The Cost of Binge Drinking

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Abstract

We estimate the effect of binge drinking on accident and emergency attendances, road accidents, arrests, and the number of police officers on duty using a variety of unique data from Britain and a two-sample minimum distance estimation procedure. Our estimates, which reveal sizeable effects of bingeing on all outcomes, are then used to monetize the short-term externalities of binge drinking. We find that these externalities are on average £4.9 billion per year ($7 billion), about £80 for each man, woman, and child living in the UK. The price that internalizes this externality is equivalent to an additional 9p per alcoholic unit, implying a 20% increase with respect to the current average price.

JEL Classification: I12, I18, K42
Keywords: Alcohol, health, road accidents, arrests, externalities

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1. Introduction

More than two and a half billion people worldwide are alcohol users (World Health Organization 2014). With a global death toll of 5.9% (approximately 3.3 million deaths every year) and a burden of disease of 5.1% (about the same as that caused by tobacco), harmful use of alcohol has been identified as one of the leading preventable causes of death and a key risk factor for chronic diseases, injuries and cancer around the world (McGinnis and Foege 1993; Nakamura, Tanaka, and Takano 1993; Single et al. 1999; Mokdad et al. 2004; Balakrishnan et al. 2009; Zaridze 2009; Beaglehole and Bonita 2009; Rehm et al. 2009; Stewart and Wild 2014).

Binge drinking is an alcohol abuse pattern characterized by periods of heavy drinking followed by abstinence, which generally results in acute impairment and is believed to contribute to a substantial proportion of alcohol related deaths and injuries (Chikritzhs et al. 2001; Naimi et al. 2003; Courtney and Polich 2009). Little is known about the causal impact of binge drinking on individual outcomes, such as road accidents and arrests. Little is also known about the social cost of bingeing. The estimation of the effect of binge drinking and of its social cost are the two main areas of interest of our study.

We estimate the effect of binge drinking on outcomes using a variant of the two-sample instrumental variables procedure developed by Angrist and Krueger (1992) with a variety of unique data from Britain, a country with one of the highest binge drinking rates in the world (World Health Organization 2014). We use this technique because reliable measures of alcohol involvement are generally not available from the same data sources that collect information on outcomes. This method combines the first stage and reduced form results to produce estimates of the effect of binge drinking on a wide range of outcomes obtained from multiple sources.

Our choice of instrumental variables is motivated by the findings reported by a large body of medical and public health research. Although often associated with college and high school students (Wechsler et al. 1994; Miller et al. 2007), binge drinking is common also among other subpopulations, including non-students and those in their mid to late twenties (Naimi et al. 2003). A number of studies emphasize the importance of the social dimension of binge drinking (e.g., Skog 1985; Cook and Moore 2000; Courtney and Polich 2009). As a social convention, heavy episodic alcohol consumption requires some form of coordination (Young 1993; Jackson and Watts 2002). Time of the day and day of the week are two features that several studies highlight as strong predictors of binge drinking (e.g., Simpson, Murphy, and Peck 2001; Taylor et al. 2010). In many countries, Friday and Saturday evenings and nights are the times over which binge drinkers typically coordinate. Age and weekend nights therefore will be our instruments. More precisely, we achieve identification of the effect of bingeing on outcomes through differences in age and differences in the day of the week and time of the day in which bingeing is expected to occur. Sections 3 and 6 will return to this issue.

We focus on four outcomes — i.e., accident and emergency (A&E) attendances, road accidents, arrests, and the number of police officers on duty — which have never been analyzed in the context of binge drinking. We estimate that binge drinking increases the average number of daily injury-related A&E admissions by 8%, the daily mean of fatal road accidents by 50%, the average number of arrests for all alcohol-related incidences by another 45%, and has a positive (albeit imprecisely estimated) effect on police officers on duty in the order of 30%. We perform several robustness checks to examine the sensitivity of our baseline results to different definitions of the instruments and repeat our analysis with new first stage regressions based on alternative sources. We also consider the possibility that our second stage regressions identify not only the effect of bingeing but also that of other risky behaviors, such as the use of illicit drugs. Each of these exercises confirms the baseline estimates.

The effect estimates are then used to monetize the short-term externalities of binge drinking. This is a different strategy from what past research has attempted to accomplish using accounting exercises based on descriptive statistics. Our results indicate that a conservative estimate of these externalities is £4.86 billion per year ($7 billion), or £77 per year for each man, woman, and child living in the UK. The Pigouvian tax that internalizes this externality is 9p per unit of alcohol consumed, or £7,288 per alcohol related arrest.

Excessive alcohol consumption has been the direct target of several health policy interventions worldwide (World Health Organization 2010 and 2014). Many countries engage in policies aimed at reducing the harm of alcohol abuse, such as introducing alcohol taxes, setting a minimum price per unit of alcohol, and restricting availability through age limits on consumption. In the United States, the reduction of binge drinking is one of the leading health goals in Healthy People 2020 (National Center for Health Statistics 2014). In the UK, the government's alcohol strategy sets out explicit proposals to cut binge drinking (Home Office 2012), which are complemented by advertising strategies, such as the recent Change4Life campaign (Department of Health 2014), and the plan to license drugs, such as nalmefene tablets, designed to reduce alcohol consumption among problem drinkers (National Institute for Health and Care Excellence 2014). Identifying the effect of binge drinking and assessing its externality are of relevance to these public policy initiatives.

The remainder of the paper is structured as follows. Section 2 discusses the related literature. Section 3 sets out the methods and identification issues. Section 4 describes and summarizes the data used. Section 5 presents the baseline estimates of the effect of binge drinking on outcomes, while Section 6 explores further evidence. Section 7 computes the externality associated with binge drinking. Section 8 provides a policy discussion in light of our findings. Section 9 concludes.

2. Related Literature

The economic research on binge drinking is scant. The most recent comprehensive survey by Cawley and Ruhm (2012) discusses binge drinking in the context of other risky behaviors (e.g., cigarette smoking, illicit drug use, and overeating) but provides a slender review of studies that examine the specific effects of bingeing.

Much research instead focuses on heavy (or problem) drinking. Heavy drinking and binge drinking, however, are not necessarily the same, with the latter having a greater social component and happening within a short time period. The existing evidence is that heavy drinking decreases educational attainment (e.g., Cook and Moore 1993; Koch and Ribar 2001; Renna 2007; Lye and Hirschberg 2010; Carrell, Hoekstra, and West 2011) and employment (Feng et al. 2001; Terza 2002; Johansson et al. 2007).[1] Standard instruments used for the identification of these effects are state variations in alcohol taxes and in the minimum legal drinking age and, less often, religiosity and parental alcohol problems.

Few studies focus on the peer effects side of binge drinking. For instance, using randomized roommate assignment, Duncan et al. (2005) find that males who reported binge drinking in high school drink much more in college if their roommate also binge drank in high school than if assigned a nonbinge-drinking roommate. No such peer effects are found for females.[2]

Besides their use as instrumental variables, alcohol taxes and policies that restrict alcohol availability, such as changes in the state minimum legal drinking age (MLDA), have also been used to assess responses in alcohol consumption (but not much in binge drinking) and other outcomes. Cook and Moore (2001) estimate that a one-dollar increase in the beer excise tax would reduce the prevalence of youth alcohol use by two percentage

[1] Dee and Evans (2003) and Chatterji (2006) argue that the effects on education are likely to be small, while the insignificant effect on employment found by Mullahy and Sindelar (1996) seems to be driven by nonlinearities (Terza 2002).

[2] Lundborg (2006) finds similar results using Swedish survey data where peer effects are identified by variation in behavior across classes within schools and grades.

points, with no effect on binge consumption. They also find that youths who are younger than the minimum purchase age for alcohol in their state are 2.5 percentage points less likely to binge drink and 5.5 percentage points less likely to drink in the past 30 days.

Carpenter et al. (2007) find that a 1% increase in alcohol taxes is associated with a 1% decrease in high school seniors' heavy drinking (they do not analyze bingeing). Similarly, increases in the MLDA during the 1970s and 1980s reduced heavy drinking by 4% among high school seniors.[3] Beer taxes have also been found to be negatively correlated with child abuse committed by women (Markowitz and Grossman 2000), teen abortions (Sen 2003), and work days lost due to industrial injuries (Obstfeldt and Morrisey 1997). The effects of alcohol taxes on vehicle fatalities have also been widely studied with most research suggesting a strong negative association (Cook 1981; Chaloupka et al. 1993; Ruhm 1996; Young and Bielinska-Kwapsiz 2006).

Minimum purchase ages, however, may have the unintended consequence of leading youths to switch from alcohol to illicit drugs. DiNardo and Lemieux (2001) estimate that raising the state MLDA from 18 to 21 does increase the prevalence of youth marijuana consumption by 2.4 percentage points. Also Crost and Guerrero (2012), Crost and Rees (2013), and Anderson, Hansen, and Rees (2013) find evidence that marijuana and alcohol are substitutes. More generally, Conlin, Dickert-Conlin, and Pepper (2005) find that alcohol access and illicit-drug-related crimes (and not just those related to marijuana) are substitutes.[4] Although this evidence focuses on American youth for whom the MLDA is a binding constraint, the presence of such spillover effects is important for our identification strategy. We will return to this point in Sections 3 and 6.

Other policies that restrict availability include 'dry laws', that is, alcohol sale bans at certain times of the day, or on certain days of the week, or in specific premises. Across studies in different countries there is an overall agreement on the relationship between dry laws and alcohol related outcomes. Marcus and Siedler (2015) analyze the ban introduced in 2010 by one German state on alcohol sales at off-premise outlets (e.g., petrol stations and supermarkets) between 10pm and 5am and mainly targeted at young people. They find a reduction of 9% in alcohol related hospitalizations among adolescents and young adults. Taking advantage of a similar ban in the Swiss canton of Geneva, Wicki and

[3]Reviewing a large literature, Wagenaar and Toomey (2002) conclude that the evidence indicates an inverse relationship between the MLDA and youth alcohol consumption, traffic crashes, teenage child-bearing, and other social problems, such as homicides and vandalism. See also Dee (2001) and Carpenter and Dobkin (2009, 2011). Recently, however, Lindo, Siminski, and Yerokhin (2014) find no evidence that legal access to alcohol has an effect on motor vehicle accidents of any type in Australia, even though they have large effects on drinking and on hospitalizations due to alcohol abuse.

[4]Earlier studies, however, found evidence that alcohol and marijuana are complements (e.g., Pacula 1998; Williams, Pacula, and Chaloupka 2004).

Gmel (2011) find that this led to a reduction of 40% in alcohol related hospital visits by teenagers. Biderman, De Mello, and Schneider (2010) estimate that the adoption of mandatory night closing hours for bars and restaurants in the São Paolo metropolitan area reduced homicides by 10%. Heaton (2012) shows that repealing the law banning Sunday liquor sales in Virginia increased the number of crimes by between 5% and 10%.

Even fewer studies attempt to use the estimated effects of binge drinking on behavior to assess its social cost. Clearly, this is not a trivial exercise. Cawley and Ruhm (2012) discuss the challenges to conduct cost analysis for health behaviors.[5] In our analysis we follow a similar approach to that used by Levitt and Porter (2001) to assess the externalities generated by deaths due to drunk driving. In our case, however, we do not assume away the cost borne by binge drinkers, as this is arguably part of the total cost of bingeing that society must face and it is unclear whether binge drinkers took the risk of accidents, injuries or crimes fully into account. Similar procedures have been applied by Cawley and Meyerhoefer (2012) to measure the impact of obesity on medical care costs and by Heaton (2012) to compare the costs of additional crime relative to the state revenues generated by additional liquor sales on Sundays.

3. Methods

A. Statistical Model

Most large surveys or administrative records that measure binge drinking status do not collect information on outcomes, such as crime and road accidents.[6] We therefore employ a two-sample procedure which requires only one data set with information on binge drinking status and a second data set with data on outcomes. Based on a variant of the two-sample instrumental variables methodology developed by Angrist and Krueger (1992), this procedure combines the first-stage and reduced form results to generate estimates of

[5] Many studies attempt to measure the social cost of alcohol, with estimates of more than 1% of GDP in high-income and middle-income countries (e.g., Rehm et al. 2009). But by looking at individuals' future income losses and lost welfare due to behavioral distortions (e.g., sober drivers being afraid to drive at nights during the weekend for fear of being hit by a drinking driver), much of this research does not account for individual preferences. See also Miller et al. (2006) and Bouchery et al. (2011).

[6] Very few data sets contain measures of both outcomes and alcohol involvement. One exception is the study by Levitt and Porter (2001) in the case of car crashes. Their primary measure of alcohol use, being the police officer's evaluation of whether or not a driver had been drinking, is however subjective and relies on the perception of the officer on duty. Another exception is the work by Hansen (2012) on recidivism in drunk driving, which relies on blood alcohol content measured through breathalyzers. None of these measures is available in our data sets on outcomes. Moreover, the negative externalities of binge drinking do not materialize just through drunk driving. Other methods that have been exploited in the public health literature include the use of roadblocks, hospital based surveys, and on-site saliva testing (Lund and Wolfe 1991; Simpson et al. 2001; Johnston and McGovern 2004; Savola et al. 2005; Hoskins and Benger 2013). These methods are more expensive and generally carried out on small samples.

the effect of binge drinking on outcomes.[7]

Let Y_i be a given outcome for individual i, B_i denote the endogenous binge drinking status, and $\mathbf{z}_i$ be a vector of observed excluded instruments. Assuming away the role of covariates for simplicity, the limited information form representation of the model is given by

$$\begin{aligned} Y_i &= \beta_0 + \beta_1 B_i + \epsilon_i && (1)\\ B_i &= f(\mathbf{z}_i; \alpha) + \nu_i, && (2) \end{aligned}$$

where f is a function of the excluded instruments, and ϵ_i and ν_i are random shocks. In this set up, β_1 is the causal effect of binge drinking on Y and represents our parameter of interest.

Our first-stage data set is the Health Survey for England (HSE), which has information on B_i and $\mathbf{z}_i$, but not on Y_i. The instrumental variables used in (2) are age and drinking days. Extensive research finds that youth, college students, and individuals in the mid to late twenties are substantially more likely to binge drink than others (e.g., Wechsler et al. 1994; Naimi et al. 2003; Hemstrom et al. 2002; Williamson et al. 2003; McMahon et al. 2007; Miller et al. 2007). A large literature emphasizes the social dimension of binge drinking (Skog 1985; Moore et al. 1994; Cook and Moore 2000; Parker and Williams 2003; McMahon et al. 2007; Van Wersch and Walker 2009), which in turn requires some form of time coordination, with Friday and Saturday evenings being the times over which drinkers typically coordinate (Simpson, Murphy, and Peck 2001; Taylor et al. 2010).[8] Assuming $f(\cdot)$ is additive and linear in parameters and letting $\mathbf{z}_i = \{a_i, w_i\}$, the first stage equation is then given by

$$B_i = \alpha_0 + \alpha_1 a_i + \alpha_2 w_i + \alpha_3 a_i w_i + \nu_i, \qquad (3)$$

where a_i is equal to 1 if i's age is between 18 and 30 years and equal to zero if the individual is aged 50 or more and w_i is equal to 1 if individual i binge drinks at the weekend and zero otherwise.

In the second stage we use a variety of data sets, each of which contains information

[7] Another application of the two-sample instrumental variables method is the study by Dee and Evans (2003), which examines the effect of teen drinking on education. See Inoue and Solon (2010) for a discussion of the links between the two-sample instrumental variables estimator and its two-sample two-stage least squares variant and their standard errors.

[8] There must be other forms of coordination, such as coordination over places (bars, pubs, restaurants, dorms, and private homes). For most of the paper, we ignore place coordination. In Section 6, however, we shall account for it when we analyze data from time use diaries.

on a given outcome Y as well as the same instrumental variables used in the first stage, but not on binge drinking status. The data come from administrative records on accident and emergency attendances, road accidents, arrests, and police officers on duty. They will be described in more detail in the next section, along with the HSE.

Substituting (3) into (1) yields the following reduced form relationship between outcome Y and the instrumental variables

$$Y_i = \pi_0 + \pi_1 a_i + \pi_2 w_i + \pi_3 a_i w_i + u_i, \tag{4}$$

where $u_i = \beta_1 \nu_i + \epsilon_i$ and

$$\pi_0 = \beta_0 + \alpha_0 \beta_1 \text{ and } \pi_j = \alpha_j \beta_1, \; j = 1, 2, 3. \tag{5}$$

The reduced form (4) has a straightforward difference-in-difference (DiD) interpretation in which π_3 is the treatment effect estimate of being a young adult in weekend nights on outcome Y_i. Knowledge of π_3 is important in and of itself because it informs us about the differential propensity of young people to be admitted to A&E or to commit crime during weekend nights *relative to* older individuals. This point emphasizes that identification of β_1 is obtained through differences in age and differences in times/days over the week when individuals binge drink.

Testing that $\pi_3 = 0$ as well as that $\pi_1 = \pi_2 = 0$ tests the hypothesis that $\beta_1 = 0$. As discussed in Angrist and Kruger (2001) and Chernozhukov and Hansen (2008), this procedure is robust to weak instruments since no information about the correlation between control variables (which have been left out so far) and $\mathbf{z}$ is required to test that there is no relationship between the outcome and the instruments.

Estimating (3) with HSE data permits us to retrieve the first stage parameter vector, α. Using the data on outcomes, the DiD model (4) can be estimated to identify the vector of reduced form parameters, π. With $\widehat{\alpha}$ and $\widehat{\pi}$ at hand, $\beta = \{\beta_0, \beta_1\}$ can be identified using the system of restrictions (5) by minimizing.

$$G(\beta) = [\widehat{\pi} - \widehat{\alpha}\beta]' \; \Omega \; [\widehat{\pi} - \widehat{\alpha}\beta] \tag{6}$$

with respect to β, where Ω is a positive definite (optimal) weighting matrix.[9] That is, the minimum distance estimator is the $\widehat{\beta}$ that minimizes the criterion function G. This

[9] As the optimal weighting matrix is potentially subject to small sample bias, we shall check the robustness of our results replacing the optimal weighting matrix with the identity matrix as suggested by Altonji and Segal (1996).

is what we call two-sample minimum distance estimator (TS-MDE). From (5) it follows that β_0 and β_1 are overidentified, since we have four equations and two unknowns. We shall test these overidentifying restrictions in our analysis.

B. Identification Issues

Here we focus on two issues about the identification of the effect of binge drinking. The first refers to whether the effect we estimate can be assigned to binge drinking only rather than to other risky behaviors, such as illicit drug use, or to the possible over-representation of young drivers in nighttime traffic accidents. The second concerns our instrumental variables.

The first issue questions whether our approach identifies the effect of binge drinking or if we estimate the effect of another behavior, such as drug use, or both. After alcohol, marijuana is the second most commonly used intoxicant by youth in the United States (Johnson et al. 2005), the UK and other industrialized economies (Smart and Ogborne 2000). Some studies find that brain abnormalities (such as hippocampal volume loss and asymmetry, which may lead to lower learning, memory impairment, and reduced spacial memory and navigation skills) are more likely to affect heavy drinkers than joint cannabis and alcohol users (Lisdhal, Medina et al. 2007). Others also point out that marijuana's impairing effects on driving are moderate when taken alone, but can be severe when combined with alcohol, indicating there might be a "potentiating effect" of multi-drug use (e.g., Robbe 1998; McCarthy, Lynch, and Pedersen 2007). The results found by DiNardo and Lemieux (2001), Crost and Guerrero (2012), Crost and Rees (2013), and Anderson, Hansen, and Rees (2013) show that marijuana and alcohol are substitutes among youth, suggesting that the fraction of the young adult population that combines heavy cannabis and alcohol use is small. Taken together therefore these results provide support for the claim that we are likely to pick up the effect of alcohol abuse primarily.

Young people also use other illicit drugs, such as cocaine and heroin. A large medical literature on the concurrent use of alcohol and cocaine documents an offsetting effect of cocaine on alcohol induced behavioral deficits. A common finding of the research on the co-use of alcohol and cocaine is that the effect of alcohol/cocaine combination on violent behaviors and motor vehicle accidents is almost entirely driven by alcohol alone (Del Rio and Alvarez 2000; Pennings, Leccese, and de Wolff 2002). Similar findings have emerged among concurrent users of alcohol and other types of drugs, such as heroin, hallucinogens, and non-medical painkillers (Midanik, Tam, and Weisner 2007), although the prevalence rates of the simultaneous use of each of such drugs and alcohol are even smaller than in the case of the marijuana/alcohol combination.

We take the bulk of these results as strong evidence in favor of the predominant effect of alcohol abuse rather than that of illicit drug use on the outcomes of interest in our analysis. If the outcome effects driven by drugs alone exist, these are likely to be modest. In Section 6 we shall return to this issue and, using special British survey data, provide fresh evidence on the prevalence of concurrent use of alcohol and various types of illicit drugs and their possible use complementarity.

Another concern is whether young drivers are over-represented in nighttime traffic accidents simply because they have a greater likelihood to drive at night than their older counterparts. Past research has documented that one of the main causes of car crashes at night, other than alcohol, is sleep deprivation (Maycock 1996). Many experimental studies find that young and older individuals are more likely to be involved in traffic crashes at any time of the day than middle-aged drivers, with the young lacking skills and being more willing to take risks and with the old having more perceptual problems and difficulty in judging and responding to traffic flow (McGwin and Brown 1999). It has also been shown that age is negatively correlated with the likelihood of falling asleep at the wheel and that older people are relatively less sleepy than young drivers with similar levels of sleep loss (Horne and Reyner 1999). Little is known, however, about the possibility that the interaction of alcohol (or binge drinking) with sleep while driving at night differs by age. An even more basic issue for us is to assess whether the population of young people at risk of being involved in a traffic accident at night is larger than the population of older people. To address this question, Section 6 will examine time-use diaries and show that this possible argument is not borne out by the data.

