]***		[0.004]***		[0.004]***		[0.008]***
22nd Birthday +1		0.212		0.161		-0.013		0.468		0.114		0.436
		[0.002]***		[0.004]***		[0.005]***		[0.004]***		[0.004]***		[0.008]***
Constant	7.004	7.005	4.982	4.983	5.137	5.138	5.502	5.504	5.437	5.438	3.505	3.507
	[0.003]***	[0.003]***	[0.009]***	[0.009]***	[0.010]***	[0.010]***	[0.007]***	[0.007]***	[0.007]***	[0.007]***	[0.022]***	[0.022]***
Observations	1455	1455	1455	1455	1455	1455	1455	1455	1455	1455	1455	1455
R-squared	0.57	0.67	0.06	0.09	0.83	0.83	0.37	0.51	0.4	0.41	0.35	0.36

Notes: Each observation is the log of the count of the number of arrests occurring x days before a person's 21st birthday. All regressions are estimated off death counts for people 19 to 22 years old. The first column of each pair corresponds to the results presented in the matching figure. Standard errors are in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. The FBI offense codes for each crime category are as follows: Violent includes FBI offense codes (01A,01B,02,03,04,08); Property includes (05,06,07,13,14); Alcohol Includes (21,22,23,24,25) and CJSC Code 47. City and County Ordinances include "Public Urination", "Alcohol on Beaches", "Alcohol in Park", "Alcohol Consumption on Public Streets" and other offenses. We have not included the residual category in this table, it has a 4 percent increase in arrests.

Table 4: Age Profile of Arrests for Violent Crimes in California, 2000-2006

	Murder		Manslaughter		Rape		Robbery		Aggravated Assault		Other Assault	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Over 21	0.013	0.015	0.043	0.039	0.008	0.000	0.064	0.065	0.052	0.045	0.088	0.074
	[0.084]	[0.084]	[0.054]	[0.054]	[0.081]	[0.082]	[0.036]*	[0.036]*	[0.019]***	[0.019]**	[0.025]***	[0.023]***
Age	-0.092	-0.081	-0.181	-0.179	0.013	0.022	-0.268	-0.267	0.020	0.020	0.017	0.020
	[0.134]	[0.135]	[0.089]**	[0.090]**	[0.136]	[0.136]	[0.055]***	[0.056]***	[0.031]	[0.031]	[0.040]	[0.040]
Age Squared	0.011	0.016	-0.082	-0.081	0.019	0.024	-0.035	-0.034	-0.025	-0.025	0.007	0.009
	[0.064]	[0.064]	[0.044]*	[0.044]*	[0.066]	[0.066]	[0.026]	[0.026]	[0.015]	[0.015]	[0.020]	[0.020]
Age*Over 21	-0.009	-0.020	0.253	0.256	0.039	0.044	0.043	0.041	-0.018	-0.004	-0.062	-0.041
	[0.191]	[0.191]	[0.125]**	[0.126]**	[0.189]	[0.191]	[0.086]	[0.087]	[0.044]	[0.044]	[0.058]	[0.054]
Age Sq*Ov 21	-0.016	-0.021	0.029	0.027	-0.085	-0.096	0.046	0.046	0.023	0.017	-0.015	-0.026
	[0.091]	[0.091]	[0.061]	[0.061]	[0.092]	[0.093]	[0.042]	[0.043]	[0.022]	[0.021]	[0.028]	[0.026]
20th Birthday		0.212		-0.258		1.194		0.042		0.180		0.300
		[0.027]***		[0.020]***		[0.029]***		[0.012]***		[0.007]***		[0.008]***
20th Birthday + 1		0.772		0.436		-0.310		0.085		-0.172		-0.022
		[0.027]***		[0.020]***		[0.029]***		[0.012]***		[0.007]***		[0.008]***
21st Birthday		-1.092		0.495		0.098		-0.092		0.291		0.556
		[0.059]***		[0.039]***		[0.058]*		[0.027]***		[0.013]***		[0.015]***
21st Birthday +1		0.518		-0.199		0.385		0.071		0.331		0.663
		[0.059]***		[0.039]***		[0.057]***		[0.027]***		[0.013]***		[0.015]***
22nd Birthday		-0.985		0.473		-0.302		-0.003		0.057		0.238
		[0.029]***		[0.020]***		[0.027]***		[0.015]		[0.007]***		[0.008]***
22nd Birthday +1		-0.292		-0.220		0.103		0.062		0.057		0.363
		[0.029]***		[0.020]***		[0.027]***		[0.015]***		[0.007]***		[0.008]***
Constant	1.075	1.077	0.159	0.159	0.999	1.001	2.86	2.86	4.259	4.259	3.945	3.945
	[0.059]***	[0.059]***	[0.038]***	[0.038]***	[0.058]***	[0.058]***	[0.024]***	[0.024]***	[0.013]***	[0.013]***	[0.017]***	[0.017]***
Observations	1455	1455	1455	1455	1455	1455	1455	1455	1455	1455	1455	1455
R-squared	0.05	0.06	0.01	0.01	0.01	0.01	0.43	0.43	0.2	0.21	0.05	0.08

Notes: See notes to Table 3. Murder, Manslaughter, and Rape are estimated from the log(count+1) because they have 139, 1037, and 188 observations respectively that are 0.

Table 5: Age Profile of Arrests for Property Crimes in California, 2000-2006

	Burglary		Larceny		MV Theft		Stolen Property		Vandalism	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Over 21	-0.012	-0.011	-0.007	-0.009	0.048	0.048	0.034	0.032	0.029	0.017
	[0.025]	[0.026]	[0.020]	[0.021]	[0.030]	[0.030]	[0.036]	[0.037]	[0.039]	[0.039]
Age	-0.094	-0.092	-0.214	-0.210	-0.152	-0.147	-0.220	-0.225	-0.102	-0.099
	[0.041]**	[0.041]**	[0.033]***	[0.033]***	[0.049]***	[0.049]***	[0.060]***	[0.060]***	[0.058]*	[0.058]*
Age Squared	0.041	0.042	-0.007	-0.006	-0.006	-0.003	-0.050	-0.052	0.056	0.058
	[0.020]**	[0.020]**	[0.015]	[0.015]	[0.023]	[0.023]	[0.028]*	[0.028]*	[0.027]**	[0.027]**
Age*Over 21	-0.072	-0.077	0.109	0.110	-0.011	-0.014	0.138	0.146	-0.066	-0.051
	[0.059]	[0.059]	[0.047]**	[0.047]**	[0.070]	[0.071]	[0.084]*	[0.085]*	[0.092]	[0.091]
Age Sq*Ov 21	-0.038	-0.038	0.002	-0.002	0.033	0.031	0.047	0.049	-0.053	-0.061
	[0.029]	[0.029]	[0.022]	[0.022]	[0.034]	[0.035]	[0.041]	[0.041]	[0.045]	[0.044]
20th Birthday		0.144		0.132		0.095		-0.213		0.192
		[0.008]***		[0.006]***		[0.010]***		[0.012]***		[0.012]***
20th Birthday + 1		0.038		0.213		0.324		-0.274		0.070
		[0.008]***		[0.006]***		[0.010]***		[0.012]***		[0.012]***
21st Birthday		0.104		0.191		0.102		0.138		0.685
		[0.018]***		[0.014]***		[0.021]***		[0.025]***		[0.029]***
21st Birthday +1		-0.232		-0.115		-0.186		0.185		0.370
		[0.018]***		[0.014]***		[0.021]***		[0.025]***		[0.028]***
22nd Birthday		0.228		-0.340		-0.010		0.312		0.405
		[0.009]***		[0.008]***		[0.011]		[0.013]***		[0.015]***
22nd Birthday +1		-0.254		0.048		-0.285		0.221		0.405
		[0.009]***		[0.008]***		[0.011]***		[0.013]***		[0.015]***
Constant	3.799	3.799	4.093	4.094	3.315	3.316	2.875	2.874	2.909	2.909
	[0.018]***	[0.018]***	[0.015]***	[0.015]***	[0.022]***	[0.022]***	[0.026]***	[0.026]***	[0.026]***	[0.026]***
Observations	1455	1455	1455	1455	1455	1455	1455	1455	1455	1455
R-squared	0.58	0.58	0.68	0.69	0.32	0.32	0.2	0.21	0.38	0.39