The second issue pertains to the instruments and the exclusion restrictions they imply. It is important to reiterate that identification of the effect of binge drinking is achieved through *differences* in age, day of the week, and time of the day. In the case of age, individuals in the treatment group are aged 18 to 30 years and are compared to individuals aged 50 or more in the control group. In the case of time and day, the instrument is given by Friday and Saturday nights, where the night is defined between midnight and 05:59 (or 06:59, depending on the data used in the second stage), allowing for some delay between the time in which individuals drink and the time in which the effects of excessive drinking are observed. We shall perform several robustness checks against each of these definitions.

What if the orthogonality assumption that the differences in age, day of the week, and time of the day are not correlated with the unobservables that drive our outcomes are violated? It is hard to anticipate how this will affect β_1. For instance, if young individuals are involved in more road accidents than older individuals in the nights of working days (and not just of weekends), then our estimated effects will tend to be biased downward in size, because we will conflate differences in binge drinking status by age groups across all days of the week with the true impact of binge drinking. The bias instead will go in the opposite direction if older individuals are implicated in more accidents in the nights of working days. Because of these (and other) possibilities, we shall consider a variety of falsification tests and sensitivity checks with different definitions of treatment and control groups (see subsection 6.B).

4. Data and Descriptive Analysis

We examine four outcomes with four different sources. First, we analyze Accident and Emergency attendances from the Solihull Care Trust. Second, we examine national administrative data from the Department of Transport that collects information on road accidents. Third, our crime data come from two sources, the West Midlands Police and the Metropolitan Police Service (MPS). Fourth, we analyze police numbers, made available by Durham Constabulary and the MPS. In what follows, we describe the data and report the reduced form estimates. Finally, we present the HSE data that are used to estimate binge drinking status, our first stage regressions, and discuss the first stage results.

A. Accident and Emergency Attendances

Data on A&E records are provided by Solihull Care Trust (SCT). SCT was one of 152 primary care trusts in England, which were abolished in March 2013 as part of the UK Health and Social Care Act 2012. Solihull is a town in the West Midlands of England approximately 10 miles away from the city of Birmingham with a population of about 210,000 in 2010. With a median population size across primary care trusts of around 285,000, SCT is smaller.

In Table A1 we present comparisons between Solihull (column (b)) and the national profile (column (a)) along the age profile and a set of health behaviors, including binge drinking, smoking and healthy eating. We do not find any statistically significant differences in the health measures between Solihull and the national average. Solihull, however, appears to have a older profile (37% versus 34%) with a smaller 18–30 age group (15% versus 18%).

We have over 140,000 attendance records from midnight on the 1st of April 2008 to midnight on the 21st of January 2011. Attendances are recorded using the Tenth Revision of the International Classification of Diseases, ICD-10 (World Health Organization 2007), which specifies the exact cause of attendance, with injuries being highly likely to occur as a result of alcohol abuse (Brismar and Bergman 1998; Cherpitel 1993).[10]

A graphical cut of the data is given in Figure 1.A. This shows the mean number of injury related A&E attendances by hour of the week for individuals in the treatment group aged 18–30 and individuals in the control group aged 50 or more. On the horizontal axis, 0 corresponds to the first hour of Monday (00:00 to 00:59) and 168 refers to the last hour of Sunday (23:00 to 23:59). The darker portion of the line represents attendances that occurred during the night (from 00:00 to 06:59). The pattern of attendances across the two age groups is almost identical during weekday nights. A gap however emerges as the weekend approaches. The vertical lines indicate the baseline definition of weekend (Friday and Saturday nights), when the gap is largest.

We can disaggregate the SCT data by the nature of the injury and body region injured. This is important because falls are known to be a common consequence of excessive alcohol consumption, and head, hands, and elbows are generally the most affected body regions (Savola et al. 2005; Keundig et al. 2008). With the nature of the injury we can distinguish open wounds from superficial injuries. Figure 1.B presents the average number of attendances for head injuries by hour of the week. There are more spikes in comparison to Figure 1.A, and these appear most prominently for the 18–30 age group during the early hours of Sunday morning.[11] Off-weekend days instead have a very similar trend for treatment and control groups. Similar patterns are found for hand and elbow injuries, open wounds, and superficial injury attendances (not shown for convenience).

Another way of presenting the data is given in Panel A of Table 1. This reports the reduced form DiD treatment effects (π_3 in (4)) obtained using ordinary least squares regressions for the number of attendances among individuals aged 18–30 relative to the attendances among individuals aged 50 or more during the weekend between midnight and 06:59. The data are aggregated into cell means by year, quarter of the year, day of the week, and age group, and the regressions are then weighted by cell size and include controls for year, quarter of year, and sex. The statistical fit s good, with R^2 ranging from 0.49 to 0.72. We find a positive and highly significant effect of an additional 1.35 injury related attendances for the 18–30 age group at the weekend (first column). The

[10] Although the ICD-10 coding identifies alcohol related admissions, our data only records the primary diagnosis. Therefore, if a drunk individual is in A&E because of a head injury, head injury would be the primary diagnosis, and not alcohol intoxication.

[11] As in the case of injury related attendances, the gap between the two age groups begins to open up before Friday and Saturday, with a gap already emerging on Thursday. We shall perform robustness checks of our baseline estimates by including Thursday (or Monday) as part of our treatment period.

next two columns report the estimates on attendances for which the primary diagnosis is an injury to the head (second column) or to hands and elbows (third column). In both cases we find positive and significant increases in attendances. So we do in the last two columns, which report estimates on attendances related to the nature of the injury. The largest increase is measured in the case of open wounds, but the effect is significant also in the case of superficial wounds.

B. Road Accidents

The Road Accidents Data (RAD) are collected by the police for the Department of Transport whenever an accident involves at least one personal injury. We have all the RAD administrative records from 2006 to 2009 for England and Wales on over 1.2 million vehicles. Each record contains details about the accident and the individuals involved, including their age and sex, the exact time and location of the accident, and its severity, which in turn is distinguished into fatal, serious, and slight. One disadvantage of the data is that during the period under analysis the RAD do not report any measure of alcohol involvement.[12]

The link between motor vehicle accidents and alcohol-attributable injuries is well documented (e.g., Levitt and Porter 2001; Rehm et al. 2003; Taylor et al. 2010). Past research has also shown that experience as a driver and as a drinker is a key determinant of the probability of a road accident. Younger individuals are less experienced drivers and drinkers and are therefore expected to face greater risks of an accident (Asch and Levy 1990; Rossow 1996; Room and Rossow 2001; Rossow et al. 2001).

Figure 2.A shows the average number of road accidents for England and Wales by hour of the week for individuals aged 18–30 and individuals aged 50 or more. As in the case of A&E attendances, the largest gap between the two age groups emerges during the weekend. Panel B of Table 1 reports the reduced form estimates with all road accidents occurring between midnight and 06:59 as the dependent variable (first column). The data are aggregated to the day of the week for each quarter of the year for the two age groups separately. We include controls for year and quarter to account for variation in road quality and safety as well as for seasonality in road accidents. The linear fit of the data is remarkably good, with R^2 going from 0.79 to 0.95.

Our DiD estimate reveals an increase of 12.4 accidents per weekend for the treated group at the treated time. Table 1 also shows the estimates broken down by severity type. Each of the three types shows a significant impact. There are 0.74 additional accidents

[12] The arrest data, however, described in the next subsection, contain some road accident variables, which will be used in the analysis.

that are fatal at the weekend for the treatment group (second column). This result can be seen clearly in Figure 2.B, which confirms that the gap in fatal injuries between the two age groups is largest at weekend nights. We find a significant increase in the number of serious injuries of 2.6 per weekend. Before midnight at the weekend, the pattern of these accidents is similar to that of fatal accidents with the gap opening up earlier in the evening. For the most common type of road accidents, those slight in nature, we observe a similar week-night pattern, with the same divergence at weekends.[13] The magnitude of this effect is 9 additional slight accidents (panel B, Table 1). Finally, we examine the number of causalities as a result of the accident that occurred. We find a reduced form treatment effect of 22.3 extra road related causalities (last column in panel B).

C. Arrests

Our data on arrests come from the West Midlands Police (WMP) and the London Metropolitan Police Service (MPS).[14] These are the two most populated police areas in the UK, jointly covering a population of over 10 million people, with 2.6 and 7.8 million covered by the WMP and the MPS, respectively.[15]

We have counts of offences for each day of one week in February, May, August, and November for three years from 2009 to 2011. None of the twelve weeks includes a public holiday. Since information on the exact time of arrest is not available, each day is split into four 6–hour blocks. As our focus is on the effect of binge drinking on arrests we concentrate on the 00:00–05:59 block. The data are split into three age groups, i.e., below 30, between 30 and 50, and over 50. With 85% of arrests in the data involving men (and 90% when we consider just the night window), our analysis focuses on men only.

We identify two broad categories of arrests. The first category comprises arrests that are directly related to alcohol. These in turn distinguish ‘drunk’ (which is a combination of drunk and incapable, drunk and disorderly, and drunk in a public place) from ‘drunk driving’ (a combination of drunk in charge of a motor vehicle, accidents with a positive breath test, and accidents with a refusal on breath test). The second category is indirectly related to alcohol, that is, these are crimes where the consumption of alcohol is presumed to have played a role in the offence (Room and Rossow 2001; Carpenter and Dobkin 2011). These arrests comprise violent crimes (which include actual bodily harm, grievous bodily

[13]For both serious injuries and slight injuries, the figures are not shown for convenience.

[14]These data were requested and obtained through the 2000 Freedom of Information (FoI) Act.

[15]Compared to the SCT data used for the analysis of A&E attendances, we have a higher proportion of individuals aged 18 to 30 in the areas covered by the MPS and WMP. Appendix Table A1 shows that the WMP area is not statistically significantly different from the national aggregates along both health behaviors and age profile. London instead is significantly younger, and also has a significantly smaller proportion of binge drinkers, smokers and a greater proportion of healthy eaters.

harm, violent disorder, and affray), common assault, sexual assault, criminal damage, robbery, theft, and burglary.

Figure 3 shows the mean number of arrests for alcohol related incidences (both directly and indirectly related to alcohol) for each 6–hour block for men aged under 30 and those aged over 50. The top two lines correspond to the 18–30 age group. The solid darker line indicates arrests occured in the night (00:00–05:59), while the dashed lighter line represents arrests recorded in the other three time blocks (06:00–11:59, 12:00–17:59, and 18:00–23:59). In working days, more arrests are made during the day, but the opposite occurs over the weekend when more arrests are made during the night. The bottom two lines show the average number of arrests for those aged 50 or more. During the working week the patterns (albeit not the levels) are similar across the two age groups. But the night pattern of arrests for older men remain low and flat during the weekend, while there is a large increase in arrests among those under age 30, almost trebling the mean for workday nights.

Panel C of Table 1 reports the reduced form estimates for all alcohol related arrests, controlling for year and quarter. The data for both police forces are pooled and then aggregated into cell means by year, day of the week, and quarter for a total of 168 observations. The dependent variable is the average number of crimes per night by each age group; the fit of the data is good, with R^2 generally above 0.5 except in the case of sexual assault. We find an additional 74.3 arrests for the 18–30 age group at the weekend for all alcohol related crimes (first column).[16] About 27.6 extra arrests are directly related to alcohol (second column), with almost 11.5 arrests involving individuals who were drunk and over 14 involving people who were drunk drivers. Nearly 47 additional arrests are indirectly related to alcohol, with almost 22 extra arrests for violent crimes, 4 for common assault, 6.3 for criminal damage, and 2.9 for robbery. The reduced form estimate is not significant for the remaining three types of crimes (sexual assault, theft, and burglary).

D. Police Officers on Duty

As in the case of the arrest data, the numbers of police on duty were obtained using the 2000 FoI Act. The data were provided by the Durham Constabulary and the London Metropolitan Police Service. Appendix Table A1 shows that the populations in Durham and London have health behaviours that defer from the national aggregates in opposite directions: Durham residents are significantly less healthy while Londoners are healthier. Durham Constabulary covers a substantially smaller population than the MPS, with

[16]Men in the 18–30 age group are also more likely to be arrested in general and more so during the weekend.

around 0.6 million people. The MPS is the largest police force in the country with 33,367 full time equivalent police officers in 2010. This corresponds to 43 officers per 10,000 individuals of the population. The Durham Constabulary has 25 officers per 10,000 residents.

The data cover the same 16 weeks in four 6–hour daily blocks as those in the arrest data. For each hour of the day we know the number of police officers who were recorded on duty. Officers who were absent or on sick leave are removed. One disadvantage of the data is that we only know the total number of officers on duty and not the specific number of officers who were engaged in alcohol related crime prevention and other relevant activities.

The solid line in Figure 4 represents the average number of police officers on duty during the night (00:00–05:59) in the two police forces per 10,000 residents, while the dashed line refers to the numbers in service during the rest of the day. We observe a steady increase during the week nights, with police numbers reaching their peak on Friday night (Saturday morning). The patterns are very similar in Durham and London, with most officers deployed in the middle of the day during working days and at weekend evenings.

Panel D of Table 1 reports the reduced form DiD estimates. The dependent variable is the number of police officers on duty per 10,000 individuals of the treated population (i.e., the 18–30 year olds in London and Durham) where we restrict the time period between midnight and 6am. We find an additional 3.2 officers on duty per 10,000 residents at night during the weekend. This is made up of 2.5 additional officers for the London Met and almost 4 additional officers for the Durham Constabulary.

Notice that for this outcome we can only consider the effect of weekend nights. Police officers in fact are not deployed on the basis of the public's age and thus we cannot exploit any variation in individual age for identification purposes. Equations (3) and (4) therefore will only include the constant and w, implying the structural parameters, β_0 and β_1, are exactly identified. Because of this, we expect the standard errors around the TS-MDE point estimates for this outcome to be large (Hall 2005).

E. First Sample Data and Definitions of Binge Drinking

To estimate the first stage equation as given in (3), we use nationally representative data from the Health Survey for England (HSE). This is a questionnaire based cross-sectional survey collected annually since 1991, with around 12,000–20,000 respondents each year. To match the years of analysis on the second-stage outcome measures, we use the three surveys from 2008 to 2010. The HSE contains a wide range of demographic variables as well as self-reported and objective measures of health. Our interest is in the self-reported questions regarding alcohol consumption. The survey asks which day the respondent drank most in the past seven days, and also how many units were drunk on the heaviest day in the past seven days.

Three main definitions of a binge are considered, i.e., 8 or more, 12 or more, and 16 or more alcoholic units. To put these definitions into context, four pints of a 4% alcohol by volume (ABV) beer (each pint being 568 milliliters) are equivalent to four glasses of a 13% ABV wine (each glass being 175 milliliters). Both correspond to 9.2 units of alcohol. Similarly, seven pints of beer, or seven glasses of wine, correspond to 16.1 units.

We use alternative definitions for three main reasons. First, they allow us to capture possible effect nonlinearities, which have been shown to be important in the context of alcohol consumption (e.g., Cook and Moore 2000; Taylor et al. 2010). Second, the definition of binge drinking varies across studies and regulatory agencies. For instance, Cawley and Ruhm (2012) define binge drinking as 5 or more *drinks* on a single occasion. This is the same definition used in other US studies for men, while for women the definition is of 4 or more drinks in a row (e.g., Wechsler et al. 1994; Wechsler and Nelson 2001; Cook and Moore 2001; Naimi et al. 2003; Rehm et al. 2003; Courtney and Polich 2009). Others instead use the notion of alcoholic *units*. For instance, the British professional body of doctors defines binge drinking as 10 or more units in a single session (Royal College of Physicians 2001), whereas others define it as more than 8 units in one day (Wright and Cameron 1997), or half the weekly recommended units on a single occasion, i.e., 14 units for men and 12 units for women (Webb et al. 1996, 1998; Norman et al. 1998; Underwoood and Fox 2000), or 12+ units in one session (Measham 1996).[17]. Since the HSE collects information on alcoholic units, our definitions can only be based on units and not on drinks. Using the parameters given in the NIH Clinician's Guide (National Institute on Alcohol Abuse and Alcoholism 2005), one drink corresponds to two alcoholic units. So the 5+ drink definition of bingeing applied by many US studies is exactly in between our 8+ and 12+ unit definitions.

Third, we are concerned with the fact that the HSE contains only self-reported measures of alcohol consumption and does not collect objective biological markers, such as blood alcohol concentration. Self-reported measures of drink participation and intensity are likely to be subject to underreporting (Midanik 1988), which some have assessed to

[17] Another example is given by the UK Department of Health whose guidelines define binge drinking as double the recommended maximum daily alcohol intake of 3–4 units for men and 2–3 units for women. See <http://www.parliament.uk/documents/post/postpn244.pdf>

lead to a coverage of consumption in the region of 40–60% of known alcohol sales data (Knibbe and Bloomfield 2001).[18] Other studies, instead, argue that alcohol involvement is likely to be overstated in surveys (e.g., Ekholm et al. 2008), and in any case the extent of misreporting is unknown among binge drinkers.

Using more than one definition of bingeing therefore should provide us with a more accurate picture of the range of possible effects estimated at different levels of alcohol consumed. Moreover, to account for underreporting more directly, we will also show the results from the analysis in which we consider binge drinking as consuming 6 or more units of alcohol, i.e., only 2.5 pints of beer or two and a half glasses of wine.[19] Finally, as a further way to account for the potential problems of recall bias and misreporting, we also present and discuss the estimates for the subsample of 'drinkers', i.e., those individuals who report having drunk in the past seven days.

F. First Stage Regression Results

Using the HSE data, we estimate binge drinking status as specified in (3) needed for the second stage analysis. We consider alcohol consumption patterns by day of the week (weekend versus week day), time of the day (night versus day), and by two different age groups (those aged 18–30 years versus those aged 50+ years), and examine the probability that the heaviest drinking day over the past week is a binge episode. We also control for a standard set of background variables, including gender, ethnicity, education, and indicators for having a long standing illness and ever being a smoker. The dependent variable is equal to 1 to indicate a binge, and 0 otherwise.

Table 2 presents the results. The first three columns report the estimates for the whole sample, while the last three focus on the subsample of drinkers. Irrespective of sample and outcome, we find that high levels of alcohol consumption are more likely to occur in the nights over the weekend, and this behavior is significantly more likely to be observed among those aged 18–30 than among those aged 50 or more. In panels A–C, which refer to A&E attendances, road accidents, and arrests, the α_3 coefficient on the interaction between weekend nights and the younger age group is typically the single most important determinant of binge drinking status. A joint test of all the three predictors shows that these are highly significant in determining binge drinking status across all levels of alcohol consumption, as well as outcomes and samples. The F-test values range between 117 and

[18] In a review of the biomedical literature, however, Del Boca and Darkes (2003) raise doubts about the usefulness of alcohol sales to gauge the size of underreporting in alcohol self-reports.

[19] We also performed our entire analysis using other definitions, i.e., 10+ and 14+ units. The results from this analysis are consistent with those presented below and are thus not shown.

1,347 in panels A–C and between 60 and 1,039 in panel D, well in excess of the standard critical levels of validity reported by Stock and Yogo (2005) and Murray (2006).[20]

5. Baseline Results

A. A&E Attendances

Table 3 contains the optimal two-sample minimum distance estimates of β_1, the effect of binge drinking on injury related A&E attendances estimated using equation (6). Bootstrapped standard errors obtained with 1,000 replications are shown in parenthesis.[21] The relationship between alcohol consumption and attendances is monotonically increasing, with higher units consumed leading to an increase in attendances. This monotonic relationship holds for all injuries as well as for each of the different types of injury under analysis.

The estimates for all injury related attendances over the whole sample (drinkers and non-drinkers together) suggest that binge drinking defined as individuals drinking 8 or more units would lead to 3.29 additional A&E attendances per day. This rises to 5.71 and 9.42 when we examine 12 and 16 or more units, respectively. The baseline estimate for 12+ units corresponds to 8.1% of the mean number of injury related visits over the whole day and to 157% of the mean number of injury related visits per night. These are large effects. The impact on head injuries is even more substantial. The estimate of 2.54 additional attendances implies a 23% increase in the average number of head injury visits during the day and a 265% increase over the night. There is evidence of strong nonlinearities. For instance, going from 8+ to 16+ units leads to an almost three fold increase in the estimate effect for all injuries for the whole sample.

Table 3 also reports the TS-MDE results for the subsample of those who drank in the previous week. The estimates are very similar to those found for the whole population. Notice however that conditioning on the subsample of drinkers slightly compresses the distribution of the estimates. The estimate using 16+ units is smaller and the estimate using 8+ units is greater than the corresponding estimates obtained on the 'All' sample. The compression at the top of the consumption distribution may reflect that heavier

[20] We examined the robustness of the first stage results with respect to how we define the treated and control age groups, using the same alternative definitions as when we perform sensitivity checks on the second stage regressions presented in subsection 6.B. The results from these alternative measures (not shown) remain very similar to those found with our baseline definitions.

[21] See Inoue and Solon (2010) for a discussion about how to estimate standard errors for the two-sample instrumental variables estimator. The first column of the Table 3 reports the mean number of attendances over the entire day. For ease of exposition the mean number of visits over the night are not reported. The corresponding means for road accidents and arrests are reported in the first column of Tables 4 and 5, respectively.

drinkers are more able to cope with the additional alcohol and this does not translate into additional attendances, as we would have observed for a group of individuals less used to consuming large amounts of alcohol. It is worth stressing that these are short run effects of binge drinking, and the longer run effects are likely to be different.

B. Road Accidents

The effects of binge drinking on road accidents are shown in Table 4. Again we present the estimates obtained on the whole sample and on the subsample of drinkers. Defining binge drinking as 8 or more units of alcohol leads to an additional 49 road accidents. The effect increases to 129 accidents for 16 or more units. There were an average of almost 176,000 road accidents per year over the period, approximately 480 accidents per day, of which 37 occurred each night. Therefore the estimates for all accidents in Table 4 are between 10% (for 8+ units) and 26% (16+ units) of the daily average number of accidents.

The effect obtained in the case of 12+ units is 17%. This overall effect accounts for 3.7 additional fatal accidents (54% of the daily fatal road accident mean and 295% of the mean at night), 17 additional serious accidents (26% of the daily mean and 248% of the mean at night), and 61.5 additional slight accidents (15% of the daily mean and 215% of the night mean), with all the estimates being highly statistically significant. Doubling the quantity of alcohol that defines a binge from 8+ to 16+ units increases the estimated impact on accidents (all and by type) by a factor of about 2.5. This suggests the presence of nonlinear effects.

It is interesting to notice that Levitt and Porter (2001) find that on average 18.5% of all car crashes in the US between 1983 and 1993 were attributable to drinking drivers, while our estimate for 12+ units is 17%. These figures are remarkably close to each other, despite the fact they refer to different countries and different time periods and even though not all road accidents involve cars and not all drinking drivers are binge drinkers.

Using only the subsample of drinkers for the first stage estimation leads to very similar effects. As with the A&E attendance results, we again find a slight compression of the distribution of coefficients. This is for the same reasons given before: regular drinkers are more accustomed to the effect of alcohol, so they may need more units for it to have an effect.

C. Arrests

Table 5 reports the TS-MDE results for arrests. All the estimates in the table are statistically significant except for burglary with binge drinking defined as 8 units of alcohol and above. The first row is for all alcohol related arrests. This comprises of both arrests that are directly related to alcohol and arrests that are indirectly related. When bingeing is defined as 8 or more units in one session we find an effect of 134 additional arrests. This is 24% of the daily mean number of arrests of all alcohol related incidences. The impact increases to 251 and 470 extra arrests for 12+ and 16+ units respectively, corresponding to 45% and 85% of the daily mean.

About 35% of the effect on all arrests is due to arrests that are directly related to alcohol abuse. In turn, a large fraction of the effect on such arrests involve drunk driving. According to the estimates in Table 5 this amounts to 55% (=25.4/46.0, where 46.0 is the coefficient on directly alcohol-related arrests, and 25.4 is the coefficient on drunk driving using the 8+ unit definition). This proportion goes up slightly to 56% and 57% when we use the 12+ and 16+ unit definitions, respectively.

Arrests that are indirectly related to alcohol abuse play a large role. In particular, a binge of 8 or more units causes 38.5 additional arrests for violence related incidences per day, around 30% of the mean daily violent arrests and 132% of the mean at night. This rises to 68 arrests for a binge of 12 or more units (54% of the daily average arrests) and to almost 122 for a binge of 16 or more units (96% of the daily average). The estimated effects on arrests for criminal damage, theft, common assault, burglary, robbery, and sexual assualt are smaller but always quantitatively important and statistically significant.[22] Nonlinearities are substantial: going from 8+ to 16+ units trebles the effect estimates on arrests directly related to alcohol and increases the effect on arrests indirectly related to alcohol by a factor of 4.

Restricting the analysis to drinkers typically leads to the usual compression of the distribution of coefficients. But, at the largest definition of binge drinking, the effects on arrests for sexual assault, robbery, theft, and burglary are all larger in the drinkers' subsample compared to all individuals, although the differences are not statistically significant.

D. Police Officers on Duty

Table 6 presents the β_1 estimates on police numbers. In five out of the 18 point estimates shown in the table, we find negative effects. These tend to emerge when we use the 8+ unit definition. If instead we look at the estimates obtained using the 16+ unit definition, we notice extremely large positive impacts. The 12+ unit estimates are generally closer to the reduced form effect estimates, π_3, reported in Table 1, panel D, except for the

[22] It is worth recalling that this is not always the case with the reduced form estimates reported in Table 1.

MPS (London) case, for which the effect is negative both in the whole sample and in the drinkers' subsample.

In all cases, however, regardless of whether we look at London or Durham separately or together, we find that none of the TS-MDE results is statistically significant. As mentioned in subsection 4.D, this could be due to the fact that the only source of variation for identification of the effect on this outcome comes from weekend nights, and thus β_0 and β_1 are just identified. As well known in the literature, the exact identification in this case leads to large standard errors around the point estimates (Hall 2005; Murray 2006). When computing the costs in Section 7, we therefore will rely on the rather conservative, but precisely estimated, reduced form effects.

6. Further Evidence

To gain greater understanding of, and confidence in, our estimated effects, we performed several robustness checks. Here we present and discuss some the results from this analysis.

A. Differences by Sex

Although binge drinking has been usually seen as an issue for men, recent research and media commentaries point out the increasing prevalence of bingeing among young women (e.g., Motluk 2004; Young et al. 2005; Miller et al. 2007; Lyons and Willott 2008). It is interesting, therefore, to see whether our baseline effect estimates vary by sex. The results of this exercise are reported in Appendix Figure A1, which shows the estimates for A&E attendances and road accidents (panels A and B, respectively).[23] In both figures, each bar represents the relevant TS-MDE coefficient obtained using 12 units or more as the definition of binge drinking with the whiskers showing the 95% confidence interval.

We find statistically significant differences in all A&E outcomes, except for attendances due to superficial wounds. In all cases we estimate greater attendances for men. Turning to road accidents, estimate heterogeneity by sex is quantitatively large, with all the differences being statistically significant. Bingeing results in more road accidents involving men. For instance, drinking 12 or more units of alcohol would lead to 24 additional accidents per day caused by women and 51 extra accidents by men.

These results therefore confirm the view that binge drinking is primarily associated with men (Holmila and Raitasalo 2005; Rahav et al. 2006). But, with one third of hospital emergency attendances and one third of road accidents accounted for by women, they also lend support to the growing relevance of alcohol abuse amongst young women.

[23]We cannot perform the analysis on policy officers on duty because officers are not deployed on the basis of people's sex, while in the case of arrests we already focus on men only.

B. Sensitivity Analysis

Appendix Figures A2.A and A2.B show the TS-MDE results for A&E and road accidents respectively from 11 different exercises in which each time we change one of the assumptions used in the baseline estimation. Each figure presents the estimates obtained using 12+ units as measure of binge drinking. For each outcome the estimates are presented as a set of bars, with the first bar in each block reporting the baseline results for ease of comparison.

The exercises are as follows. First, we performed two changes to the age of the individuals in the control group, using individuals aged 40 and above in one case and individuals aged 60 and above in the other. Second, we changed the definition of age in the treatment group, reducing the age range to ages 18–25 in one case and expanding it to include individuals aged 18–40 in another case. Third, we modified the treatment time window, examining A&E attendances and road accidents recorded over a shorter time period in one case (00:00–04:59), and over a longer period in the other (00:00–08:59). In order to capture the heterogeneity of drinking patterns observed in Figures 1–4, we changed the definition of weekend to include Friday mornings in one case and Monday mornings in another. Instead of using the optimal weighting matrix that is potentially subject to small sample bias (Altonji and Segal 1996), we also performed our analysis using the equally weighted minimum distance (EWMD) estimator. To account for the bias induced by the possible underreporting of alcohol consumption in survey self-reports, we present estimates using the definition of binge drinking of 6+ units, keeping unchanged our baseline definitions of treatment and control age groups and treatment times. Finally, to test the validity of our findings further, we report the results from one falsification test in which we redefine Mondays, Tuesdays and Wednesdays as our "placebo" weekend (excluding Saturdays and Sundays) and change the treatment age group to individuals aged 31 to 49.[24]

For A&E attendances — irrespective of whether we look at all injuries, body parts or injuries by type — the baseline estimate is in the middle of the range of the estimates found when we change the age in the control group (Figure A2.A). We invariably find lower estimates if the lower age bound is reduced to 40 and greater estimates if it is increased to 60. But both estimates are never statistically significantly different from their baseline

[24]We performed other falsification tests in which we redefined the placebo weekend as Mondays and Tuesdays or Tuesdays and Wednesdays and also changed the treatment time, from night times to day times. The second stage results from these alternative tests are similar to those reported in the text and are thus not shown. Moreover, the F-test statistics from the first stage estimation are much lower than those reported in Table 2. With values ranging between 1.7 and 26.6, we cannot reject the null hypothesis that the instruments are uncorrelated with binge drinking status in many cases.

counterparts. A similar, perhaps most striking, pattern emerges when we change the age in the treatment group. Compared to the baseline estimates, across all types of attendances, we find significantly lower effects when the treatment group is restricted to those aged 18–25 and significantly larger effects when it is expanded to include those aged 18–40.

Reducing the treatment time period of attendances to 04:59 does not lead to any significant departure from the baseline estimates, while increasing it to 08:59 decreases the effects on all injury related attendances, and attendances due to injuries to hands and elbows. It might be that many A&E attendances between 7am and 9am are not related to earlier periods of heavy drinking. Changing the definition of weekend (i.e., including either Friday mornings or Monday mornings) does not alter our earlier results. Changing the weighting matrix and estimating the effects using the equally weighted minimum distance estimator lead to only marginally lower estimates than the baseline estimates found with the optimal weighting matrix. As expected, the equally weighted TS-MDE coefficients are also slightly less precise than the baseline estimates. The estimates are lower when a binge is defined to occur at 6+ alcoholic units. If underreporting is avoided or greatly reduced with this definition, then the binge drinking effects on A&E admissions are smaller than those implied by the baseline estimates, although they are always statistically significant. Arguably, however, a 6+ unit definition sets an extremely low bound in alcohol intake (two and a half pints of beer or glasses of wine), which may fail to capture the type of intoxication we are most interested in. Finally, the estimates obtained from the falsification test are small, wrong-signed, and often statistically insignificant. Although placebo tests cannot be definitive, these results provide additional support to the identifying assumptions about our instrumental variables.

A similar pattern of findings emerges for road accidents (Figure A2.B). For each type of accident, the magnitude of the baseline estimate is quantitatively close to the magnitude of the other estimates, except for the cases in which the treatment group age is changed. As in the case of attendances, restricting the treatment group to individuals aged 18–25 reduces the effect, while expanding the group to individuals aged 18–40 significantly increases the effect on all accidents, regardless of their severity. As with the A&E results, using equal weighting rather than optimal weighting for the minimum distance estimation leads to slightly lower and less precisely estimated effects. Similarly, a 6+ unit definition leads to substantially lower (albeit always statistically significant) effects. Without exception, the falsification test estimates are reassuringly wrong-signed and insignificant.

In Appendix Figure A2.C we present the results for the sensitivity analysis of the effect on arrests. Given the data, we are more limited in the number of checks we can perform. In this case, we change the definition of control group (to include individuals aged 30–50, rather than individuals aged 50 or more as in the baseline analysis), the treatment time (18:00–23:59, rather than 00:00–05:59), and the definition of weekend (including either Friday mornings or Monday mornings). With the original data coming in four 6-hour blocks each day we are also more constrained in performing falsification tests than we did for the previous two outcomes. But, as before, we find small F-test statistics (with values above 10 in only two out 15 cases) and wrong-signed estimates. Because of this and because of space limitations in the graph, they are not reported in Figure A2.C. Besides the estimate for all alcohol related arrests, the figure also shows the estimates for each specific type of crime.

Lowering the age of the control group generally reduces the effect on all types of arrests, but this reduction is never statistically significant. Our baseline estimates are also robust to changes in the definition of weekend and to the use of the EWMD estimator. Bringing the treatment time forward, instead, reduces the effect on arrests, perhaps because the arrest effects of binge drinking becomes evident only later in the night. Defining a binge as having drunk 6 or more units leads always to smaller effects, which are statistically not different from zero in the cases of common and sexual assault, robbery, theft, and burglary.

Figure A2.D shows the sensitivity analysis for the TS-MDE results for police officers on duty, combining the data from the Durham Constabulary and the Metropolitan Police Service. As in the case of arrests we are more limited on the checks that we can carry out. Changing the time period to between 18:00 and midnight leads to a large negative estimate. This is true also when we estimate the equally weighted TS-MDE model and when we use the 6+ unit definition, although in both such cases the magnitude of the estimates is relatively small. Changing the weekend definition instead produces positive effects on police numbers, which are not significantly different from our baseline estimate. Unlike the other outcomes, therefore, the TS-MDE effects on police numbers are more sensitive to the battery of robustness checks we performed. This is unsurprising given the large standard errors of the baseline estimates.

C. Overidentification and Heterogeneity

Given the four restrictions in (5), it is clear that β_0 and β_1 are overidentified for all outcomes, except policy officers on duty. We tested the overidentifying restrictions. Across all outcome measures we reject the null that the instruments ($a, w, a{\times}w$ and the constant) identify a common β_0 and a common β_1. This violation implies that different instruments produce different treatment effects, and while our instruments might be valid they may be picking up different parameters (Deaton 2010; Parente and Santos Silva 2012).

Having heterogenous treatment effects in the case of the effect of binge drinking on arrests, road accidents and A&E admissions is entirely plausible, and perhaps unsurprising, as it is likely that there would be 18–30 year old individuals who would not commit crimes or drive a vehicle independently on how much alcohol they consume.

To understand the extent to which the violation of the overidentifying restriction tests reveals estimate heterogeneity, we examine how the estimated β_1 varies as the instrument set changes. In particular, since identification of β_0 and β_1 using (6) can be achieved with one instrument and the intercept, we consider three cases with a combination of two instruments and three cases in which each instrument is used separately. In these latter cases we have exact identification.

The results of this exercise are summarized in Figures 5.A–5.C. In each figure and for each outcome, the first bar represents the baseline TS-MDE coefficient obtained using all three instruments. We emphasize four findings. First, removing an instrument reduces the precision of our estimates as would be expected (Hall 2005). Second, removing the interaction term between a (being aged 18–30) and w (weekend nights) leads to lower estimates. Again, this is not surprising given that $a \times w$ is the instrument that underpins our reduced form treatment effect of binge drinking, π_3, so removing it must affect the second stage estimates quite considerably. These results also help us understand why the TS-MDE point estimates obtained for the police number outcome have generally large standard errors.

Third, across all outcomes measures, the baseline estimates lie in the middle of the range of alternative effects. This should lend greater confidence to our baseline results. Fourth, the largest estimate variation occurs in the case of arrest outcomes, where removing instruments in some cases leads to statistically insignificant effects. This is particularly evident when either the interaction term, $a{\times}w$, is removed or the only included instrument is the age 18–30 dummy variable. This result emphasizes the joint relevance of age and weekend nights to determine the effect of binge drinking, indicating the importance of coordination for social drinking among young people. It also underlines the likelihood of a greater effect heterogeneity among individuals aged 30 or less.

D. New Results Using Time Use Data in the First Stage

One limitation in using data from the HSE for our first stage estimation is that although we know the day when the drinking took place we do not know when during the day this occurred. To address this timing issue, we employ time use data. The only large-scale time use survey available for the UK is the Time Use Survey (UK-TUS), which was conducted in 2000/2001 on about 11,600 individuals aged 8 years or more.[25] Respondents, who provide information on a wide range of background characteristics, fill two 24-hour diaries, one to be completed on a weekday and one on a weekend, giving us a total of more than 18,000 adult diaries. Each day is broken down into ten minute sections with both activity and location being recorded.

Although the UK-TUS has detailed diary information, it imposes two problems on our analysis. First, it covers an earlier period than that covered by the other data sets used so far, and it dates before the 2003 Licensing Act that affected opening and closing hours of public houses and clubs. To account for potential differences in behavior associated with changes in the legal environment, we present estimates for different times of the day and night.

Second, UK-TUS respondents do not report whether or not they consume alcohol while engaging in a particular activity. With no information on alcohol involvement, we cannot define binge drinking using a minimum number of alcoholic units on a single occasion, as we did with the HSE data. Instead, a binge is now defined to occur when an individual is in a pub, restaurant or bar while *not* eating at specific times of the evening and night. Excluding eating is an attempt to increase the probability that our binge drinking measure does identify an activity that is predominantly drink related. Moreover, focusing on specific public places, such as pubs and bars, allows us to add to the social coordination over time considered so far the coordination over space, which is known to play a role in alcohol use initiation and binge drinking (Borsari and Carey 2001; Wechsler et al. 2002; Naimi et al. 2003).

Table 7 reports the TS-MDE impacts on A&E attendances, road accidents, arrests, and police officers on duty in panels A, B, C, and D respectively. The first column shows estimates for the period from 18:00 to midnight, the second column considers the period from 20:00 to 23:00 (which was the typical closing time of pubs and bars at the time the UK-TUS was conducted), the third column extends this to include the hour after closing time, the next column covers the 20:00–02:00 period, while the last column shows results

[25] See <http://www-2009.timeuse.org/information/studies/data/uk-2000-01a.php>and <http://www-2009.timeuse.org/information/studies/data/downloads/uk/2000-01/ReviewTimeUseSurvey2000.pdf>.

for the 20:00–03:00 period. All first-stage estimates are presented in Table A2.[26]

Compared to the results in Tables 3–6 found when the HSE data were used in the first stage, the new TS-MDE effects are substantially greater. Even the smallest estimates, which are found when the critical time period for a binge drinking session is restricted to the 20:00–23:00 period (first row in panels A–C), are about three times higher than our baseline estimates in Tables 3–5 and, in the case of police officers on duty (first row in panel D), 90 times higher than the baseline estimate in Table 6. When the time period for a binge drinking event includes time after midnight, the estimates for all outcomes become even larger. The lack of information on actual alcohol involvment is likely to inflate the effect that we attribute to binge drinking, as it might capture other behavioral aspects that are not necessarily related to alcohol consumption. In the next section where we calculate the cost of binge drinking, therefore, we will rely on our more conservative baseline estimates obtained using the health survey data in the first stage and, for police deployment, on the small (but statistically significant) reduced form estimates.

E. The Role of Illicit Drug Use: Evidence from the Arrestee Survey

In subsection 3.B we questioned whether our method identifies the effect of binge drinking or if instead it estimates the effect of other risky behaviors, such as drug use or multi-drug use. A huge body of medical, psychological, and forensic research find that most of the deleterious effects of the combined use of alcohol and illicit drugs are driven by alcohol alone (Pennings, Leccese, and de Wolff 2002; Lisdhal Medina et al. 2007; Midanik, Tam, and Weisner 2007). Furthermore, there is increasing evidence suggesting that alcohol and cannabis (the two most widely used substances among youth in all advanced economies) are substitutes (e.g., DiNardo and Lemieux 2001; Crost and Guerrero 2012; Anderson, Hansen, and Rees 2013).

In this subsection, we provide fresh evidence on the concurrent use of alcohol and illicit drugs. For this purpose we use data from the three sweeps of the Arrestee Survey collected between 2003 and 2006. This is the first nationally representative survey of self-reported drug misuse among individuals arrested in England and Wales, which contains also measurements of alcohol consumption (Boreham et al. 2007).

[26]Without sufficient geographic detail, the first stage estimates for A&E admissions, road accidents, and arrest are the same. These are reported in the top panel of Table A2. Being aged 18 to 30 and the interaction of this variable with the weekend indicator variable are positive and significant, but the weekend dummy variable is never statistically significant. Jointly, the three variables are highly significant, with F-test values invariably above 100. For the analysis on police numbers, a and $a \times w$ cannot be used as instruments. The figures in Panel B show that the weekend indicator is not a strong determinant of binge drinking status, something that might lead to large standard errors in the second stage. Including the constant in the computation of the F-statistics (not shown) confirms this result.

The survey allows us to use only one definition of binge drinking as consuming at least 8 drinks on one occasion weekly (or more frequently) *and* having had an alcoholic drink in the 24 hours preceding the arrest. One drink here is half the amount of the definition used in the U.S., or 1.15 alcoholic units. This means that 8+ drinks correspond to 9.2+ units. According to this definition, 36% of all arrests observed in the survey involve individuals who had a binge before their arrest.

Unfortunately, the survey does not elicit information on the timing (and quantity) of drug use at the same level of detail, but only asks whether a drug was taken in the 48 hours, the week, or the month before the arrest. Arguably one week or one month represent a long time span over which different substances can be consumed, and possibly not at the same time when alcohol was consumed. To identify alcohol and drug co-use we therefore restrict attention to drug consumption in the last 48 hours before the arrest. By doing so, however, we can only focus on a subset of drugs (i.e., heroin, cocaine and crack) and not on others, including cannabis.

We then document the prevalence rates of concurrent use of alcohol and different drugs among the arrestee population in England and Wales, and how the co-use of each alcohol-drug combination is associated with different types of crimes, after controlling for a set of basic demographic variables.[27] These results are reported in Table 8, where we also show the findings for cannabis, even if the information elicited on its consumption refers only to the month before the arrest.