Notes: See notes to Table 3.

Table 6: Age Profile of Arrests for Alcohol-Related Crimes in California, 2000-2006

	DUI		Liquor Laws		Drunkenness		Disorderly Conduct		Vagrancy	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Over 21	0.351	0.342	-1.598	-1.606	0.430	0.399	0.288	0.268	0.047	0.036
	[0.015]***	[0.014]***	[0.044]***	[0.044]***	[0.031]***	[0.022]***	[0.080]***	[0.080]***	[0.084]	[0.085]
Age	0.046	0.054	-0.292	-0.287	-0.022	-0.013	-0.245	-0.233	0.117	0.117
	[0.026]*	[0.025]**	[0.036]***	[0.036]***	[0.036]	[0.035]	[0.125]*	[0.126]*	[0.133]	[0.134]
Age Squared	-0.064	-0.060	-0.098	-0.095	-0.011	-0.007	-0.012	-0.006	0.073	0.073
	[0.013]***	[0.013]***	[0.017]***	[0.017]***	[0.017]	[0.017]	[0.058]	[0.058]	[0.063]	[0.063]
Age*Over 21	-0.036	-0.031	-0.350	-0.351	-0.220	-0.179	-0.003	0.011	-0.309	-0.289
	[0.035]	[0.032]	[0.106]***	[0.106]***	[0.067]***	[0.050]***	[0.178]	[0.178]	[0.191]	[0.193]
Age Sq*Ov 21	0.067	0.058	0.278	0.274	0.082	0.057	0.049	0.033	-0.028	-0.037
	[0.017]***	[0.016]***	[0.052]***	[0.052]***	[0.030]***	[0.024]**	[0.084]	[0.084]	[0.091]	[0.092]
20th Birthday		0.383		0.413		0.374		0.634		-0.323
		[0.005]***		[0.007]***		[0.007]***		[0.024]***		[0.028]***
20th Birthday + 1		0.338		0.131		0.513		0.548		0.370
		[0.005]***		[0.007]***		[0.007]***		[0.024]***		[0.028]***
21st Birthday		0.318		0.437		1.278		0.881		0.290
		[0.009]***		[0.041]***		[0.015]***		[0.055]***		[0.061]***
21st Birthday +1		0.440		0.382		1.350		0.674		0.696
		[0.009]***		[0.040]***		[0.015]***		[0.054]***		[0.060]***
22nd Birthday		0.284		0.895		0.550		0.700		0.649
		[0.004]***		[0.022]***		[0.007]***		[0.027]***		[0.029]***
22nd Birthday +1		0.288		0.570		0.836		0.382		-0.267
		[0.004]***		[0.022]***		[0.007]***		[0.027]***		[0.029]***
Constant	4.811	4.813	4.058	4.059	4.012	4.013	1.621	1.624	1.060	1.060
	[0.010]***	[0.010]***	[0.016]***	[0.016]***	[0.016]***	[0.016]***	[0.058]***	[0.058]***	[0.059]***	[0.059]***
Observations	1455	1455	1455	1455	1455	1455	1455	1455	1455	1455
R-squared	0.89	0.9	0.93	0.93	0.52	0.6	0.08	0.09	0.01	0.02

Notes: See notes to Table 3. Vagrancy is estimated from log(count+1) because there are 186 observations that are 0.