The first column of Table 8 shows the extent of use for each drug in the sample. About 12% of all arrestees had heroin, almost 8% had crack, and only 4% had cocaine. Multiple drug use is possible (that is, the three categories are not mutually exclusive), so the extent of use of any substance is about 17%, less than half the rate found for alcohol consumption. If we consider consumption in the month before arrest, we find that the extent of use of the same three substances is unsurprisingly larger involving 27% of arrestees (not shown), whereas 48% of them had used cannabis and 57% any type of drug. Even though the month window biases all these figures upward, cannabis is unequivocally the most popular drug among arrestees, while cocaine consumption is modest.

The second column presents the prevalence rates of combined binge drinking and drug use. The highest rate of co-use is observed in the case of cannabis, involving 19% of arrestees. But again this is likely to be an overestimate since it records cannabis consumption in the whole month before the arrest. The prevalence rates of joint alcohol-

[27] The same results are found when we constrain the sample to people aged 18–30 years, which represent almost 60% percent of the arrestee sample. We therefore concentrate on the estimates found for the whole sample.

drug use in the 48 hours pre-arrest are much smaller, at about 2.5% for heroin and cocaine users and 2% for crack users. Considering all these three substances together, only 5.4% of the arrestee population engage in simultaneous use of alcohol and any of such drugs. The rate observed for the same drugs over the month preceding the arrest is twice as large, with 11% of arrestees reporting co-use. If we extend our focus to all drugs during the previous month, we find a much higher prevalence rate of 23%, 85% of which is attributable to cannabis.

In the next column we report the correlation between arrest and joint consumption of alcohol and illicit drugs. This is obtained from least squares regressions that include controls for age, sex, ethnicity, education, presence of children, health and drug related problems, and indicators for arrest and prison histories. A negative correlation indicates that as arrests increase with bingeing they decline with drug use. This is suggestive of substitutability between drug and alcohol consumption in the production of arrests. A positive correlation is an indication of complementarity. For all arrests and each type of crime, we find evidence of substitutability between binge drinking and the use of all types of drug, except for cocaine. The same four associations emerge when we consider co-use defined over the month prior to arrest. If we consider all drugs with the month window definition, we find that alcohol and any drug continue to be substitutes, while alcohol and cannabis are statistically unrelated (as found by Thies and Register [1993]).

We underline three results. First, the extent of use of any type of drug combined with binge drinking is fairly limited even among the arrestee population, for which the prevalence rates are always below 3%. It is possible that these rates are even lower in the general population. The only exception is cannabis, whose co-use rate of almost 20% is nonetheless an overestimate because its consumption was measured over a longer pre-arrest period and it might have not coincided with alcohol abuse. Second, binge drinking is the main driver of arrests, with alcohol and drug use being either substitutes or unrelated at the time of arrest. Third, the exception to this result is cocaine, for which co-use with alcohol can potentiate the likelihood of arrest due to violent crimes, such as theft and assault (Pennings, Leccese, and de Wolff 2002). But, as mentioned earlier, this involves an extremely small proportion of arrestees and, quite likely, an even smaller fraction of the British population. Together all these findings give us a strong indication that the effects we estimate are the result of binge drinking rather than that of other substance abuse.

F. Differential Night Driving Patterns by Age: Evidence from the UK-TUS

Another concern raised in subsection 3.B was that our effect estimates could pick up differential driving patterns during weekend nights between the young and the old. If younger individuals were more likely to drive than individuals in the control group, then our second stage results could simply reflect the over-representation of younger drivers in nighttime traffic accidents rather than alcohol abuse.

To address this issue, we use the UK-TUS data described earlier, which provide detailed information on driving times (both as a driver and as a passenger). In particular, we estimate a set of regression models as specified in (3) in which the dependent variable is given by the minutes spent driving at six different time blocks of the day. The estimates, which are obtained after controlling for gender, ethnicity, an indicator for unemployment status, population density of the area of residents, number of children, quarter of the year, and a set of income dummies, are in Table A3. The first column of the table reports the estimates found with the same time window used in the road accident analysis, midnight to 7am. The other columns refer to the alternative time blocks used in the first stage regressions based on the UK-TUS, and serve as additional robustness checks. Similar results are found when the dependent variable is defined as the fraction of time in each of the six time windows and are therefore not presented.

The top panel of Table A3 considers only the time spent on the road by individuals as car drivers. Individuals aged 18–30 drive less than a minute than their older counterparts, although this difference is not statistically significant when we look at driving between midnight and 7am. The differences by age are larger and statistically significant when we consider other nighttime blocks, but we can never find significant differences in driving intensity between weekday and weekend nights. More importantly, we cannot detect any differential pattern in the time spent driving by age over weekend nights, regardless of the definition of night. The bottom panel of the table shows the results found on the subsample of car passengers. The estimates in this case are exactly the same as those found for drivers. We also extended this analysis and included cyclists, bikers, and lorry drivers. The results remain unchanged.

With this evidence, therefore, the outcome effect estimates we have presented so far are unlikely to reflect a differential propensity by age to be on the road exactly when the impact of binge drinking becomes more apparent.

7. The Short-Term Externalities of Binge Drinking

Having established a significant and positive effect of binge drinking on accident and emergency attendances, road accidents, arrests, and police officers on duty (at least in the reduced form analysis), we now turn to monetizing the externalities for each of those four outcomes. As mentioned in Section 2 and emphasized also by Levitt and Porter (2001), Cawley and Ruhm (2012), and Cawley and Meyerhoefer (2012), a full cost-benefit analysis of binge drinking is infeasible, essentially because the utility that individuals obtain from binge drinking and the value to alcoholic beverage producers and retailers cannot be easily accounted for.

In what follows we make a number of assumptions that are likely to have the effect of understating the true negative externality of bingeing. First, we exclude absenteeism, lost employment, and reduced productivity due to binge drinking, since we do not have any measurement of the impact of binge drinking on such behaviors (MacDonald and Shields 2001; Bacharach, Bamberger, and Biron 2010). Second, we do not account for longer term health problems caused by heavy alcohol consumption, such as cirrhosis of the liver, alcoholic gastritis and cardiomyopathy, and mental and behavioral disorders. Some reckon that these represent huge, perhaps the largest, costs to society (e.g., Rehm et al. 2009). Third, we rely on our baseline estimates, which are in the middle of the range of the estimates found with alternative treatment and control groups and substantially lower than those found when the first stage estimation is performed on time-use data. Fourth, we pick conservative unit costs.

To compute the externality, we scale up the TS-MDE effects reported in Tables 3–5 and, for police numbers, the reduced form effects of Table 1. Two scaling factors are used, one related to time (to obtain annual figures) and the other, if needed, related to space (to obtain national estimates).[28] We then multiply each of the annual national estimates of the effect of binge drinking by the unit cost that is officially published by a government statistical agency. Finally, we sum up the values across the four outcomes and obtain an estimate of the total externality. The arrest estimate is corrected with the multiplier compiled by the UK Home Office, which attempts to account for the potential underreporting of crime in the police data.[29]

Table 9 summarizes the results. Appendix B explains how they were obtained. To

[28]The balancing analysis reported in Appendix Table A1 and discussed in Section 4 shows that the subpopulations we study are either representative of the UK population (as in the case of the A&E data) or more likely to give us downward biased estimates of the effect of bingeing (as in the case of the arrest data, where the subpopulation is younger but also healthier than the national average.

[29]Details on the Home Office multiplier can be found at <http://tinyurl.com/crime-costs2010>.

better appreciate the range of the cost, we present results using the effect estimates for both all individuals and the subpopulation of drinkers, and for the three definitions of drink intensity we use (8+, 12+, and 16+ units). Around each point estimate, we also report the 95% confidence interval obtained using the bootstrap standard error of the TS-MDE or reduced form effects. Panel B of the table shows another set of results found when the arrest cost figures are not adjusted with the Home Office crime multiplier.

The point estimates of the total externality are between £2.6 billion and £8.7 billion in 2014 prices when we use the TS-MDE effects found on all individuals for 8+ and 16+ units, respectively. The corresponding figures computed on the effects found on drinkers are £4.0 billion and £8.5 billion, while those obtained when not applying the Home Office multiplier range from £1.8 billion to £5.5 billion on all individuals and from £2.3 billion to £4.4 billion on drinkers. The baseline externality obtained on all individuals with the 12+ unit definition is £4.86. Arrests are the most substantial contributor to the externality, accounting for 55% of the total £4.86 billion cost; road accidents account for another 43%, while A&E attendances and police numbers together make up for less than 2% of the total burden. When the Home Office multiplier is not applied, arrests represent a much lower fraction of the externality and account for about 30% of the total cost.

It is worth mentioning the economic relevance of the externality. Consider the £4.86 billion estimate. In 2013/14, the UK government spent around £4.4 billion on Income-based Jobseeker's Allowance, the largest social security benefit to the unemployed with 1.2 million claimants, and £21.1 billion on Housing Benefit, the main transfer to people on low incomes and the second largest benefit after Basic State Pension in the whole of the UK government spending on social security benefits. The binge drinking externality therefore is 106% of the annual expenditures on Jobseeker's Allowance and 23% of the expenditures on Housing Benefit (Browne and Hood 2012). With 63,182,000 UK residents, the £4.86 billion figure translates into an externality of £77 on each man, woman, and child living in the country in 2011.

Although the unit costs made available through government statistical agencies provide a useful official benchmark, they are reported as point estimates without accompanying measures of sampling and nonsampling errors. As in the case of most of the other standard official statistics, such errors may be nonrandom and potentially large (Manski 2013, 2014).[30] Moreover, for several outcomes, there are alternative cost sources that could be used. To account for the uncertainty in the measurement of unit costs and examine the sensitivity of the estimates in Table 9, we recalculated our estimates using a wide array

[30]One exception is the study by Brand and Price (2000) which provides "low" and "high" estimates in addition to their "best" estimate for the average costs of crime.

of alternative unit costs.

The details of these computations are reported in Appendix C, while the results obtained from this new analysis are shown in Figure 6. We rank all the 27 alternative values from the lowest on the left to the highest on the right of the figure and show both the point estimates and their 95% confidence bands. We distinguish the estimates based on the 12+ unit definition from those based on the 8+ and 16+ unit definitions. Panel A presents the estimates computed on the samples that include all individuals, while panel B uses the estimates found on the subsamples of drinkers. To ease comparisons we also report the benchmark externalities described before, which are represented by the three horizontal lines with their corresponding 95% confidence intervals in each panel.

Focusing on the estimates for all individuals based on the 12+ unit definition, we find that the externality ranges from £4.1 billion to £11.9 billion, for an average point estimate of £5.4 billion per year. The majority of these alternative values lie within the 95% confidence interval around the benchmark estimate. We have seven estimates that fall outside this range, one below and six above. The largest four estimates are a result of increases in the unit costs of fatal, serious and slight road accidents, and violent crime. Conversely, the value below the range is found when the lowest unit cost for sexual offences is used. Similar patterns emerge in panel B for the subsample of drinkers. The only striking difference is that, in this case, across all alternative measures the externality distributions found with the three different definitions of drink intensity are much closer to each other than when the externalities are computed over all individuals.[31]

8. Discussion

The previous section mentions the quantitative relevance of the externality. We now discuss the policy relevance of our estimates and how these can be used to inform public policy debates on alcohol abuse.

Consider the estimate of £4.86 billion per year. According to industry estimates, each individual aged 15 or more consumed 9.9 liters of pure alcohol in 2011 (Sheen 2013). With almost 52 million individuals aged 15 or more in 2011 and noting that there are 100 alcoholic units in one liter of pure alcohol, the total number of alcoholic units drunk in the UK was 51.57 billion in 2011. Our estimate then implies a negative externality of over 9p per alcoholic unit or £9.43 per liter of pure alcohol, which represents an increase

[31] All the previous figures were obtained using the Home Office multiplier. We repeated the entire procedure without using the multiplier. For the sake of brevity, these results are not shown. In this case, we find that the 12+ unit estimates yield a minimum cost of £2.6 billion per year, a maximum of £8.0 billion and a median (mean) of £3.0 (3.5) billion.

of at least 20% in the current average price. This is equivalent to an additional tax of 95p per bottle of wine or 22p per pint of beer.

Minimum pricing policies have been recently under consideration in several countries, including Australia, Canada and the UK (Stockwell et al. 2012; Holmes et al. 2014). The devolved Scottish government passed, but not yet implemented, legislation to introduce a minimum price below which a unit of pure alcohol cannot be sold to consumers.[32] With the 2012 Minimum Pricing Alcohol Act, Scotland intended to set the minimum unit price for alcohol at 50p, when the average retail price per unit was estimated to be 40p, possibly 42p in 2011 prices (Leicester and O'Connell 2012). The 8p hike is still 15% short of the adjustment needed to offset the estimated burden, whereas the 10p hike would internalise the externality, keeping in mind our estimate represents a lower bound of the social burden of binge drinking.

Most of the current discussions about the introduction of a minimum unit pricing in England and Wales focus on a level of 45p (Griffith, Leicester, and O'Connell 2013; Stockwell and Thomas 2013; Holmes et al. 2014). With an average unit price at 40p in 2011 prices, this policy will fail to internalize nearly 47% of the externality of binge drinking. Besides the issue that minimum unit pricing of alcohol might not be legal (as the challenges to the Scottish legislation illustrate), concerns about this policy have also been raised in relation to its potential negative effect on responsible drinkers and on the possibility of large effects on individuals with low incomes (e.g., Ludbrook et al. 2012).

Taxing alcohol consumption, however, is a blunt policy instrument and is likely to introduce distortions into consumption decisions which this computation does not account for. We then look at the same issue from a different angle. Our estimates suggest that binge drinking leads to 376,692 additional arrests every year. The implied Pigouvian tax that internalizes this externality is approximately £7,288 per arrest. This figure is about 60% less than the loss of £17,915 (or $8,000 in 1993 prices) estimated by Levitt and Porter (2001). This in turn might reflect the fact that a significant fraction of individuals arrested for driving drunk are not necessarily binge drinkers or that the existing punishment in the UK is below that in the United States.

An alternative policy is a reform of the whole system of alcohol excise taxes by which taxes for all types of alcohol are meant to depend explicitly on the alcohol content and the tax rate is allowed to increase directly in line with alcohol strength (Griffith, Leicester, and O'Connell 2013). Although this alternative is at present only a proposal, two points

[32]Implementation of the legislation has been stalled by objections brought by alcohol industry groups and the Scotch Whisky Association the European Commission and the Scottish courts (Stockwell and Thomas 2013).

are worth stressing. First, the proposed excise duties per unit of alcohol are well below the 40p or 45p figures discussed earlier, with, for example, the excise on 4% alcohol by volume (ABV) beer being 8.9p, that on 13% ABV wine being 27.2p, and that on 40% spirits 43.4p. Internalizing the binge drinking externality with this reform will therefore imply a steep rise in excise taxes across all types of alcohol. Second, this proposal does not account for the possibility that individuals switch from more expensive to cheaper consumption options. Although this switch might not be an issue for some consumers, it could be for binge drinkers.

Yet another focus might be on policies that restrict alcohol availability rather than on price-based policies. For instance, the current minimum legal drinking age (MLDA) in the UK is 18, while it is 21 in the United States. Carpenter and Dobkin (2009) estimate that deaths due to motor vehicle accidents in the United States increase by about 15% at age 21, when drinking becomes legal. Assuming this estimate applies to all Britons aged 18–20, we compute the number of accidents that could have been prevented in the UK at the weekend and calculate the corresponding burden reduction using our baseline unit costs. We find that increasing the MLDA to age 21 would lead to a £100 million saving, which represents a 4.9% reduction in the road accident costs, or less than 2.1% in the overall externality. Repeating the same exercise on arrests using the estimates reported in Carpenter and Dobkin (2010), we compute a reduction in the overall externality of about 0.5%. This evidence suggests that, all else equal, increasing the MLDA from age 18 to 21 is likely to have only a small impact on the negative externality of binge drinking in the UK.

9. Conclusion

This paper estimates the effect of binge drinking on four different outcomes, accident and emergency attendances, road accidents, arrests, and the number of police officers on duty. For this purpose we adopt a two-sample instrumental variables procedure to overcome the problem of not observing outcomes in the same data set where information on binge drinking is available, and combine survey data with unique administrative records from Britain which have never been used before. The instrumental variables used to achieve the identification of the effect of binge drinking are *differences* in individuals' age (younger vs older), day of the week (weekend vs weekdays) and time of the day (nights vs days) in which alcohol is consumed and over which there is considerable social coordination.

Binge drinking is found to have large statistically significant effects on all outcomes. It increases the average number of daily injury-related A&E admissions by 8%, the daily mean of fatal road accidents by 50%, the average number of arrests for all alcohol related incidences by another 45%, and has a sizeable positive effect on police officers on duty in the order of 30%, although this impact is generally less precisely estimated.

To probe the robustness of the results we performed several sensitivity checks, varying the definitions of binge drinking, treatment and control age groups and treatment times, accounting for the possible survey underreporting of alcohol consumption, using time-use diary data to identify a more accurate timing of alcohol use in the first stage (first sample) estimation, exploring how combined use of alcohol and illicit drugs could affect our findings, and analyzing the differential propensity to drive during weekend nights of younger and older individuals. The results of all such tests provide clear evidence that confirms our estimated baseline effects.

We then use the baseline impact estimates to quantify the externality of binge drinking, and carry out several calculations with different estimated effects and different outcome specific unit costs. A conservative estimate of the externality is £4.86 billion per year. This implies a negative externality of 9p extra per alcoholic unit or an additional tax of 95p per bottle of a standard red wine and 22p per pint of beer. The implied Pigouvian tax that internalizes this externality is nearly £7,300 per arrest.

The methodology we present provides a simple tool for analyzing the causal effect of binge drinking. Alternative methods that rely on direct blood alcohol content or urine test results are too expensive to be performed on large representative populations, as revealed by the very small sample sizes of most of the medical studies reported in the meta-analyses and reviews by Wilk, Jensen, and Havighurst (1997), Rehm et al. (2003), and Courtney and Polich (2009). Their generalizability is therefore questionable. Others that refer to special subpopulations, such as college students (e.g., Wechsler et al. 1994; Boyd, McCabe, and Morales 2005; LaBrie, Pedersen and Tawalbeth 2007; Miller et al. 2007), specific hospital patients and problem drinkers (e.g., Meyerhoff et al. 2004; Cardenas et al. 2005), are subject to important selection biases.

Our approach instead can be easily implemented using information on alcohol usage from one sample and data on outcomes from another sample. There might be a value in not collecting outcomes together with measures of alcohol involvement, as these can be intentionally misreported either down (Brener, Billy, and Gradyet 2003) or up (Ekholm et al. 2008; Boniface and Shelton 2013), or because interviewee's response behaviour may be influenced by the nature of the interview setting (Del Boca and Darkes 2003). The two-sample approach is likely to be useful also in several other substantive applications where concerns about data availability are similar to ours and information on risky behaviors (e.g., illicit drug use, smoking, and unprotected sex) is not collected with outcomes, such as arrests, salaries, teen pregnancies, and sexually transmitted diseases.

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Figure 1.A: Total Number of Injury Related A&E Attendances, by Hour of the Week

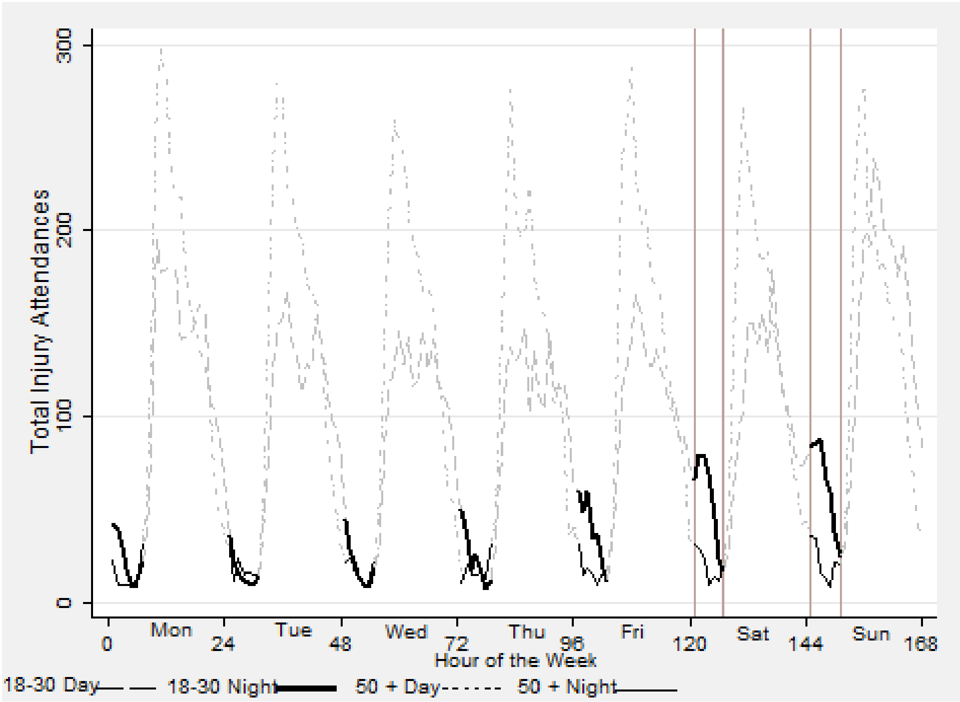
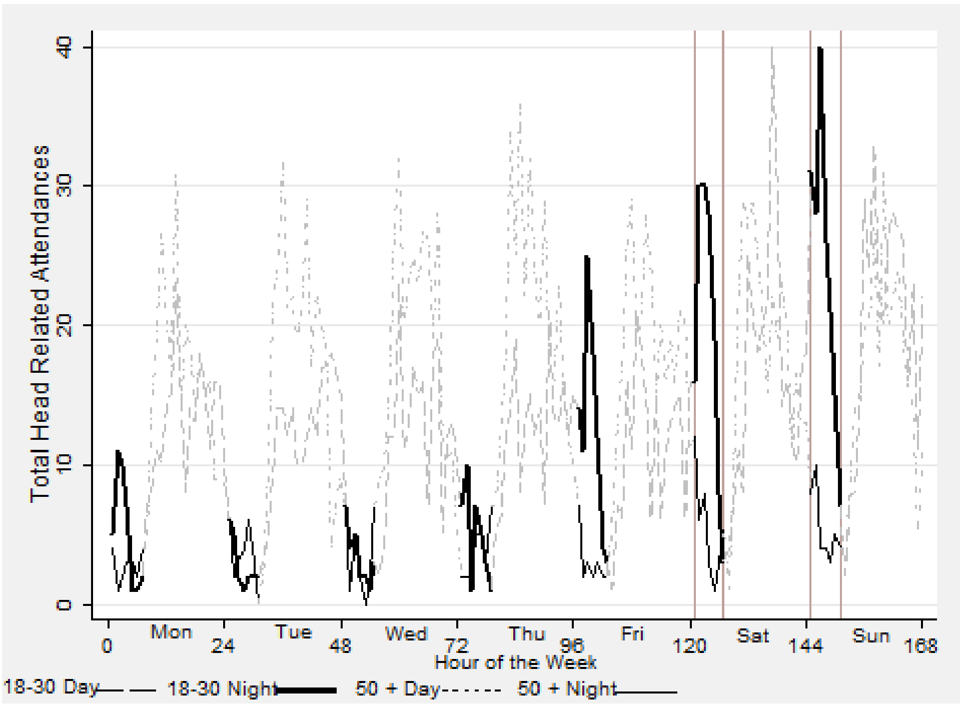


Figure 1.B: Total Number of Head Injury A&E Attendances, by Hour of the Week



Source: Solihull Care Trust (SCT) data, 1 April 2008–21 January 2011.
Note: Total numbers are averaged over the sample period. Along the horizontal axis, 0 denotes the first hour of Monday (00:00–00:59) and 168 denotes the last hour of Sunday (23:00–23:59). The vertical lines indicate the weekend nights.

Figure 2.A: Total Number of Road Accidents, by Hour of the Week

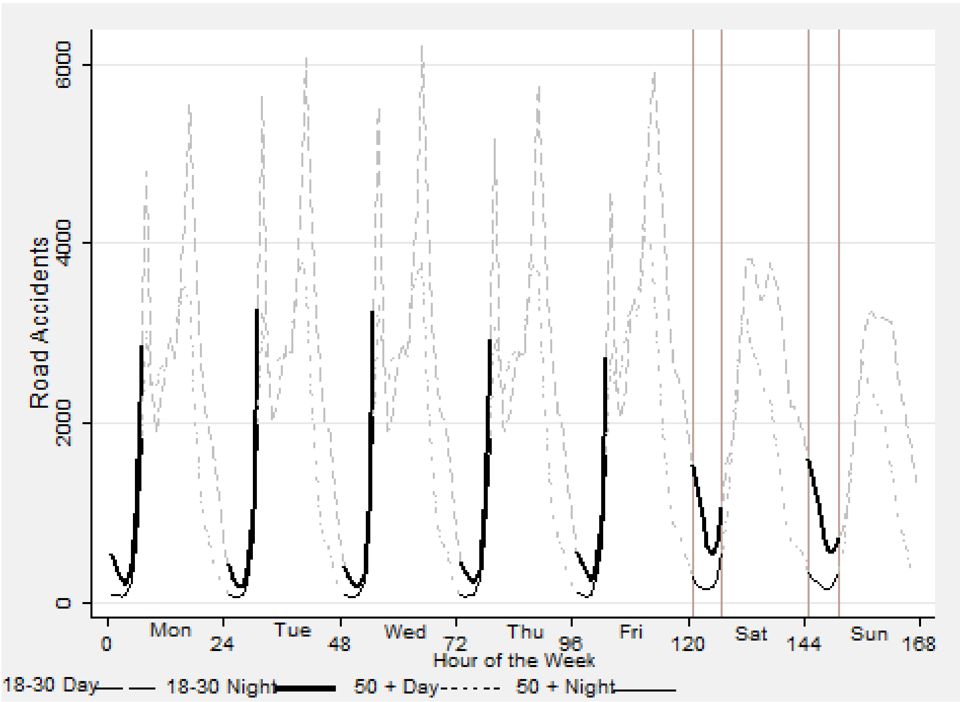
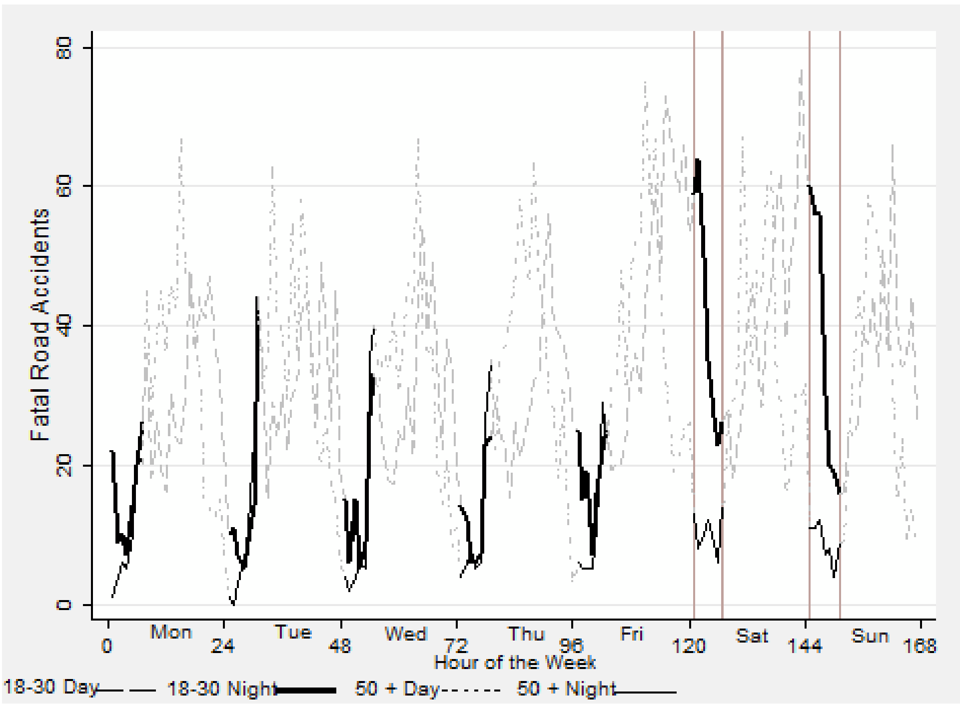
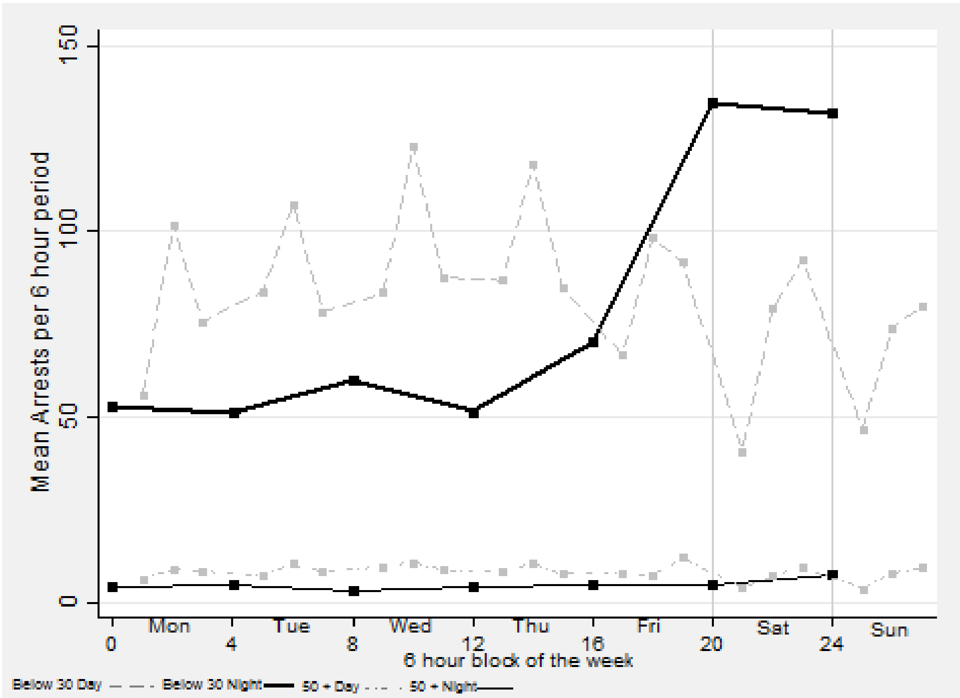


Figure 2.B: Total Number of Road Fatalities, by Hour of the Week



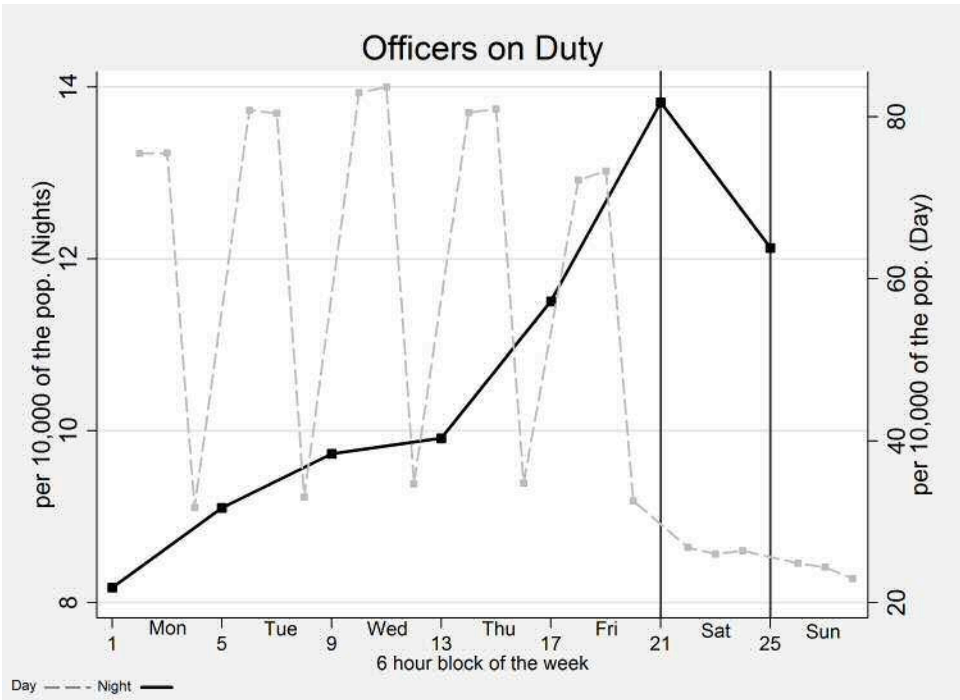
Source: Department of Transport, Road Accidents Data (RAD), 2006–2009.
Note: See the note to Figure 1.

Figure 3: Total Number of Arrests per 6 hour block



Sources: Metropolitan Police Service and West Midlands Police, FOI request; one week in February, one in May, one in August, and one in November, 2009–2011.
Note: Total numbers are averaged over the sample period. Along the horizontal axis, 0 denotes the first 6-hour block of Monday (00:00–05:59) and 24 denotes the last six-hour of Sunday (18:00–23:59). The vertical lines indicate the weekend nights.

Figure 4: Total Number of Police Officers on Duty, per Night (00:00-05:59)



Sources: Metropolitan Police Service and Durham Police Service, FOI request; one week in February, one in May, one in August, and one in November, 2009–2011.
Note: See the note to Figure 3.