Figure 1: Past Month Drinking and Binge Drinking Participation

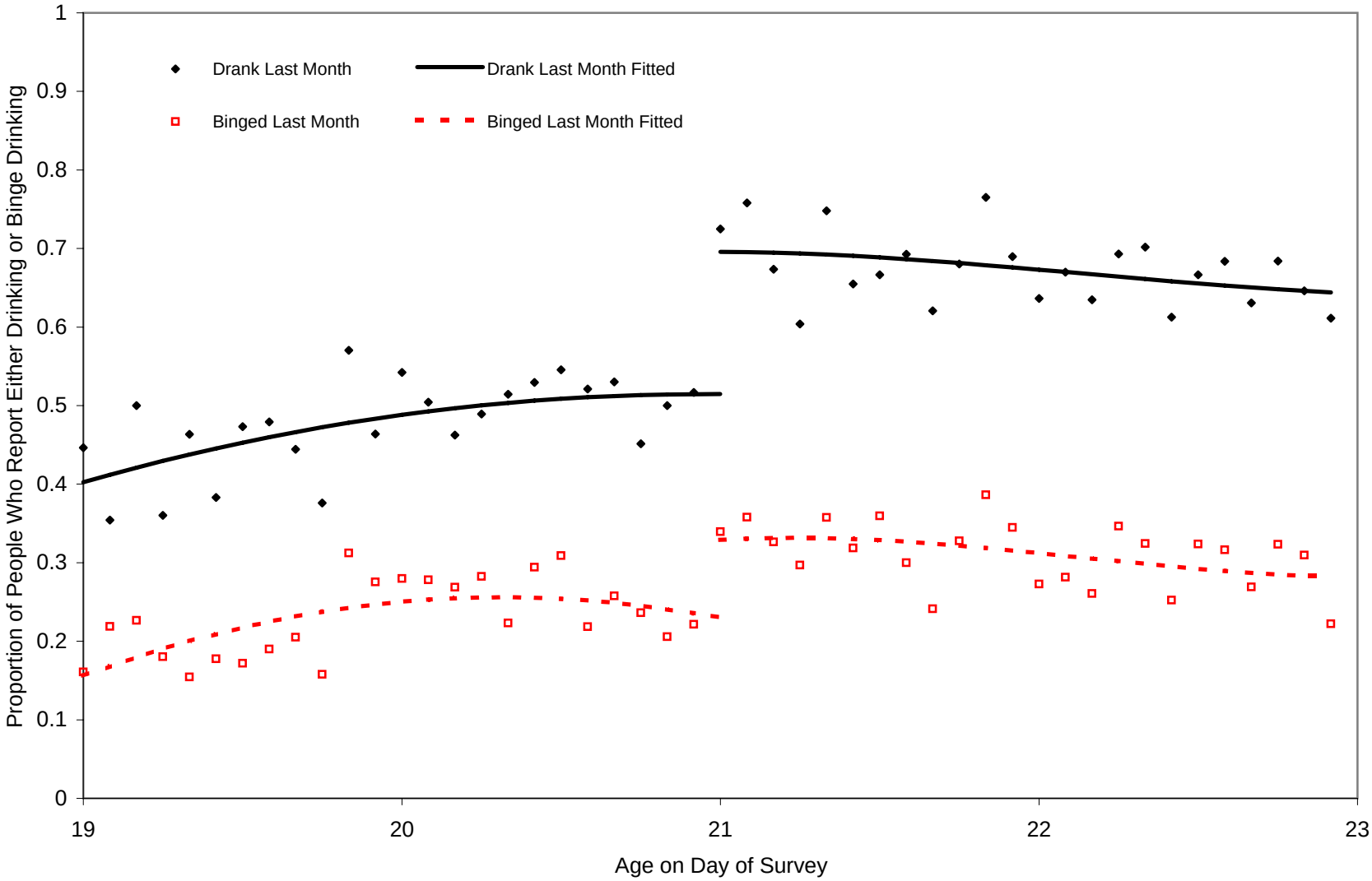


Figure 2: Past Month Drinking and Binge Drinking Intensity

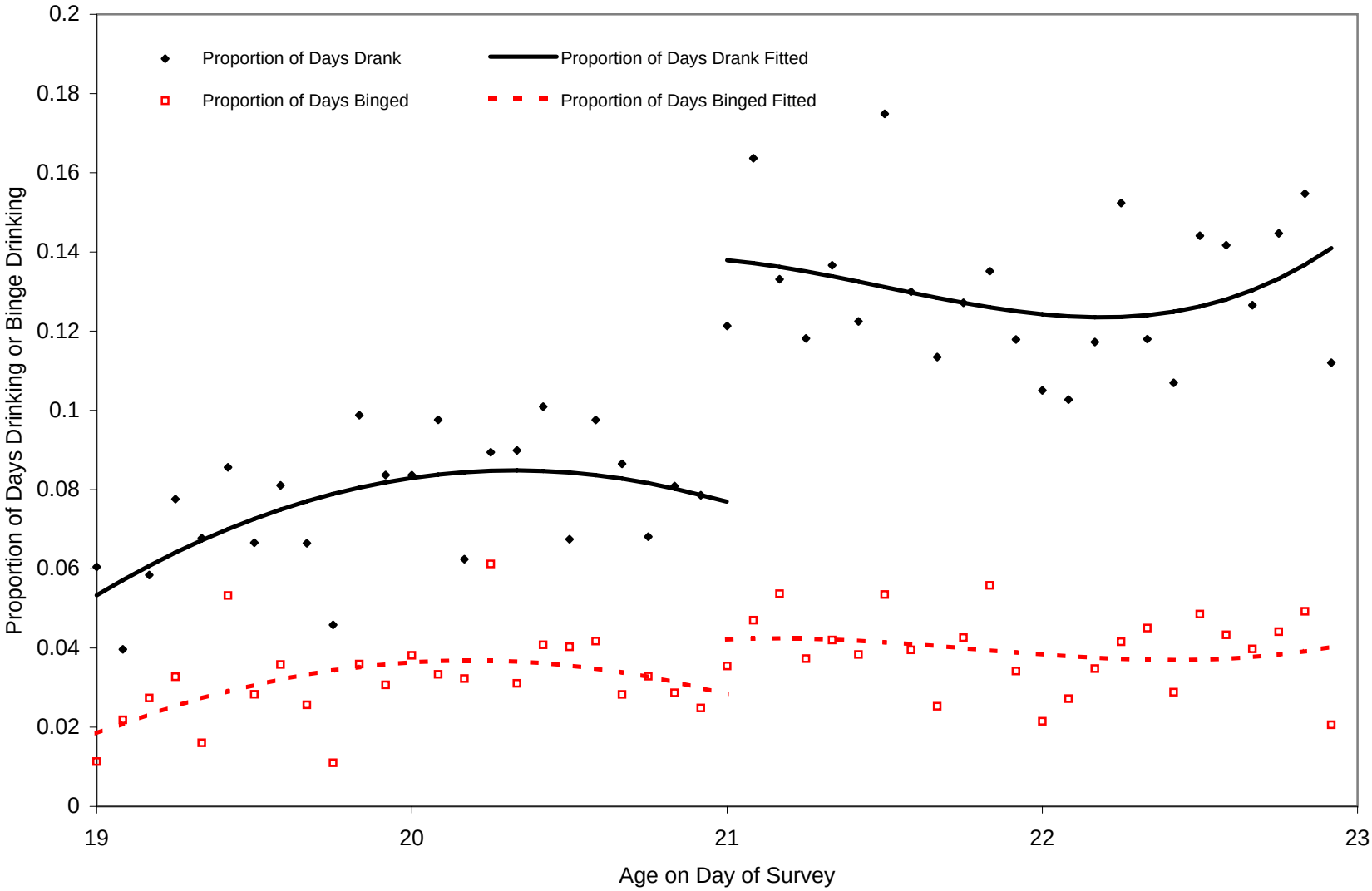


Figure 3: Average Drinks per Day Conditional on Drinking

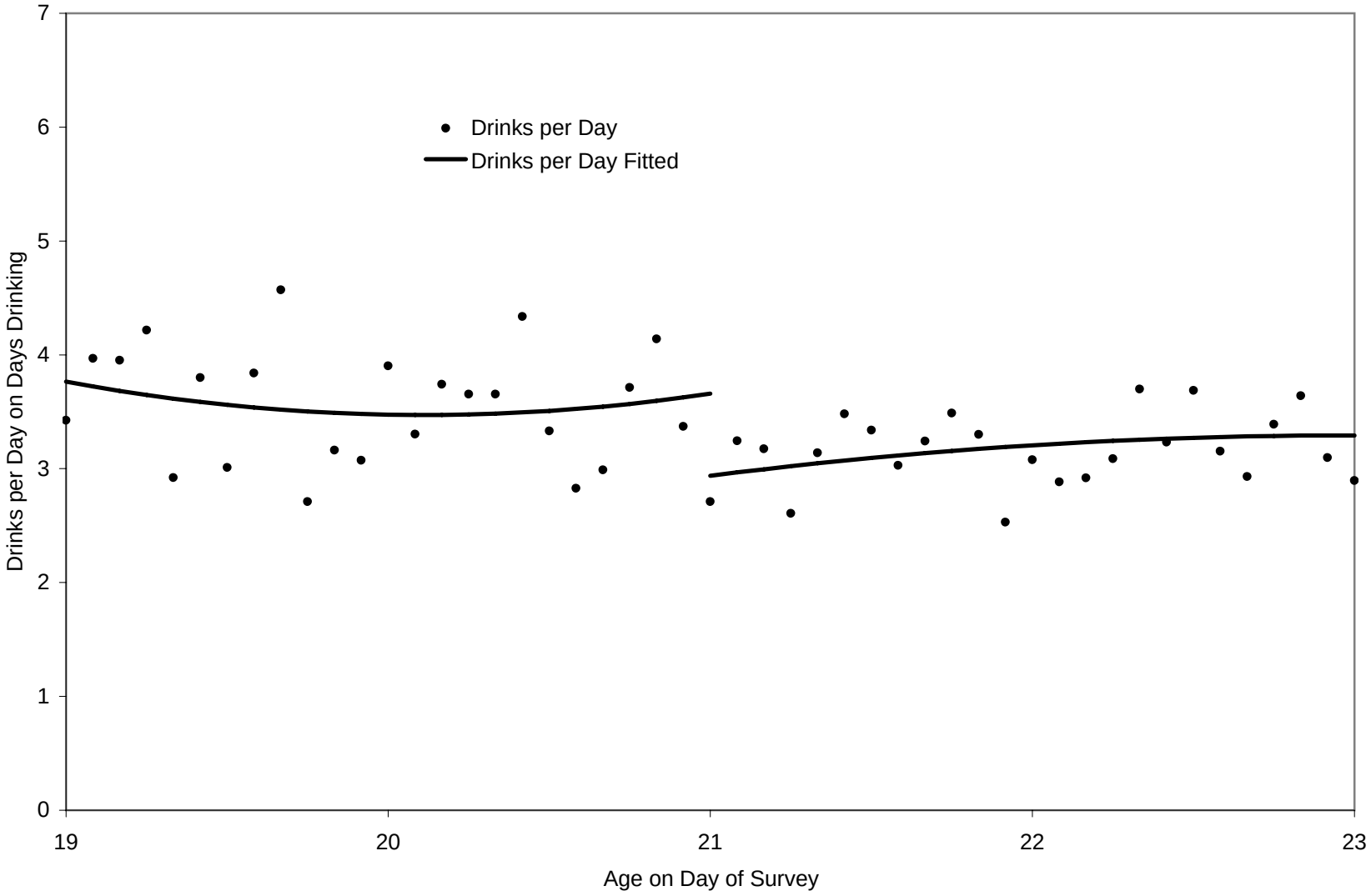


Figure 4: Drinks per Day, 20 and 21 Year Olds