Figure 5.A: Alternative Instruments, A&E Attendances

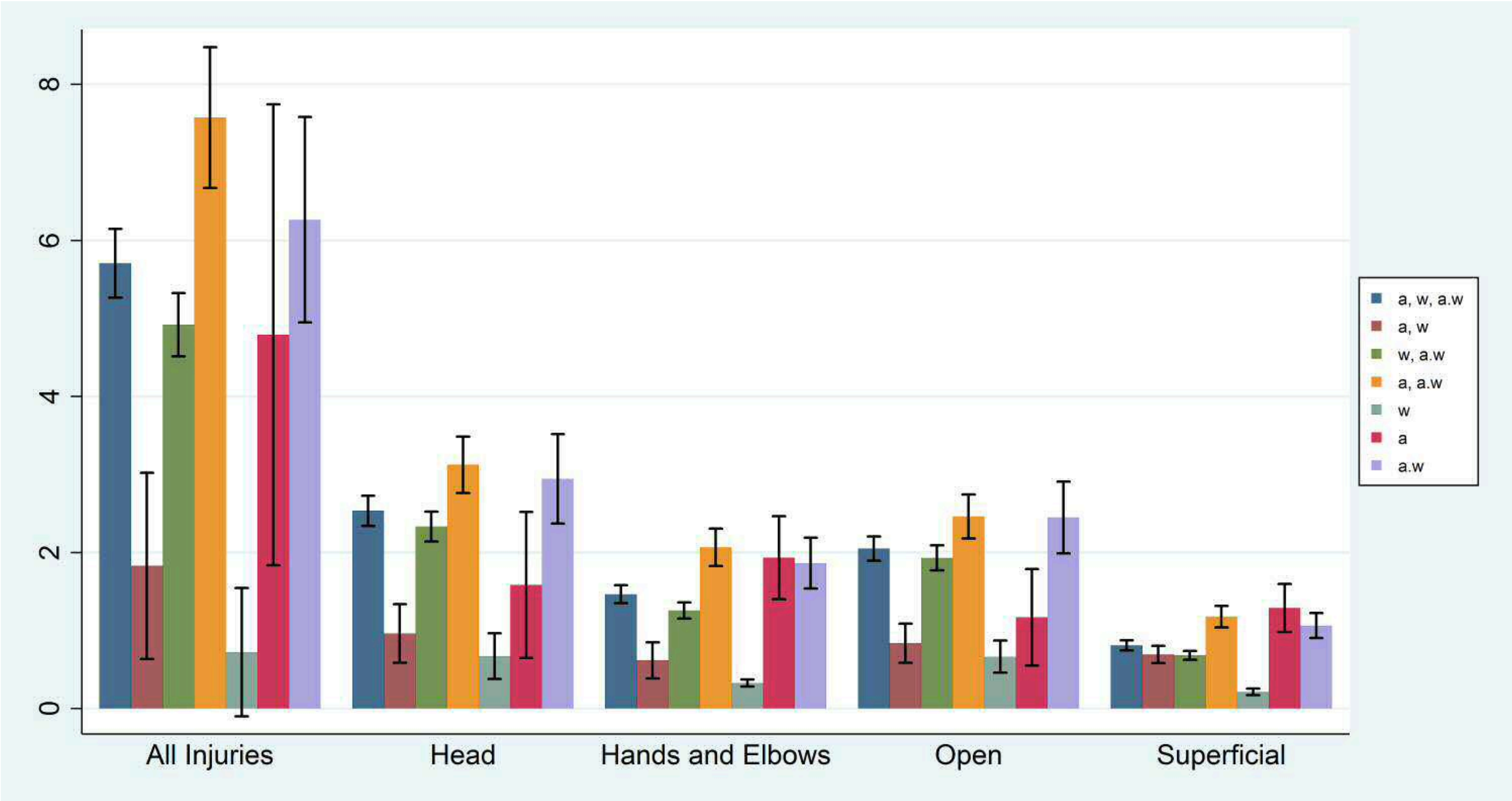


Figure 5.B: Road Accidents

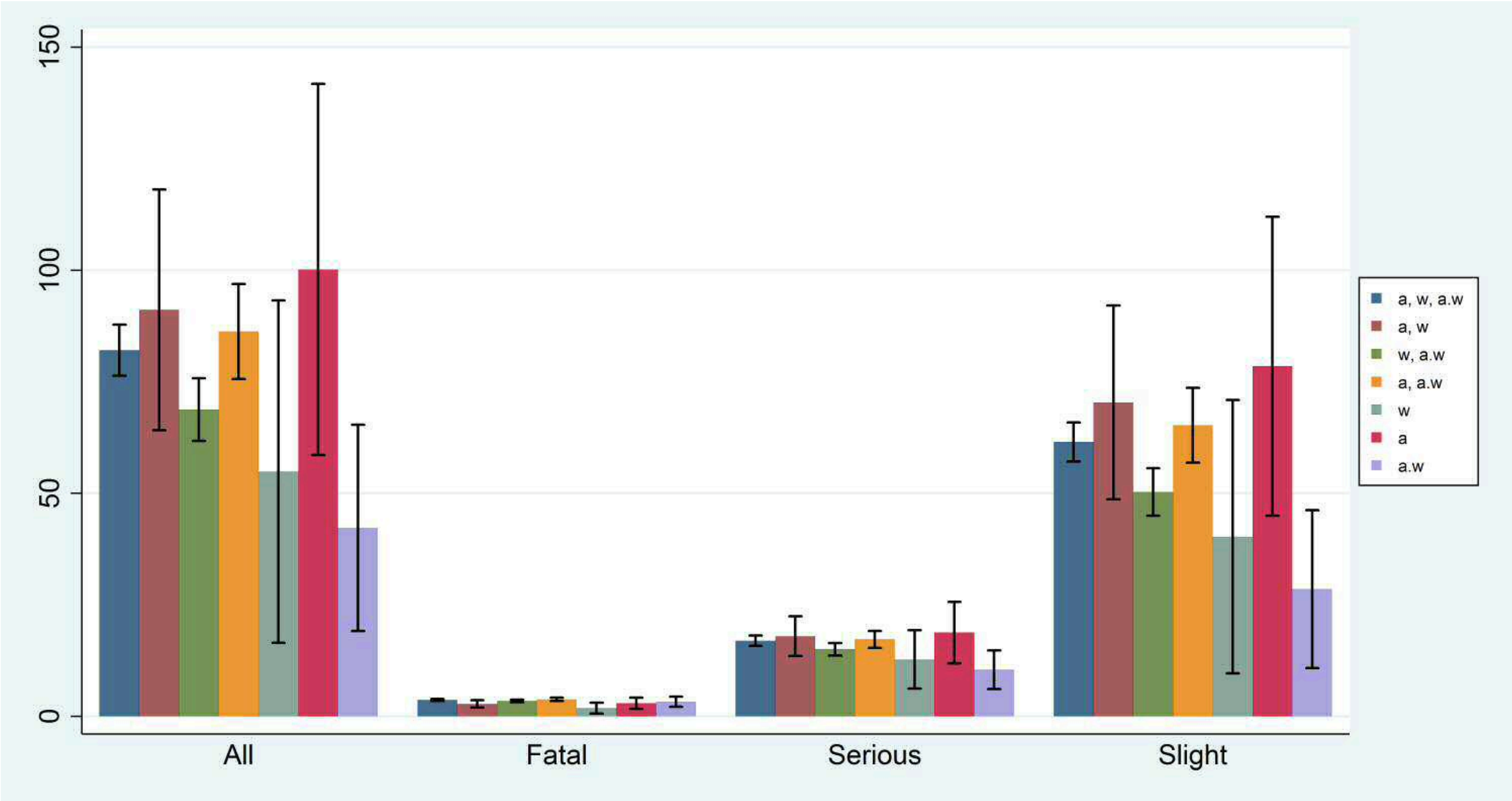
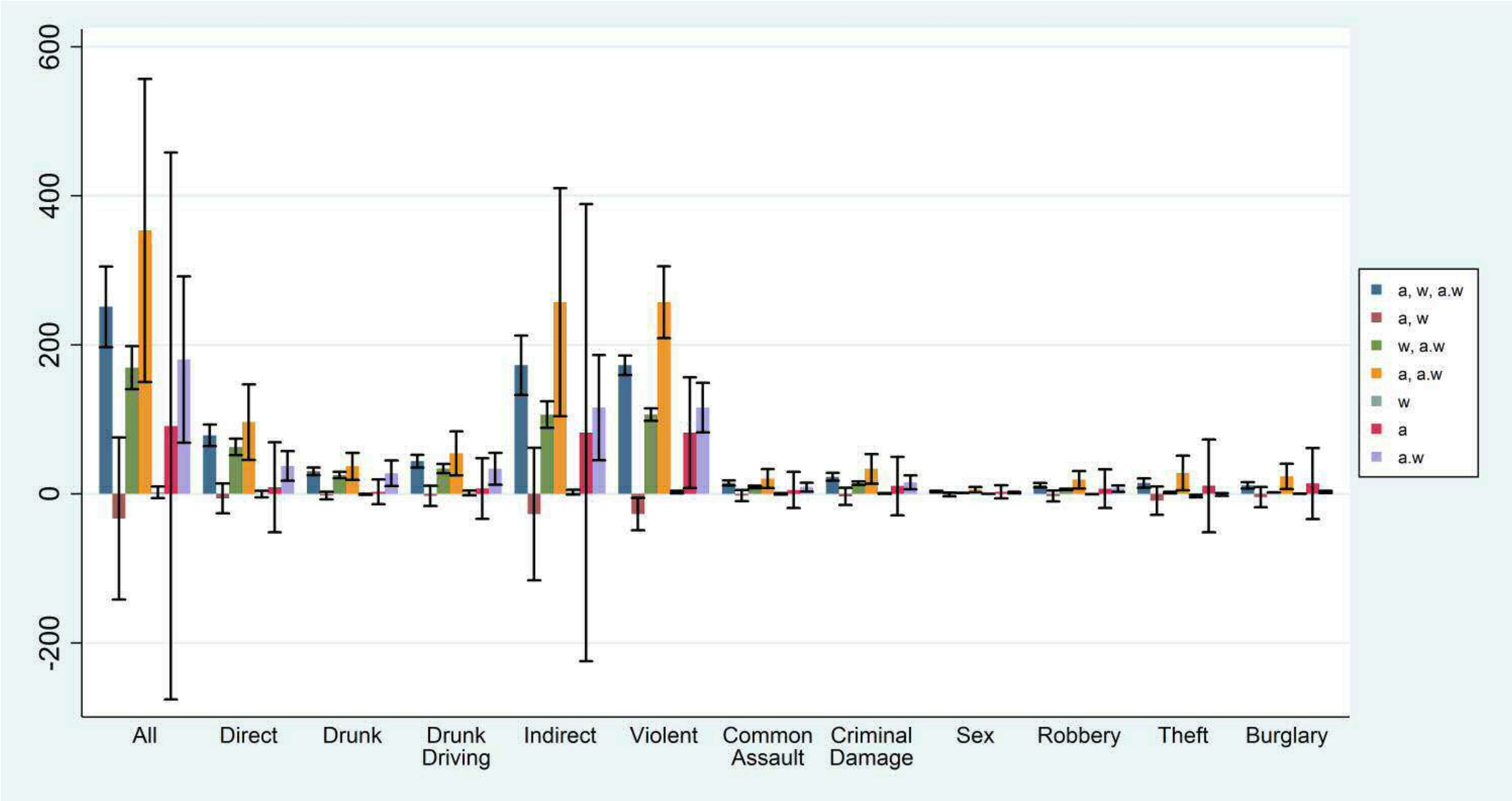
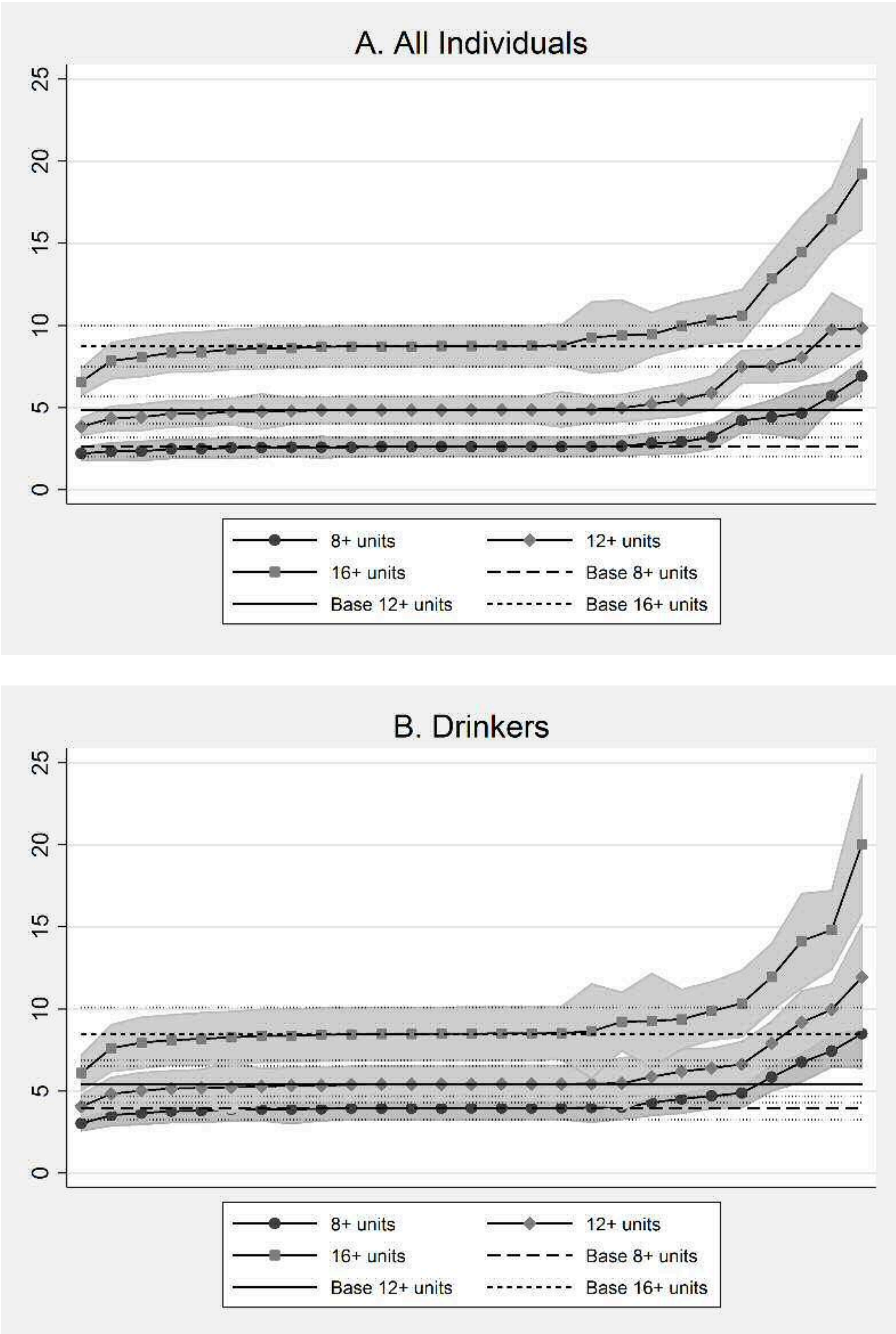


Figure 5.C: Arrests



Note: Each bar corresponds to an estimate for binge drinking defined as 12+ units using a different set of instruments. a is an indicator variable for the 18–30 age group, w is an indicator variable for the weekend, and $a \cdot w$ is the interaction term. Whiskers represent the 95% confidence interval.

Figure 6: Distribution of Alternative Externality Estimates



Note: Each dot represents the total burden associated with binge drinking with the unit cost of one specific component changed at a time. This alternative cost procedure is documented in Appendix C. Each alternative total cost is then ranked with the smallest on the left and the largest on the right. The grey areas denote the 95% confidence interval for each binge drinking definition. The horizontal lines represent the baseline estimates from Table 9.

Table 1: Reduced Form Effects of Binge Drinking

	A. A&E Attendances (N=182)					B. Road Accidents (N=224)				
	All injuries	Head	Hands and Elbows	Open wounds	Superficial wounds	All accidents	Fatal	Serious	Slight	Nr. of casualties
π_3	1.35	0.60	0.34	0.49	0.18	12.35	0.744	2.62	8.99	22.3
	(0.138)	(0.068)	(0.049)	(0.052)	(0.033)	(0.568)	(0.059)	(0.184)	(0.441)	(0.924)
R^2	0.72	0.65	0.57	0.67	0.49	0.950	0.791	0.889	0.947	0.952

C. Arrests (N=168)

	Direct				**Indirect**							
	All	Direct	Drunk	Driving	Indirect	Violent	Common assault	Criminal damage	Sexual	Robbery	Theft	Burglary
π_3	74.30	27.55	11.45	14.37	46.75	21.95	3.98	6.25	0.62	2.88	1.23	0.87
	(3.958)	(1.518)	(0.798)	(1.074)	(3.253)	(1.391)	(0.622)	(0.963)	(0.393)	(0.777)	(1.126)	(0.973)
R^2	0.940	0.891	0.807	0.840	0.925	0.889	0.698	0.708	0.348	0.558	0.679	0.627

	D. Police Officers (N=167)		
	All forces	MPS (London)	Durham Constab.
π_1	3.24	2.54	3.95
	(0.288)	(0.457)	(0.288)
R^2	0.726	0.476	0.786

Note: Estimates are obtained from OLS regressions. Standard errors are in parentheses. The data are aggregated into cell means by year, quarter of the year, day of the week, and age group. With these aggregations the number of observations in panel A are 182 (=7 days $\times$ 3 years $\times$ 2 age groups $\times$ 4 quarters = 168 + 14 observations of an additional quarter [=7 days $\times$ 1 year $\times$ 2 age groups $\times$ 1 quarter]), in panel B 224 (=7 days $\times$ 4 years $\times$ 2 age groups $\times$ 4 quarters), in panel C 168 (=7 days $\times$ 3 years $\times$ 2 age groups $\times$ 4 quarters), and in panel D 167 (the same as in panel panel C minus one missing observation). All regressions are weighted by cell size, and include controls for the weekend, an age group dummy variable equal to 1 if the individual is aged between 18 and 30 years and zero if the individual is aged 50 or more. The π_3 coefficient refers to the estimate on the interaction term between the weekend indicator, w, and the 18–30 indicator, a, in equation (4), while the π_1 coefficient in panel D refers to the estimate on w.

Table 2: First Stage Estimates on Binge Drinking

	All			Drinkers		
	8+ units	12+ units	16+ units	8+ units	12+ units	16+ units
A. A&E Attendances						
α_1	0.225	0.102	0.044	0.176	0.090	0.040
	(0.007)	(0.005)	(0.004)	(0.009)	(0.007)	(0.005)
α_2	0.034	0.049	0.044	0.101	0.097	0.084
	(0.008)	(0.006)	(0.004)	(0.015)	(0.011)	(0.009)
α_3	0.202	0.169	0.118	0.141	0.124	0.082
	(0.013)	(0.010)	(0.008)	(0.020)	(0.015)	(0.011)
Observations	17,383	17,383	17,383	10,791	10,791	10,791
R^2	0.197	0.142	0.097	0.145	0.125	0.094
F-test	912.9	634.2	432.7	385.0	339.5	261.5
B. Road Accidents						
α_1	0.216	0.100	0.041	0.167	0.084	0.036
	(0.006)	(0.004)	(0.003)	(0.008)	(0.006)	(0.004)
α_2	0.079	0.073	0.057	0.180	0.141	0.108
	(0.007)	(0.005)	(0.004)	(0.012)	(0.009)	(0.007)
α_3	0.178	0.159	0.114	0.090	0.098	0.068
	(0.011)	(0.008)	(0.006)	(0.016)	(0.012)	(0.009)
Observations	25,041	25,041	25,041	15,557	15,557	15,557
R^2	0.174	0.129	0.093	0.133	0.113	0.091
F-test	1,347.0	1,003.0	709.8	623.3	561.4	446.3
C. Arrests						
α_1	0.311	0.159	0.0643	0.245	0.137	0.0544
	(0.013)	(0.010)	(0.008)	(0.016)	(0.013)	(0.010)
α_2	0.0320	0.0574	0.0607	0.0754	0.0842	0.0884
	(0.017)	(0.013)	(0.010)	(0.027)	(0.022)	(0.017)
α_3	0.125	0.177	0.149	0.0947	0.155	0.125
	(0.026)	(0.021)	(0.016)	(0.036)	(0.028)	(0.021)
Observations	5,979	5,979	5,979	4,287	4,287	4,287
R^2	0.169	0.141	0.100	0.112	0.117	0.091
F-test	323.4	254.0	177.0	149.8	148.8	117.3
D. Policy Officers on Duty						
α_1	0.225	0.102	0.044	0.176	0.090	0.040
	(0.007)	(0.005)	(0.004)	(0.009)	(0.007)	(0.005)
Observations	17,383	17,383	17,383	10,791	10,791	10,791
R^2	0.197	0.142	0.097	0.145	0.125	0.094
F-test	1,039.0	384.8	124.8	365.8	168.2	60.6

Note: Estimates are obtained from the Health Survey of England (2008–2010 in panels A and D; 2006–2009 in panel B; 2009–2011 and males only in panel C). In each panel, the dependent variables is the self-reported units of alcohol drunk on the heaviest day in the past week. The 'Drinkers' subsample includes only individuals who report having drunk in the past seven days. The 'All' sample includes both drinkers and non-drinkers in the past seven days. The coefficient α_1 is on w (=1 if an individual drank most in last seven days on a Friday or Saturday, =0 otherwise); α_2 is on a (=1 if an individual is between 18 and 30 years of age, =0 if individual is aged 50 or more); α_3 is on $a \times w$. Standard errors are in parentheses. The F-test statistic refers to the joint significance of a, w, and $a \times w$. Additional controls that are not reported are indicators for gender (=1 if male), race (=1 if white), whether the respondent had a long standing illness (=1 if yes), whether the respondent had ever been a smoker (=1 if yes), the age at which the individual left full time education, and a set of year and quarter dummy variables.

Table 3: Effects of Binge Drinking on A&E attendances (β_1)

		All			Drinkers		
	Mean	8+ units	12+ units	16+ units	8+ units	12+ units	16+ units
All Injuries	70.6	3.29	5.71	9.42	4.29	6.09	8.92
		(0.10)	(0.23)	(0.53)	(0.15)	(0.25)	(0.51)
Head	11.1	1.51	2.54	4.1	1.9	2.64	3.78
		(0.04)	(0.10)	(0.23)	(0.06)	(0.11)	(0.23)
Hand & Elbows	14.0	0.86	1.47	2.4	1.11	1.57	2.28
		(0.02)	(0.06)	(0.14)	(0.04)	(0.07)	(0.13)
Open	10.2	1.23	2.06	3.29	1.53	2.12	3.00
		(0.03)	(0.08)	(0.18)	(0.05)	(0.09)	(0.18)
Superficial	6.9	0.47	0.81	1.33	0.62	0.88	1.27
		(0.01)	(0.03)	(0.08)	(0.02)	(0.04)	(0.07)

Note: Estimates obtained from two-sample minimum distance estimation (TS-MDE) using the optimal weighting matrix. Each coefficient represents a separate estimation. Bootstrapped standard errors obtained using 1,000 replications are reported in parentheses. First stage (first sample) estimation uses the estimates obtained from HSE data and reported in Table 2 panel A. 'Mean' refers to the daily average number of A&E attendances.

Table 4: Effects of Binge Drinking on Road Accidents (β_1)

		All			Drinkers		
	Mean	8+ units	12+ units	16+ units	8+ units	12+ units	16+ units
All	482.8	49.2	82.1	129.3	59.8	79.4	111.7
		(2.2)	(2.9)	(6.1)	(1.8)	(3.2)	(6.1)
Fatal	6.9	2.3	3.7	5.7	2.6	3.4	4.7
		(0.1)	(0.1)	(0.3)	(0.1)	(0.2)	(0.3)
Serious	64.6	10.5	17.0	26.4	12.2	16.0	22.3
		(0.4)	(0.6)	(1.2)	(0.4)	(0.7)	(1.3)
Slight	411.4	36.5	61.5	97.3	45.1	60.1	84.8
		(1.7)	(2.2)	(4.6)	(1.3)	(2.3)	(4.5)

Note: Estimates obtained from two-sample minimum distance estimation (TS-MDE) using the optimal weighting matrix. Each coefficient represents a separate estimation. Bootstrapped standard errors obtained using 1,000 replications are reported in parentheses. First stage (first sample) estimation uses the estimates obtained from HSE data and reported in Table 2 panel B. 'Mean' refers to the daily average number of arrests.

Table 5: Effects of Binge Drinking on Arrests (β_1)

		All			Drinkers		
	Mean	8+ units	12+ units	16+ units	8+ units	12+ units	16+ units
All Arrests	554.4	133.8	251.1	469.9	224.0	303.9	478.6
		(20.2)	(27.6)	(41.5)	(22.6)	(36.8)	(52.5)
Direct	60.6	46.1	78.5	136.4	68.4	90.3	132.0
		(5.4)	(7.4)	(11.3)	(5.7)	(9.2)	(14.9)
Drunk	20.1	18.0	30.1	51.0	25.7	34.0	48.4
		(1.9)	(2.6)	(4.2)	(2.1)	(3.2)	(5.7)
Drunk Driving	38.3	25.4	43.8	77.4	38.7	51.0	75.8
		(3.2)	(4.3)	(6.5)	(3.3)	(5.4)	(8.4)
Indirect	493.8	87.7	172.7	333.5	155.6	213.7	346.6
		(14.8)	(20.4)	(30.5)	(17.4)	(27.9)	(38.1)
Violent	126.1	38.5	68.2	121.6	59.4	80.1	120.1
		(4.8)	(6.6)	(10.4)	(5.3)	(8.5)	(13.6)
Common Assault	69.3	7.7	14.7	28.1	13.4	18.0	29.0
		(1.3)	(1.7)	(2.5)	(1.4)	(2.3)	(3.1)
Criminal Damage	46.5	11.8	22.9	43.9	20.6	28.2	45.5
		(1.9)	(2.7)	(4)	(2.2)	(3.6)	(5.0)
Sex	23.1	1.4	3.2	6.8	3.0	4.2	7.5
		(0.3)	(0.5)	(0.7)	(0.4)	(0.7)	(0.9)
Robbery	41.4	5.4	11.6	23.6	10.6	14.9	25.3
		(1.1)	(1.5)	(2.3)	(1.4)	(2.2)	(2.8)
Theft	107.8	4.5	14.6	36.7	15.3	21.6	43.2
		(2.3)	(3.2)	(4.5)	(3.4)	(4.9)	(5.2)
Burglary	48.7	3.7	11.5	27.5	11.3	16.6	32.1
		(1.6)	(2.2)	(3.3)	(2.3)	(3.4)	(3.9)

Note: Estimates obtained from two-sample minimum distance estimation (TS-MDE) using the optimal weighting matrix. Each coefficient represents a separate estimation. Bootstrapped standard errors obtained using 1,000 replications are reported in parentheses. First stage (first sample) estimation uses the estimates obtained from HSE data and reported in Table 2 panel C. 'Mean' refers to the daily average number of arrests.

Table 6: Effects of Binge Drinking on Police Numbers (β_1)

		All			Drinkers		
	Mean	8+ units	12+ units	16+ units	8+ units	12+ units	16+ units
All	42.2	-1.0	6.7	75.5	6.8	8.1	90.1
		(3.7)	(13.8)	(50.5)	(9.9)	(25.2)	(70.1)
MPS	56.8	-6.05	-3.7	59.3	1.1	-2.8	75.8
		(3.8)	(14.9)	(57.7)	(10.9)	(27.2)	(78.8)
Durham	27.1	-4.7	2.8	95.7	5.9	4.3	116.4
		(5.33)	(20.4)	(75.2)	(14.7)	(36.9)	(103.2)

Note: Estimates obtained from two-sample minimum distance estimation (TS-MDE) using the optimal weighting matrix. Each coefficient represents a separate estimation. Bootstrapped standard errors obtained using 1,000 replications are reported in parentheses. First stage (first sample) estimation uses the estimates obtained from HSE data and reported in Table 2 panel D. 'Mean' refers to the daily average number of police officers on duty.

Table 7: Effects of Binge Drinking on A&E Attendances, Road Accidents, and Arrests using Time Diaries in the First Stage

	6pm - Midnight	8pm - 11pm	8pm - Midnight	8pm - 2am	9pm - 3am
A. A&E Attendances					
All Injuries	24.7	17.6	19.3	25.5	31.4
	(2.2)	(1.7)	(1.8)	(2.4)	(3.1)
Head	10.5	7.5	8.3	10.9	13.4
	(0.9)	(0.7)	(0.8)	(1.0)	(1.3)
Hand Elbows	6.3	4.5	4.9	6.5	7.9
	(0.5)	(0.4)	(0.4)	(0.6)	(0.8)
Open	8.3	6	6.6	8.7	10.7
	(0.7)	(0.6)	(0.6)	(0.8)	(1.0)
Superficial	3.5	2.5	2.7	3.6	4.4
	(0.3)	(0.2)	(0.3)	(0.3)	(0.4)
B. Road Accidents					
All accidents	333.0	237.8	259.8	344.4	423
	(29.1)	(22.2)	(24.0)	(31.8)	(41.1)
Fatal	14.4	10.3	11.3	15.1	18.5
	(1.3)	(1.0)	(1.1)	(1.4)	(1.8)
Serious	67	47.9	52.4	69.5	85.5
	(5.9)	(4.5)	(4.9)	(6.4)	(8.3)
Slight	251.6	179.7	196.2	260	319
	(22.0)	(16.8)	(18.1)	(24.0)	(31.1)
C. Arrests					
All Arrests	1452.1	1048.6	1118.3	1430.4	1690.3
	(157.4)	(120.2)	(127.2)	(160.5)	(209)

Continued on next page

Table 7 – *Continued from previous page*

	6pm - Midnight	8pm - 11pm	8pm - Midnight	8pm - 2am	9pm - 3am
Direct	1452.1	1048.6	1118.3	1430.4	1690.3
	(157.4)	(120.2)	(127.2)	(160.5)	(209)
Drunk	170.0	144.3	104.6	112.4	143.5
	(22.3)	(16.5)	(12.6)	(13.5)	(17)
Drunk Driving	267.7	228.0	164.8	176.7	226.0
	(33.7)	(25)	(19.1)	(20.4)	(25.7)
Indirect	1224.2	1055.9	761.8	810.7	1037.0
	(150.1)	(114)	(87)	(91.7)	(115.8)
Violent	419.9	359.6	260.1	278.0	355.2
	(53.5)	(40.1)	(30.6)	(32.5)	(41)
Common Assault	103.0	88.5	63.8	68.0	87.1
	(12.5)	(9.4)	(7.2)	(7.6)	(9.6)
Criminal Damage	160.4	138.2	99.7	106.2	135.8
	(19.7)	(14.9)	(11.4)	(12)	(15.2)
Sex	26.2	22.9	16.5	17.4	22.3
	(3.2)	(2.5)	(1.9)	(2)	(2.5)
Robbery	89.5	77.5	55.9	59.3	75.9
	(10.9)	(8.3)	(6.4)	(6.7)	(8.4)
Theft	158.6	138.1	99.1	104.6	134.4
	(18.4)	(14.5)	(11.1)	(11.5)	(14.5)
Burglary	114.2	100.2	72.0	75.8	97.1
	(13.8)	(11)	(8.4)	(8.7)	(10.9)
D. Police on Duty					
All	1068.9	637.2	698.8	921.0	1164.6
	(4955.3)	(1179.7)	(2423.5)	(2378.7)	(1833.3)
MPS	999.7	617.2	663.1	873.1	1075.8
	(4643)	(1190.1)	(2505.3)	(2424.8)	(1854.8)
Durham	1432.2	867.8	940.1	1238.5	1548.1
	(6744)	(1646.9)	(3440.1)	(3333)	(2560.8)

Note: Estimates obtained from two-sample minimum distance estimation (TS-MDE) using the optimal weighting matrix. Each coefficient represents a separate estimation. Bootstrapped standard errors obtained using 1,000 replications are reported in parentheses. First stage (first sample) estimation uses the estimates obtained from UK-TUS data and reported in Appendix Table A2 panel A for panels A–C above and Appendix Table A2 panel B for panel D above.