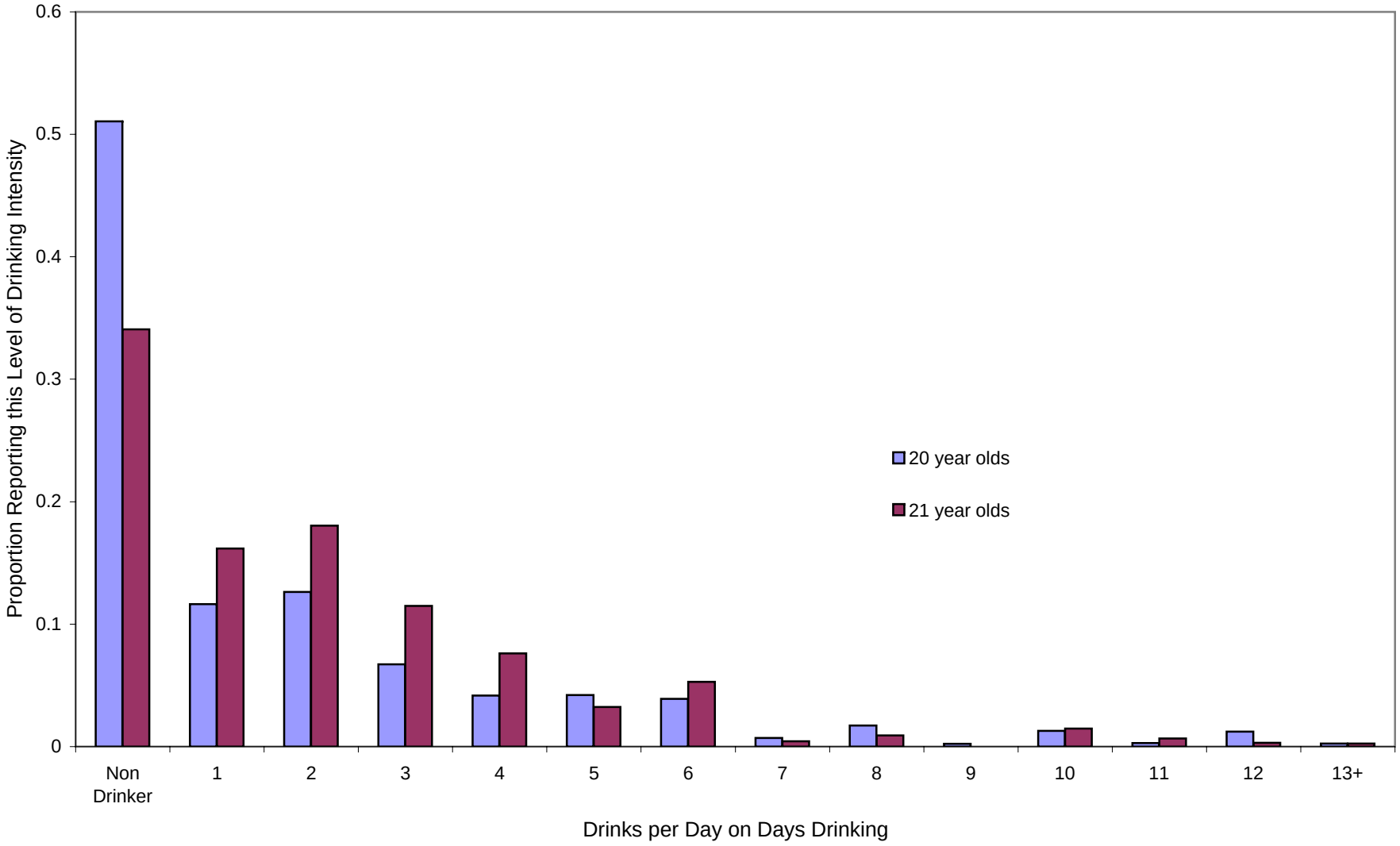


Figure 5: Arrests in California by Type of Crime California, 2000-2006

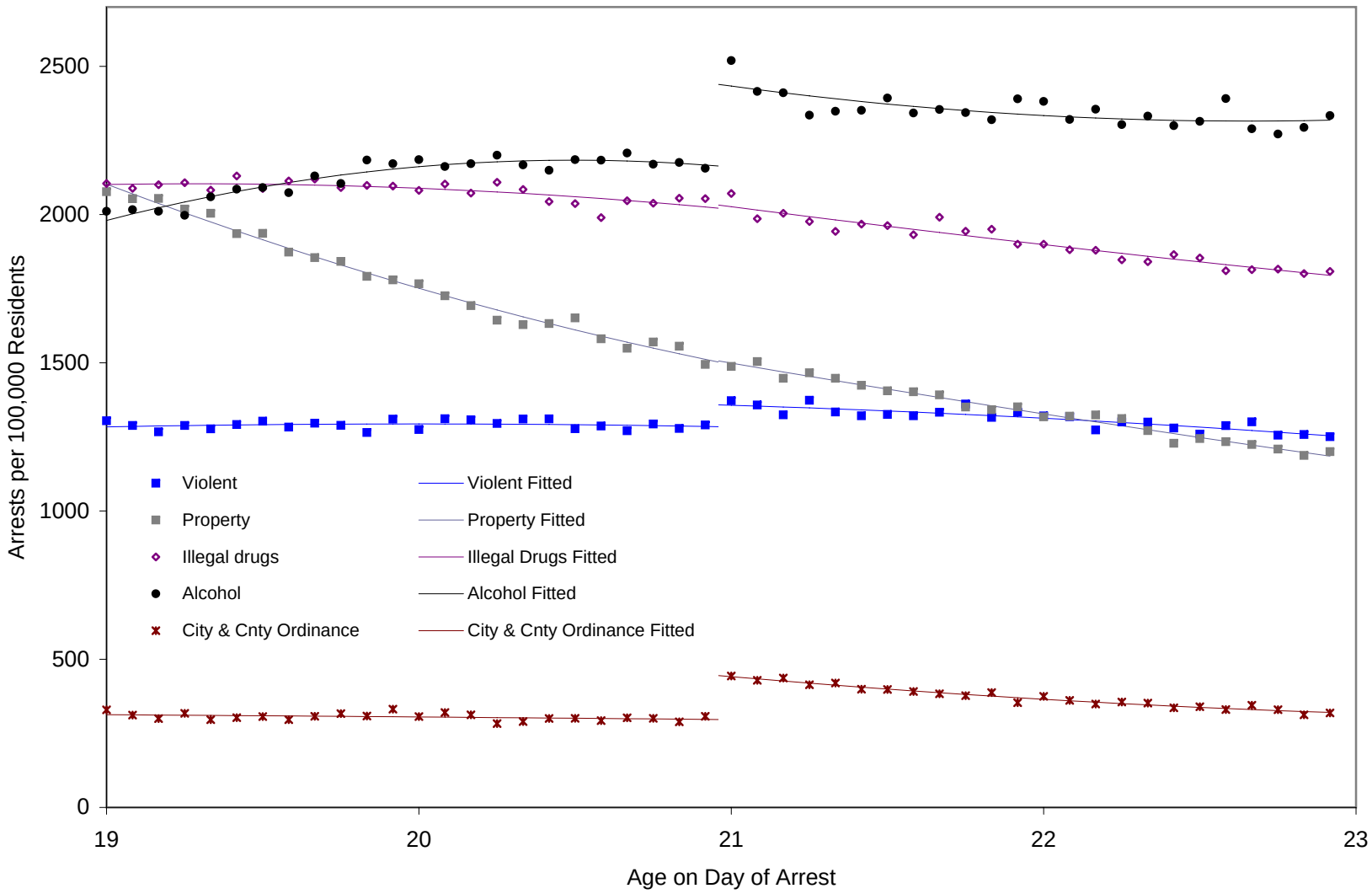


Figure 6: Arrests for Violent Crimes

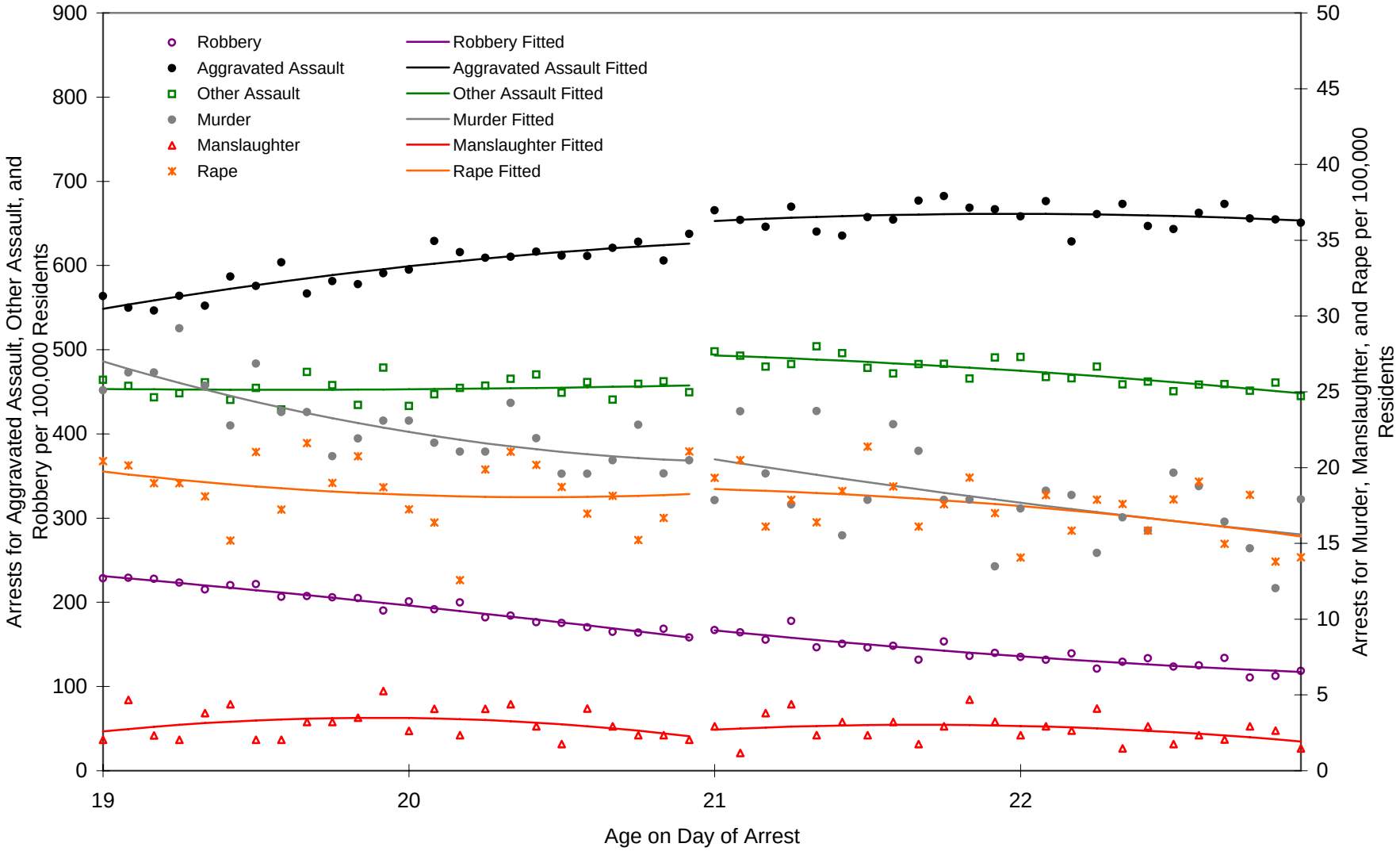


Figure 7: Arrests for Property Crimes

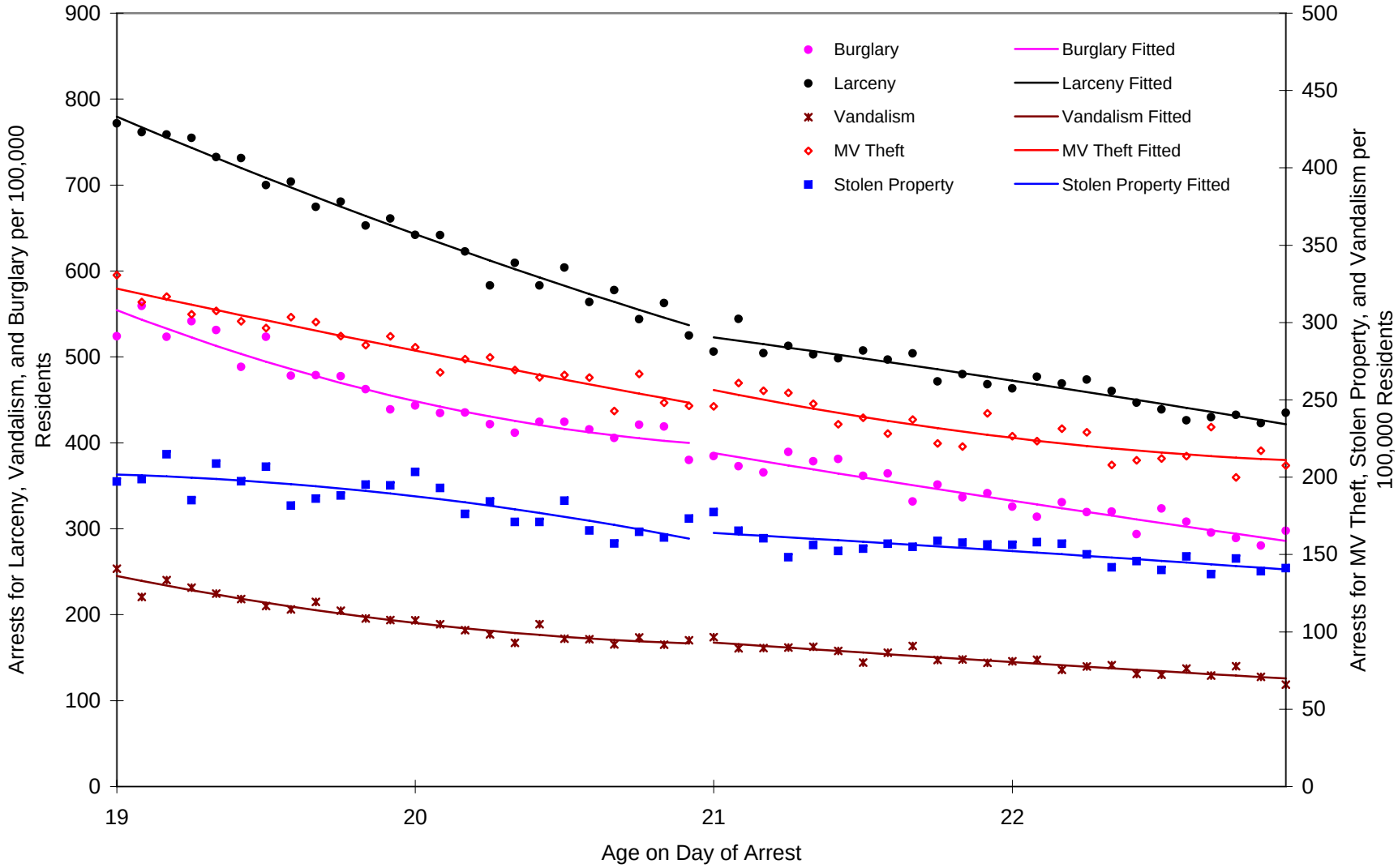