Table 8: Concurrent Use of Illicit Drugs and Alcohol and Its Correlation with Arrests

	(a) Extent of drug use	(b) Prevalence rate of alcohol-drug co-use	(c) Correlation between co-use and arrest	(d) R^2	(e) Observations
Any drug (48 hours)					
All arrests	17.1	5.42	-0.015	0.256	19,453
			(0.006)		
Violence	2.7	1.33	0.010	0.138	6,195
			(0.008)		
Theft	10.8	2.73	-0.034	0.295	6,789
			(0.013)		
Drink/drug	1.83	0.74	-0.011	0.151	2,423
			(0.016)		
Other crime	1.73	0.73	0.009	0.154	4,033
			(0.010)		
Cocaine (48 hours)					
All arrests	4.05	2.54	0.037	0.022	19,281
			(0.004)		
Heroin (48 hours)					
All arrests	12.37	2.6	-0.053	0.320	19,355
			(0.004)		
Crack (48 hours)					
All arrests	7.64	1.87	-0.019	0.161	19,305
			(0.004)		
Any drug (month)					
All arrests	57.13	22.68	0.021	0.0761	19,453
			(0.015)		
Cannabis (month)					
All arrests	47.99	19.27	0.0125	0.166	19,359
			(0.008)		

Source: Arrestee Survey, 2003–2006.
Note: Column (a) reports the prevalence rate of drug use; column (b) shows the prevalence rate of combined use of alcohol and drug; column (c) reports the OLS estimate of drug use on binge drinking controlling for age, sex, ethnicity, presence of children, health and drug problems, and indicators for arrest and prison histories; column (d) presents the R^2 of column (c)'s regression; column (e) reports the number of observations (individuals) used in the regression.

Table 9: The Externality of Binge Drinking

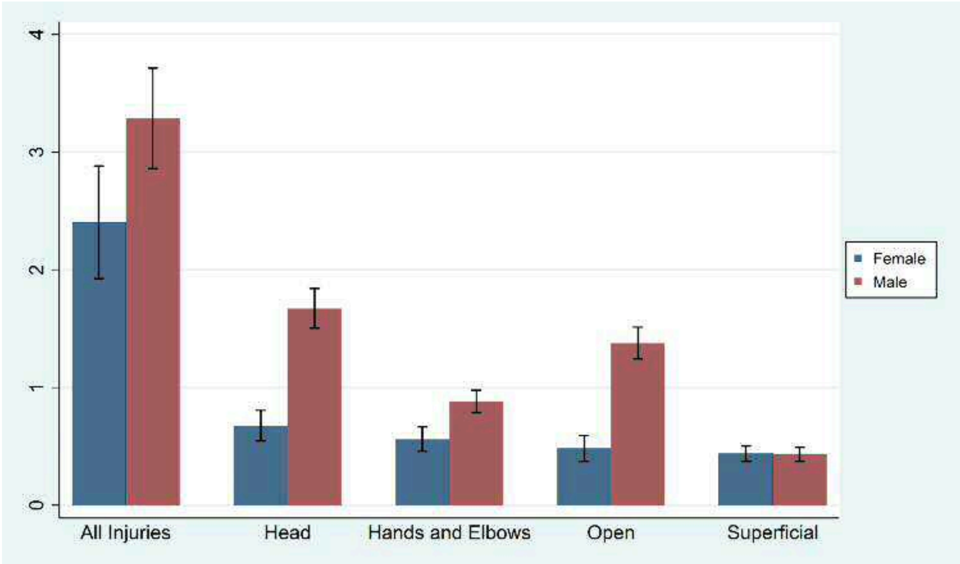
	All Population			Drinker		
	8+ units	12+ units	16+ units	8+ units	12+ units	16+ units
A. Baseline						
A&E	0.026	0.045	0.073	0.033	0.047	0.070
	[0.024, 0.027]	[0.041, 0.048]	[0.065, 0.082]	[0.031, 0.036]	[0.044, 0.051]	[0.062, 0.077]
Road	1.258	2.043	3.179	1.444	1.900	2.648
	[1.166, 1.35]	[1.907, 2.18]	[2.892, 3.466]	[1.349, 1.538]	[1.734, 2.066]	[2.334, 2.962]
Arrests	1.322	2.746	5.470	2.469	3.448	5.748
	[0.834, 1.81]	[2.07, 3.421]	[4.521, 6.418]	[1.85, 3.087]	[2.497, 4.399]	[4.458, 7.038]
Police Numbers	0.030	0.030	0.030	0.030	0.030	0.030
	[0.024, 0.035]	[0.024, 0.035]	[0.024, 0.035]	[0.024, 0.035]	[0.024, 0.035]	[0.024, 0.035]
Total	2.635	4.863	8.752	3.975	5.425	8.495
	[2.049, 3.222]	[4.042, 5.684]	[7.504, 9.99]	[3.255, 4.695]	[4.299, 6.552]	[6.878, 10.112]
B. Excluding Home Office Multiplier						
Arrests	0.479	0.896	1.669	0.774	1.056	1.653
	[0.344, 0.613]	[0.711, 1.082]	[1.082, 1.669]	[0.616, 0.933]	[0.808, 1.305]	[1.283, 2.023]
Total	1.792	3.014	5.470	2.281	3.033	4.400
	[1.559, 2.025]	[2.683, 3.345]	[4.385, 5.516]	[2.017, 2.542]	[2.609, 3.457]	[3.702, 5.098]

Note: Figures, which are expressed in billion pounds sterling (2014 prices), are obtained using the TS-MDE effects (shown in Tables 3–5 for A&E attendances, road accidents, and arrests, and in panel D of Table 1 for police officers on duty) and the unit costs presented in Appendix B. The 95% confidence intervals are presented in square brackets.

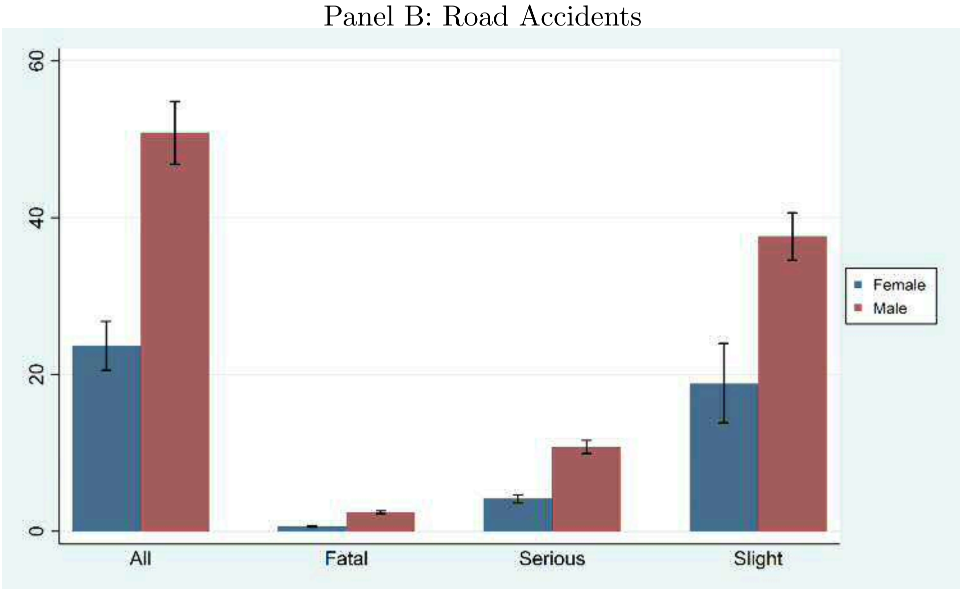
Appendix A: Additional Figures and Tables

Appendix Figure A1: Gender Splits

Panel A: A & E Attendances

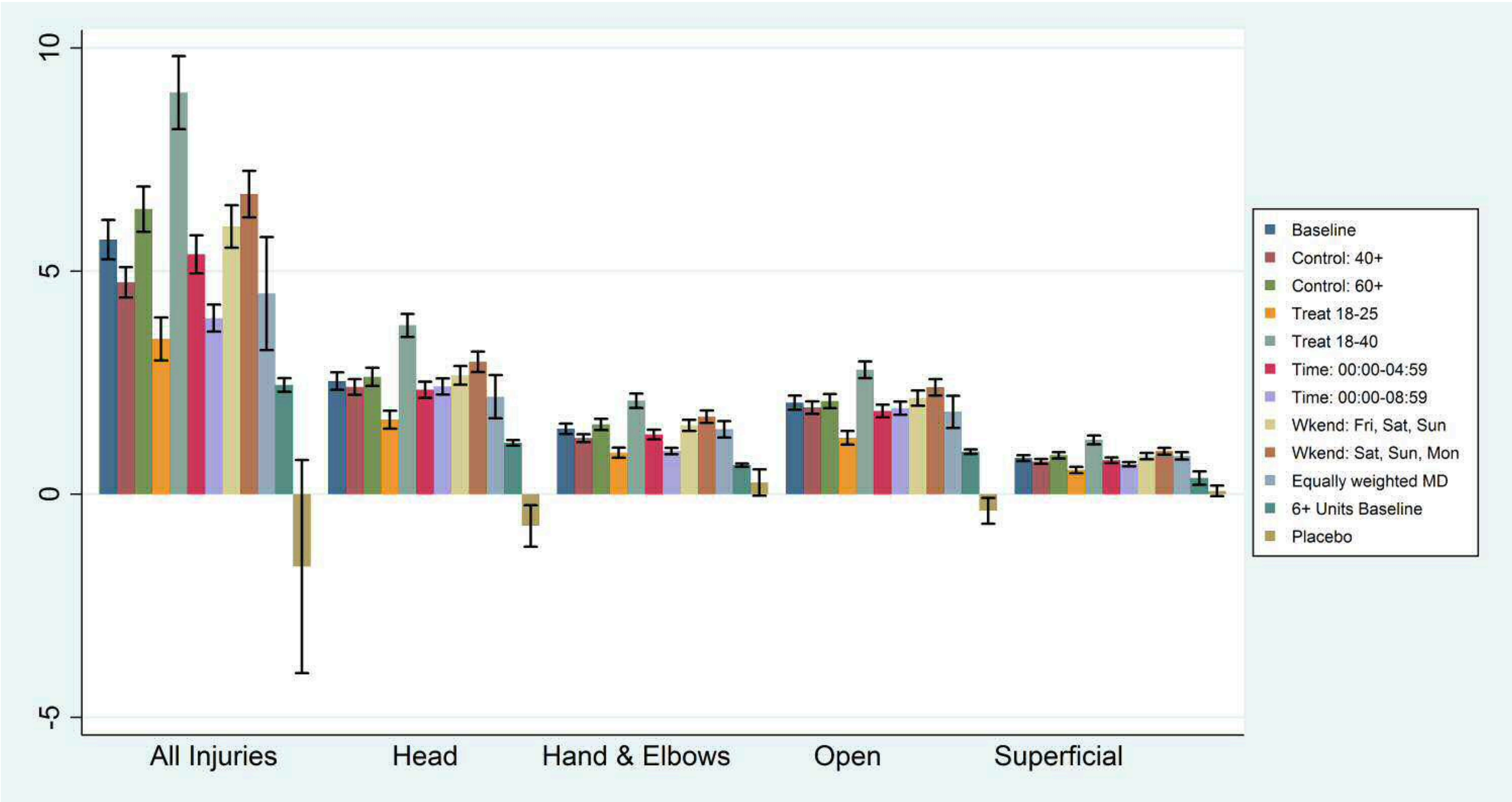


Panel B: Road Accidents



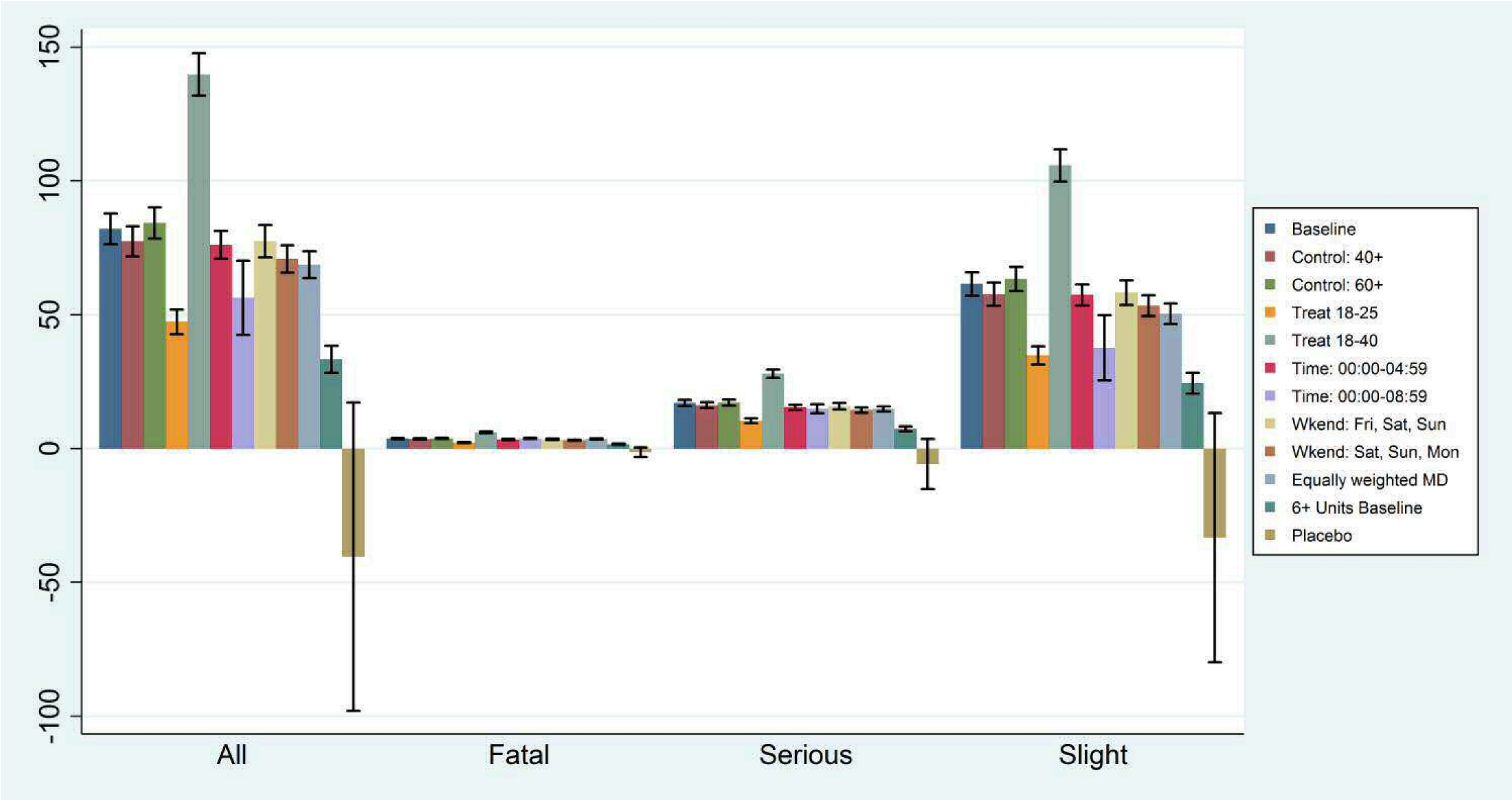
Note: Each bar represents the outcome specific TS-MDE effect obtained with the optimal weighting matrix. The whiskers depict the 95% confidence interval.

Appendix Figure A2.A: Sensitivity Analysis for A&E Attendances



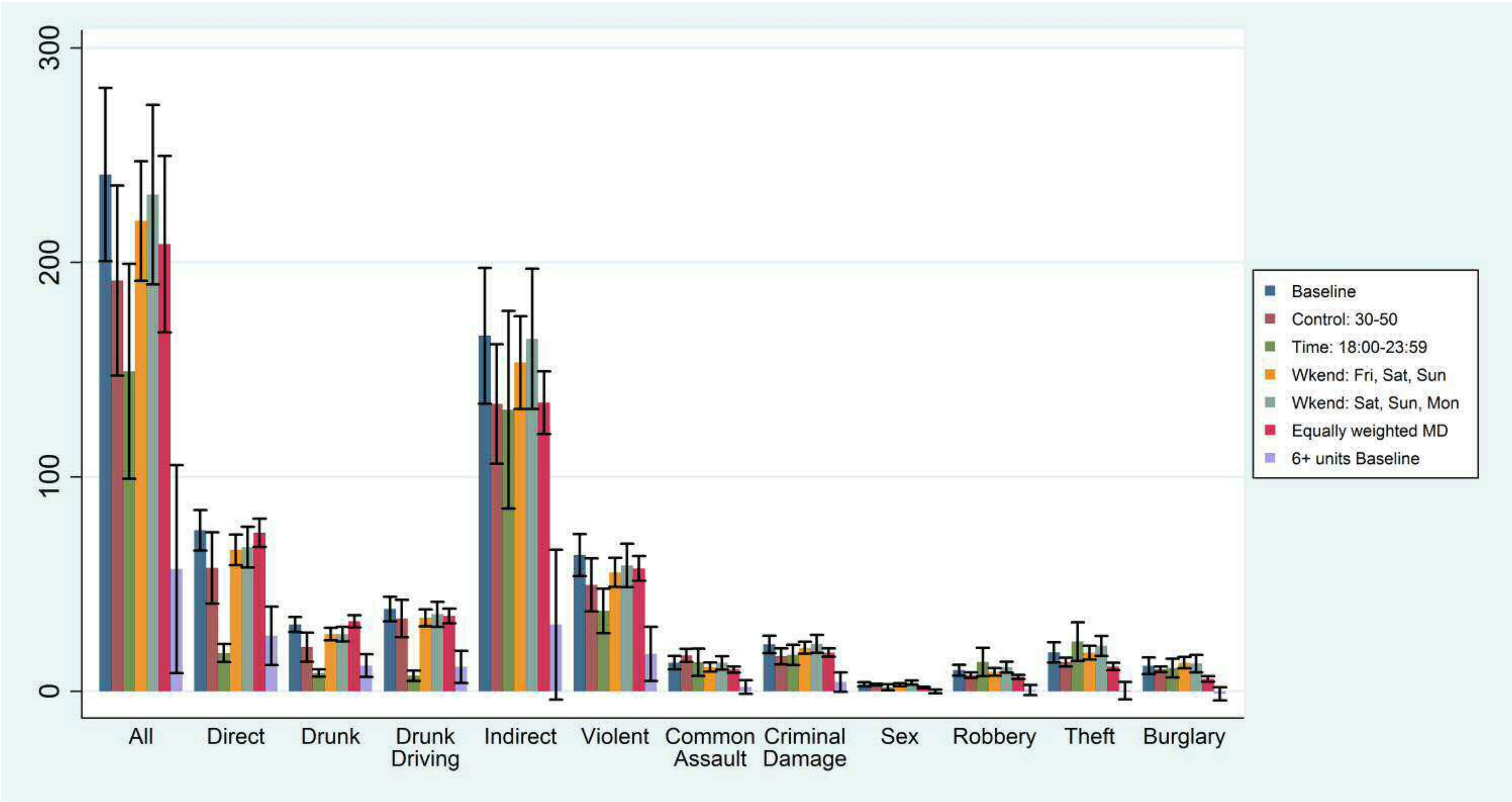
Note: Each bar represents the outcome specific TS-MDE effect obtained with the optimal weighting matrix. The whiskers depict the 95% confidence interval. From left to right, the bars are as follows: 'Baseline' (treatment age: 18–30, control age: 50+; treatment time: 00:00–05:59 Saturdays and Sundays, control time: 00:00–05:59 Mondays–Fridays); 'Control 40+' changes control group age to 40+; 'Control 60+' changes control group age to 60+; 'Treat 18–25' changes treatment age to 18–25; 'Treat 18–40' changes treatment age to 18–40; 'Time 00:00-04:59' changes treatment time to 00:00-04:59; 'Time 00:00-08:59' changes treatment time to 00:00-08:59; 'Wkend: Fri, Sat, Sun' changes the weekend definition to include Friday mornings; 'Wkend: Sat, Sun, Mon' changes the weekend definition to include Monday mornings; 'EWMD' refers to equally weighted minimum distance estimates; '6+ units, Baseline' shows estimates found using the 6+ unit definition of bingeing in the first stage with baseline control and treatment groups; 'Placebo' shows estimates found when Mondays, Tuesdays, and Wednesdays are defined as weekend (excluding Saturdays and Sundays) and treatment age group are changed to individuals aged 31–49.