Figure 8: Arrests for Alcohol-Related Crimes

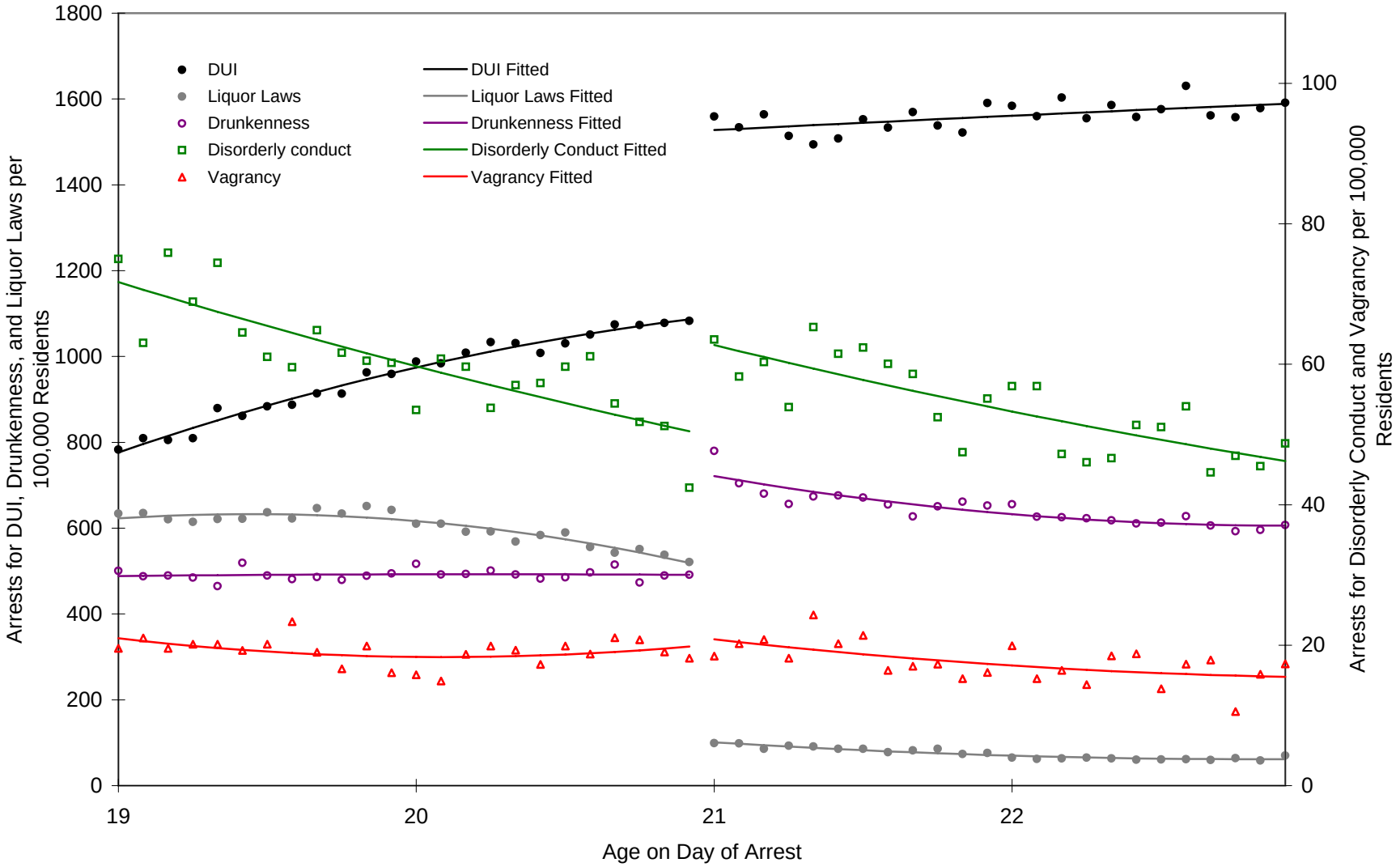
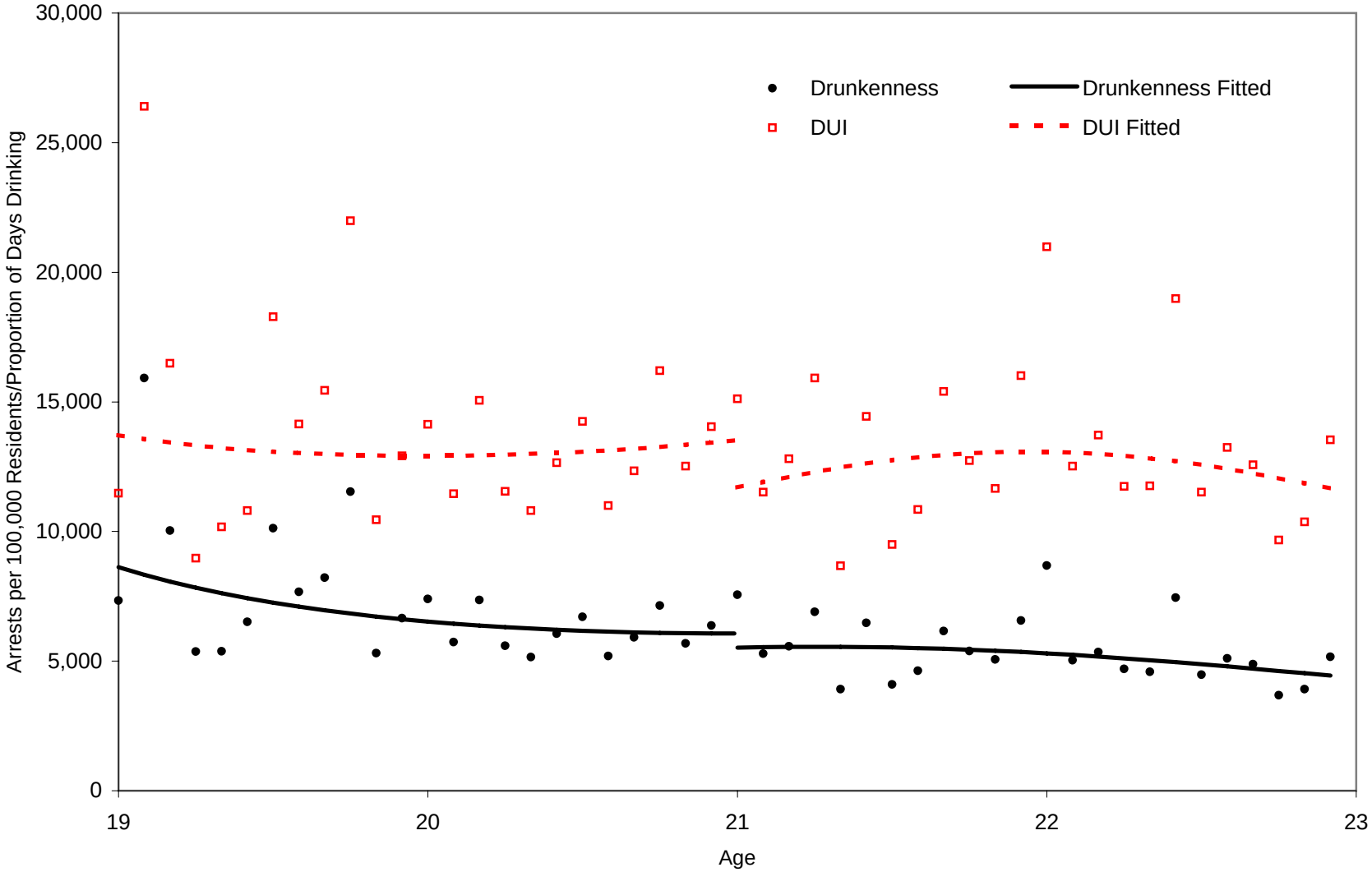
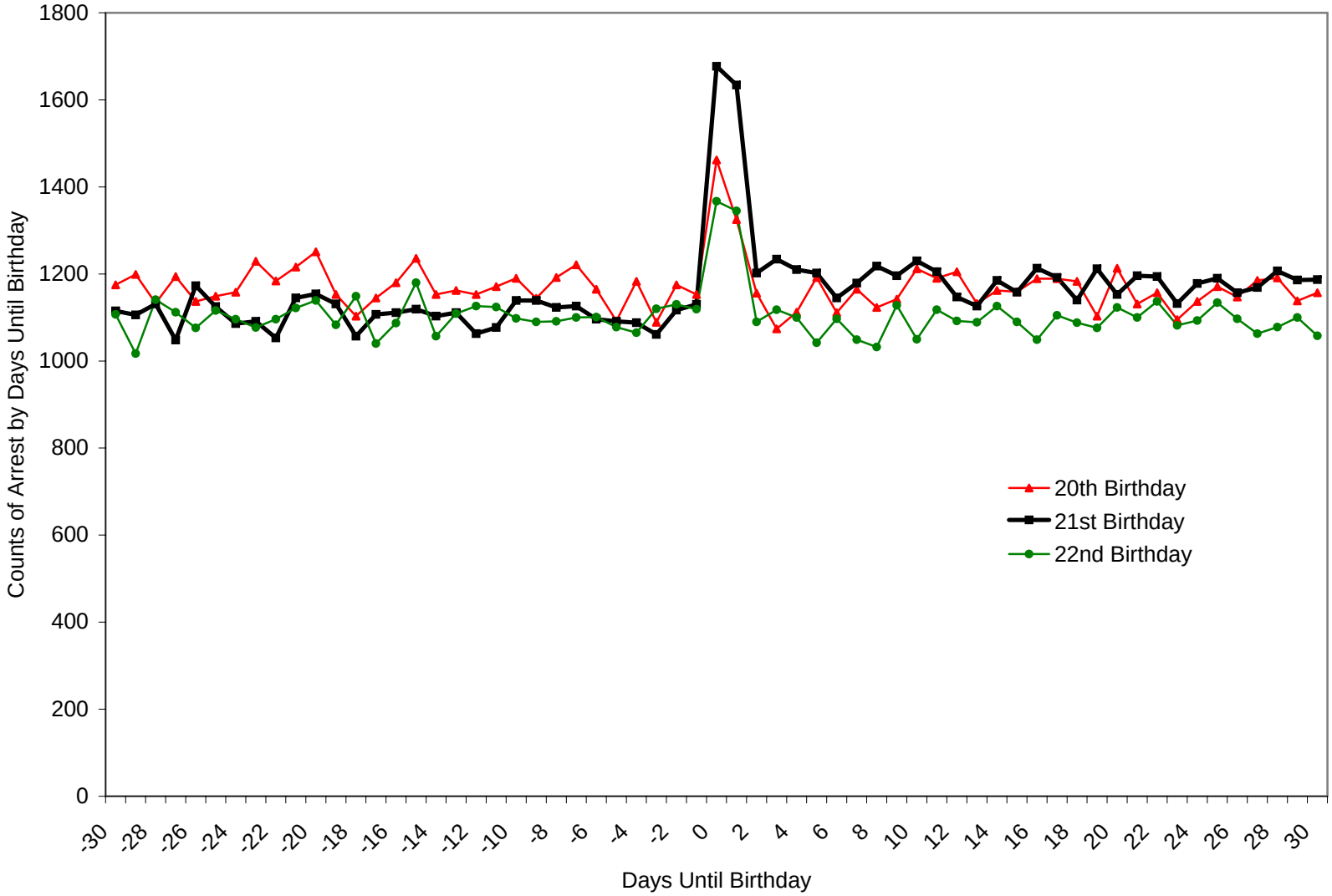


Figure 9: DUI and Drunkenness Arrests Over Proportion of Days Drinking



Appendix A: Arrest Counts by Days Until Birthday



Appendix B: Drinking and Binge Drinking Various Specifications

	Drank Last Month	Binged Last Month	Proportion of Days Drank	Proportion of Days Binged	Number of Drinks Cond. on Drinking
Quadratic	0.1675	0.0874	0.0566	0.0146	-0.6620
	[0.0541]***	[0.0492]*	[0.0169]***	[0.0094]	[0.3944]*
Cubic	0.2278	0.1500	0.0581	0.0238	-0.7504
	[0.0741]***	[0.0668]**	[0.0213]***	[0.0126]*	[0.5544]
LLR	0.2241	0.1216	0.0480	0.0125	-0.5941
	[0.0325]***	[0.0339]***	[0.0124]***	[0.0082]	[0.2758]**

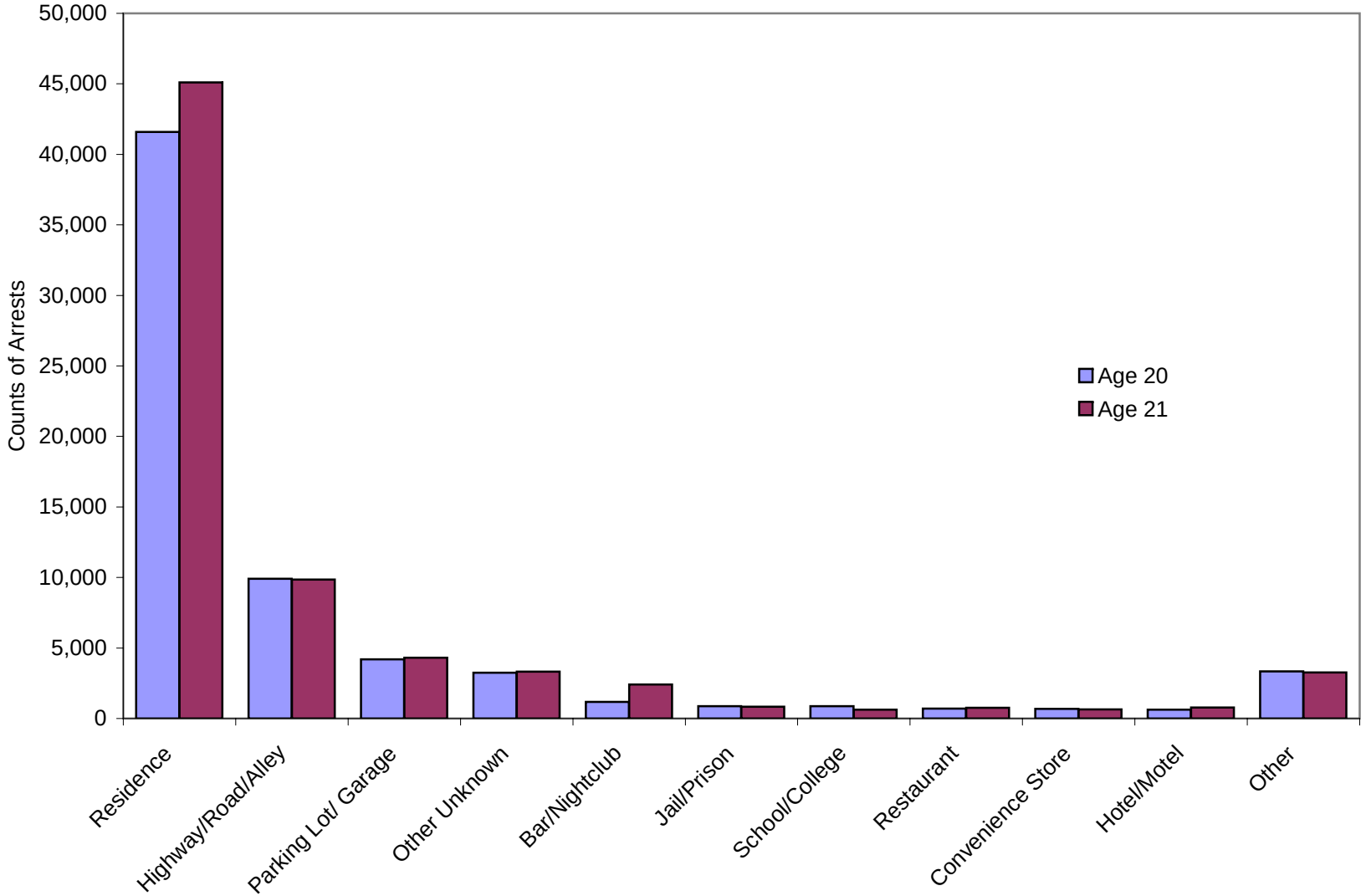
Notes: See notes from Table 1.

Appendix C: Age Profile of Arrests in California Various Specifications

	All Crimes	Violent	Property	Alcohol Related	Drug Possession or Sale	Violation of City or County Ordinance	Aggravated Assault	Other Assault
Quadratic Poly	0.0680	0.0660	0.0070	0.1410	0.0080	0.4150	0.0520	0.0880
	[0.007]***	[0.014]***	[0.013]	[0.016]***	[0.010]	[0.027]***	[0.019]***	[0.025]***
Cubic Poly	0.0620	0.0670	-0.0100	0.1420	-0.0050	0.4330	0.0470	0.1050
	[0.006]***	[0.016]***	[0.017]	[0.013]***	[0.014]	[0.037]***	[0.024]*	[0.031]***
LLR	0.0673	0.0672	0.0028	0.1566	-0.0045	0.3912	0.0603	0.0948
	[0.005]	[0.010]	[0.012]	[0.011]	[0.011]	[0.029]	[0.017]	[0.019]

Notes: See notes from Table 3. For the local linear regression the 21st birthday and the day immediately after have been dropped.

Appendix D: Location of Arrests for Assault by Age



Notes: From NIBRS arrestee data 2000-2006. The figure includes arrests for UCR offense code 13A (aggravated assault), 13B (simple assault) and 13C (intimidation). The increase in arrests for assaults at age 21 is approximately 5.5% above trajectory.