Appendix Figure A2.B: Sensitivity Analysis for Road Accidents



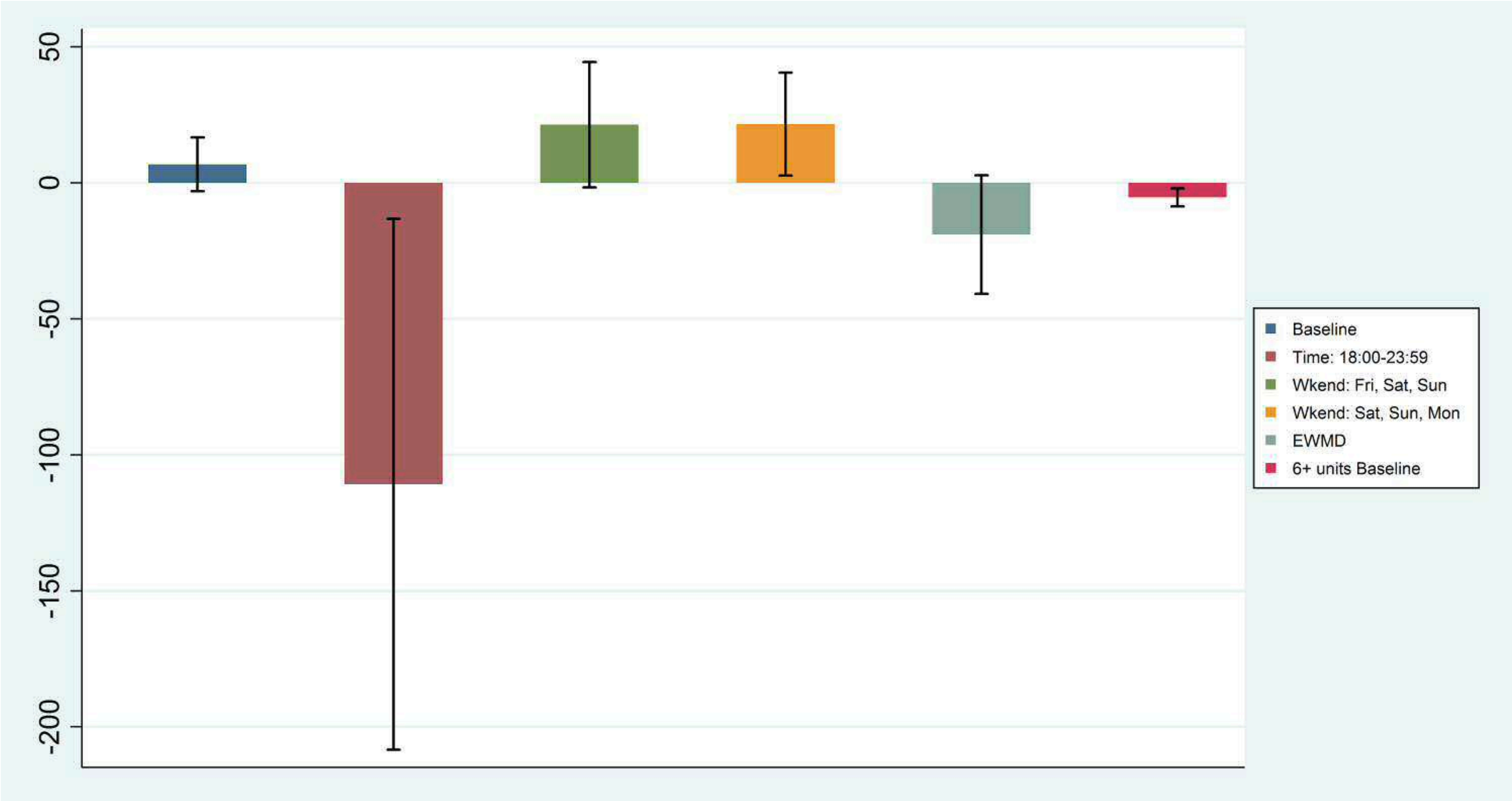
Note: See the note to Appendix Figure A2.

Appendix Figure A2.C: Sensitivity Analysis for Arrests



Note: Each bar represents the outcome specific TS-MDE effect obtained with the optimal weighting matrix. The whiskers depict the 95% confidence interval. From left to right, the bars are as follows: 'Baseline' (treatment age: 18–30, control age: 50+; treatment time: 00:00–05:59 Saturdays and Sundays, control time: 00:00–05:59 Mondays–Fridays); 'Control: 30–50' changes control group age to 30–50; 'Time: 18:00–23:59' changes treatment time to 18:00–23:59; 'Wkend: Fri, Sat, Sun' changes the weekend definition to include Friday mornings; 'Wkend: Sat, Sun, Mon' changes the weekend definition to include Monday mornings; 'EWMD' refers to equally weighted minimum distance estimates; '6+ units, Baseline' shows estimates found using the 6+ unit definition of bingeing in the first stage with baseline control and treatment groups.

Appendix Figure A2.D: Sensitivity Analysis for Number of Police Officers on Duty



Note: Each bar represents the outcome specific TS-MDE effect obtained with the optimal weighting matrix. The whiskers depict the 95% confidence interval. From left to right, the bars are as follows: 'Baseline' (treatment time: 00:00–05:59 Saturdays and Sundays, control time: 00:00–05:59 Mondays–Fridays); 'Time: 18:00–23:59' changes treatment time to 18:00–23:59; 'Wkend: Fri, Sat, Sun' changes the weekend definition to include Friday mornings; 'Wkend: Sat, Sun, Mon' changes the weekend definition to include Monday mornings; 'EWMD' refers to equally weighted minimum distance estimates; '6+ units, Baseline' shows estimates found using the 6+ unit definition of bingeing in the first stage with baseline control and treatment groups.

Appendix Table A1: Comparisons between Regional and National Statistics on Health Behaviors and Age Profiles

	(a) England	(b) Solihull (A&E)	(c) West Midlands (Arrests)	(d) London (Arrests & Police)	(e) Durham (Police)
A. Health Behaviors					
% Binge drinkers					
	17.8	17.2	17.9	12.6	25.4
p-value		0.08	0.34	0.00	0.00
N	6781	29	735	983	66
% Current smokers					
	24.1	21.8	24.1	23.4	28.1
p-value		0.26	0.83	0.00	0.00
N	6781	29	735	983	66
% Eat 5 or more fruit and veg. per day					
	26.4	28.8	25.0	29.5	20.6
p-value		0.064	0.00	0.00	0.00
N	6781	29	735	983	66
B. Age Profiles					
Age 15–29	19.8	17.3	19.7	23.7	18.7
p-value		0.00	0.71	0.00	0.26
N	6781	29	735	983	66
Age 20–29	13.4	10.4	13.0	17.8	12.2
p-value		0.00	0.06	0.00	0.09
N	6781	29	735	983	66
Age 50+	34.1	37.3	34.6	25.4	37.6
p-value		0.00	0.10	0.00	0.00
N	6781	29	735	983	66
Age 18–30	17.5	14.5	19.1	22.2	16.4

Note: Panel A: The data source is the model based estimates at the Middle Layer Super Output Area (MSOA) level. There are a total of 6,781 MSOAs in England, with an approximate average population of around 7,500 people in each MSOA. Binge drinking at the MSOA level is measured for adults aged 16+. Adult respondents to the Health Survey for England (HSE) are defined to be 'binge drinkers' if they report that in the last week they drunk 8+ units of alcohol (men), or 6+ units of alcohol (women) on any one day or more. They are not considered binge drinkers if they did not drink this amount of alcohol on any day in the past week. 'Current smokers' are defined from self-reports in the HSE; while respondents who are non-smokers report that they either 'never smoked cigarettes at all', or 'used to smoke cigarettes occasionally' or 'used to smoke cigarettes regularly'. 'Eat 5 or more fruit and veg. per day' indicates respondents to the HSE reporting that they consumed 5 or more portions of fruit and vegetables on the previous day. Panel B: The data source is the mid-2008 resident population estimates at the MSOA level for 'Age 15–29', 'Age 20–29', and 'Age 50+'. Notice that the mid-2008 resident population estimates at the MSOA level are available only in 5-year bands; therefore, the exact 'Age 18–30' is not available. For this figure, instead, the mid-2009 Primary Care Trust (Solihull) and specific police authorities (England, London, West Midlands, Durham) statistics are used.
p-value reports the p-value of a two tailed t-test of the MSOA mean of a region against the national average. N is the number of MSOAs.

Appendix Table A2: First Stage Estimates on Binge Drinking Found with the UK-TUS Data

	(1) 6pm - Midnight	(2) 8pm - 11pm	(3) 8pm - Midnight	(4) 8pm - 2am	(5) 9pm - 3am
A. A&E Attendances, Road Accidents, and Arrests					
α_1	0.003	0.002	0.003	0.002	0.003
	(0.003)	(0.004)	(0.004)	(0.003)	(0.002)
α_2	0.023	0.031	0.026	0.020	0.015
	(0.003)	(0.005)	(0.004)	(0.003)	(0.003)
α_3	0.051	0.074	0.069	0.053	0.044
	(0.005)	(0.008)	(0.007)	(0.005)	(0.004)
Observations	7,475	7,475	7,475	7,475	7,475
R^2	0.058	0.051	0.055	0.060	0.058
F-test	121.5	104.0	116.4	128.9	125.2
B. Police Officers on Duty					
α_1	0.003	0.002	0.003	0.002	0.003
	(0.003)	(0.004)	(0.004)	(0.003)	(0.002)
Observations	7,475	7,475	7,475	7,475	7,475
R^2	0.058	0.051	0.055	0.060	0.058
F-test	0.959	0.243	0.652	0.569	1.192

Note: Estimates are obtained from least squares regressions on the 2000/2001 UK Time Use Survey (UK-TUS) data. Standard errors are in parentheses. The unit of observation is individuals (by day). The dependent variable is the proportion of time an individual spends in a pub, restaurant, or cafe while *not* eating during the specified time period. There are 3,716 individuals with two observations and 43 with one observation.

Appendix Table A3: Differential Night Driving Patterns by Age

	(1) Midnight - 7am	(2) 6pm - Midnight	(3) 8pm - 11 pm	(4) 8pm - Midnight	(5) 8pm - 2am	(6) 9pm - 3am
A: Drivers						
α_1	0.223	2.154	1.140	1.480	1.678	1.136
	(0.180)	(0.576)	(0.317)	(0.346)	(0.369)	(0.282)
α_2	0.206	-0.602	-0.487	-0.246	0.044	0.343
	(0.160)	(0.512)	(0.282)	(0.308)	(0.328)	(0.250)
α_3	0.080	0.172	-0.057	-0.222	-0.208	-0.340
	(0.281)	(0.901)	(0.496)	(0.542)	(0.578)	(0.440)
Observations	7,475	7,475	7,475	7,475	7,475	7,475
R^2	0.029	0.042	0.023	0.025	0.026	0.019
F-test	1.937	7.603	7.580	8.388	8.961	6.719
B: Passengers						
α_1	0.418	1.512	0.552	0.860	0.930	0.790
	(0.155)	(0.590)	(0.346)	(0.377)	(0.394)	(0.280)
α_2	0.232	0.952	0.356	0.607	0.708	0.575
	(0.138)	(0.524)	(0.308)	(0.335)	(0.351)	(0.249)
α_3	0.206	0.175	0.187	0.317	0.743	0.518
	(0.243)	(0.922)	(0.542)	(0.590)	(0.617)	(0.438)
Observations	7,475	7,475	7,475	7,475	7,475	7,475
R^2	0.006	0.012	0.008	0.010	0.012	0.012
F-test	7.081	5.083	2.385	5.307	8.191	10.27

notes: Estimates are obtained from least squares regressions as in equation (3) using the 2000/2001 UK Time Use Survey (UK-TUS) data. Standard errors are in parentheses.

Appendix B: Benchmark Costing Procedure

In this appendix we illustrate our benchmark costing procedure using the figure of £4.863 billion given in Table 9. As explained in Section 7, this is determined using the TS-MDE estimates found on all individuals and taking 12+ alcoholic unit definition of binge drinking.

A&E Attendances — For injury attendances we have data from one care trust, Solihull Care Trust (SCT). Appendix Table A1 shows that the Solihull area compares well with national averages along health behaviors and age profiles. In order to bring SCT into line for a national estimate, we multiply the TS-MDE coefficient of 5.71 per day by 439.[33] The total annual A&E attendances calculated from these estimates then imply 390,628 (= 5.71 × 3 days × 52 weeks × the space scaling factor of 439) additional attendances. With a cost per attendance of £114 (Department of Health 2013), we obtain an estimated annual cost of binge drinking related to Accident and Emergency attendances of £44 million (as indicated in the first row of Table 9).

Road Accidents — The geographic scaling is not needed for this outcome since our daily TS-MDEs already have a national coverage. On the unit cost side, the UK Department of Transport publishes the estimated cost of different types of accidents based on a willingness to pay approach. A fatal accident is estimated to cost £2.07 million at 2014 prices,[34] while a serious accident costs £236,728, and an accident that is slight in nature is estimated to cost £24,897 (Department of Transport 2012). These costs take account of the loss of output as a result of injury, ambulance and hospital (but not A&E related) treatment costs, and the human costs of the casualty including the intrinsic loss of enjoyment of life for fatalities. They also take into consideration the damage to vehicles and property and administrative costs of accident insurance. We then use the TS-MDE estimates in Table 4 with 12+ units of alcohol found for the whole population and calculate annual incidences. This translates into 571 (=3.66×52 weeks×3 days) fatal accidents per year with a total cost of £1.18 billion (=571×£2.065 million). The annual burden of serious accidents is £0.625 billion and that of slight accidents is another £0.238 billion. Therefore, the total externality of the effect of binge drinking on road accidents amounts to £2.04 billion (second row, Table 9).

Arrests — Overall, crime contributes £2.75 billion to the cost of binge drinking. In order to scale up our estimates to the national level, we divide the number of total crimes committed at the national level, within a particular crime category, by the crimes committed in the West Midlands and the London area served by the MPS. These scaling factors then vary across crime types and range from 1.7 for robbery to 6 for criminal damage. The largest contribution to the arrest costs comes from violent crime which has a scaling factor of 3.96.[35]. The unit costs for each crime type are taken from the estimates produced by the UK Home Office (Dubourg and Hamed 2005; Home Office 2011),[36] and are inflation adjusted in 2014 prices. They range from just over £1,100 for criminal damage to £41,711 for sexual offences. We also apply the latest Home Office multiplier that takes into account unrecorded crimes by comparing police recorded crimes to the figures collected from the British Crime Survey (BCS)[37]. We then use the daily

[33] This factor comes from the fact that in 2010-2011 there were 48,740 total attendances in SCT out of a national total of 21,380,985.

[34] This is about 70% lower than the £6.6 million figure ($3 million in 1993 prices) used by Levitt and Porter (2001) and 38% lower than the £3.3 million figure ($1.64 million, 1997 prices) found by Ashenfelter and Greenstone (2004). See also the next subsection.

[35] In 2010/11 there were a total of 821,939 violent offences. Of these 41,499 in the West Midlands and 165,896 in the MPS area making a total of 207,395 for the two areas combined. This corresponds to 25.2% of the total number of offences, implying a scaling factor of 3.96.

[36] Such estimates take into account the costs associated with the anticipation of a crime, its consequences, and the response to it. Full details of the methodology are in Brand and Price (2000).

[37] For full details of the multipliers see: <http://tinyurl.com/cost-multipliers>

estimates of the effect of binge drinking on crimes given in Table 5 to calculate annual national costs. Violent offences make up a large contribution totalling £0.94 billion (=68.2 ×3 days×52 weeks× the scaling factor of 3.96×1.5 (Home Office multiplier)× the unit cost of £14,836).

Police Officers on Duty — Since the TS-MDE effects in Table 6 are not statistically significant, our cost calculations are determined using the smaller but more precisely estimated reduced form effects reported in Table 1. For the areas covered by the Durham Constabulary and the Metropolitan Police Service, we find that 3.24 additional officers per 10,000 individuals of the treated population are on duty during the night at the weekend as a result of binge drinking. The hourly cost per officer (more precisely, a police constable on a standard pay scale) is £15. Scaling the estimate up to the entire treated population of 18–30 year olds and taking account of both the hours on duty (i.e., 6) and the officer unit cost, we arrive at a total cost due to additional policing of £31.0 million (=3.24× 979 (national scale) ×6 hours per shift ×2 days of the weekend ×52 weeks×£15).[38]

Appendix C: Alternative Unit Costs

Here we provide details of our alternative unit costs (see Section 7). First, in the case of A&E attendances, there is no alternative source available. We therefore take four arbitrary (two lower and two upper) measures of costs, and examine increases and decreases of 10% and 25% in the £114 figure published by the UK Department of Health and used in Table 9. Unsurprisingly, given the small contribution of A&E costs to the total externality, these changes make little quantitative difference.

Second, in the case of road accidents, our alternative measures are the upper and lower estimates of the value of a statistical life given in Carthy et al. (1999, their Table 5). The upper value is £5.76 million (£10.8 million in 2014 prices) and the lower value £0.70 million (£1.3 million in 2014 prices). As mentioned in Appendix B, our benchmark value of £2.06 million is substantially more conservative than the estimates generally used in related studies (e.g., Levitt and Porter 2001; Ashenfelter and Greenstone 2004; Carpenter and Dobkin 2011). Likewise, the upper and lower values used here continue to be quite conservative and fall well within the range discussed in Viscusi and Aldy's (2003) meta-analysis which reports that most estimates for the value of a statistical life are between 7.1 and 16.8 million in 2014 British pounds (or 3.8 and 9 million in 2000 U.S. dollars, respectively). Taking these estimates as our base reference, we follow Carthy et al. (1999) and compute the externalities associated to a serious and a slight accident as being worth 11.5% and 1.2% of the externality associated to a fatal accident, respectively.[39] In sum, we calculate six alternative total externality measures, two (one upper and one lower) for fatal road accidents, two for serious accidents, and two for slight accidents.

Third, for arrests, we have a number of alternative measures. Using street level crime data, Braakmann (2012) examines compensating differentials in terms of property values and estimates the cost of a violent crime to be £65,603. We take this as an upper bound measure of the cost of violent crimes. Brand and Price (2000, their Tables A1.3, A1.7, A1.8, A1.10, A1.13) calculate high and low estimates of costs for a number of different crimes. In our baseline estimates for violent crime, we use their high measure of £29,063 (in 2014 prices), which is substantially lower than the Braakmann's estimate.[40] For sexual offences and robberies, we use both their high and low estimates, while for burglary, theft, and criminal damage we only use their lower estimates because their high estimates are smaller than the corresponding values used in our benchmark

[38] The national scale figure of 979 comes from the total 18–30 year olds in the MPS area and Durham police area (9,786,037) divided by 10,000.

[39] For instance, the upper unit cost for a serious accident is £659,997 (£1.24 million in 2014 prices) and that for a slight accident is £69,412 (£130,001 in 2014 prices).

[40] Using Brand and Price's (2000) lower cost estimate of violent crimes makes virtually no change to the benchmark externality assessment presented in Table 9. Thus it is not reported.

calculations. The lower estimate of violent crime is taken from the Home Office report (Home Office 2011). This source, however, does not report an estimate of the general cost for violence against the person as in Dubourg and Hamed (2005), which is used in our benchmark case. Therefore, to remain on the conservative side, we use the category of violence against the person that produces the lowest cost ("other wounding"). For common assault, Atkinson, Healy, and Mourato (2005, their Table 9) produce estimates using a stated preference approach with an upper value of £6,868 (in 2014 prices) and a lower value of £1,187 (in 2014 prices). In total, by changing the unit cost of crimes, we produce 13 alternative arrest cost estimates.

Finally, for the number of police officers on duty, we use two different measures of unit cost. For the lower measure, we replace the benchmark cost of £15 per hour with the cost of an officer just on appointment, which amounts to £11 per hour. For the upper measure, we use an officer with 10 years of service on double time, which implies £35 per hour. For this item we also provide an alternative cost measure based on the estimated effect of binge drinking rather than on an alternative unit cost. This comes from the TS-MDE effects reported in Table 6, which are generally greater than those obtained from the reduced form analysis used in the benchmark case. An additional lower bound is computed using the relevant TS-MDE coefficient minus one standard deviation. In total we have four alternative cost estimates for police numbers. As in the case of A&E attendances, these alternative costs lead to small changes in the estimated total externality.