Am I My Brother's Barkeeper? Sibling Spillovers in Alcohol Consumption at the Minimum Legal Drinking Age*

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Abstract

We use data on siblings near the minimum drinking age to provide causal estimates of peer effects in alcohol consumption, exploiting the increase in consumption of the older sibling in a regression discontinuity design. Preferred point estimates imply that younger sibling binge drinking *decreases* by 27% of the mean at the cutoff. While our results are somewhat imprecise, we are consistently able to rule out positive estimates from the literature. Our results are interpretable as the effect of peer alcohol consumption. Most prior work identifies the effect of exposure to the peer. We explain how this distinction matters for policy.

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1 Introduction

Excessive alcohol consumption has been shown to harm young adults in a variety of dimensions including health, educational performance, criminal activity, and criminal victimization (Carpenter and Dobkin, 2009; Carrell et al., 2011; Lindo et al., 2013; Carpenter and Dobkin, 2015, 2017; Chalfin et al., forthcoming). Among the many potential determinants of adolescent alcohol use, peer effects have received substantial attention from academics and policymakers.[1]

Leading empirical work provides strong evidence of positive peer effects in alcohol consumption via random assignment to college roommates (Duncan et al., 2005; Eisenberg et al., 2014; Guo et al., 2015). However, this design cannot tell us whether these spillovers are the result of peer drinking behavior or of some other correlated peer characteristic—they are "contextual" as opposed to "endogenous" peer effects, where the latter is an effect driven specifically by peer alcohol consumption (Manski, 2000). Distinguishing between these two types of causal[2] peer effects has key implications for policy. With endogenous peer effects, an intervention that influences the alcohol consumption of recipients also has an effect on the consumption of their untreated peers. This is not necessarily the case under contextual effects. Further, this design cannot be replicated in many other peer groups of interest, notably siblings—a peer group that has been the focus of a substantial and rapidly growing literature on a wide range of outcomes.[3]

This paper provides causally interpretable estimates of endogenous peer effects in alcohol consumption between siblings. We focus on siblings residing in the same household and exploit a discontinuous increase in older sibling alcohol consumption at the minimum legal drinking age (MLDA) using a regression discontinuity design (RDD). Our results are, to our knowledge, the first quasi-experimental estimates of peer effects in alcohol consumption between siblings. Further, our estimates are interpretable as the causal effect of sibling alcohol consumption. This extends the prior literature on peer effects in this behavior more generally, which primarily estimates the causal effect of exposure to heavier drinking roommates (who may have other unobserved characteristics that influence peer alcohol consumption).

Attempts to estimate causal peer effects face three main difficulties. First, the "reflection" problem implies that a simple regression of peer A's behavior on peer B's cannot determine the

[1] In its Underage Drinking Fact Sheet, the National Institute of Alcohol Abuse and Alcoholism lists peer pressure as one of three key causes of alcohol consumption among adolescents (https://pubs.niaaa.nih.gov/publications/UnderageDrinking/UnderageFact.htm).

[2] Note that despite the terminology both terms refer to causally interpretable peer effects. In this setting, a contextual peer effect is the causal effect being assigned a given roommate, while an endogenous effect is the causal effect of the roommate's drinking behavior.

[3] Including, for example, risky behaviors (Duncan et al., 2001; Fagan and Najman, 2005; Harris and López-Valcárcel, 2008; Altonji et al., 2016), health (Breining, 2014; Ho, 2017; Cawley et al., 2019; Daysal et al., 2019), fertility (Heissel, 2021), education (Goodman et al., 2015; Joensen and Nielsen, 2018; Karbownik and Özek, 2019; Altmejd et al., 2021), and military service (Bingley et al., 2021).

direction of any observed effect (Manski, 1993). Second, peer groups are (typically) endogenous, which is a concern if individuals choose peers who have similar preferences related to the behavior of interest. Finally, individuals who share a peer group are likely to experience unobservable common shocks which are correlated with their outcomes.

Our setting and identification strategy address each of these difficulties. By focusing on siblings, a peer group which is naturally occurring, we avoid the potential for selection into the peer group. By restricting the sample to siblings with different ages, we ensure that the variation in alcohol consumption that we utilize is both exogenous (avoiding the problem of common shocks) and specific to one of the two siblings (avoiding the reflection problem).

To estimate spillover effects in alcohol consumption we utilize data from the 1997 National Longitudinal Survey of Youth (NLSY97), which has two key features for our analyses. First, the data contains unique "household roster" information on all individuals living with the NLSY97 respondent. These rosters include the birth months of each household member and their relationship to the respondent, which we use to construct our running variable. Second, it contains several dimensions of alcohol consumption for survey respondents that are measured at relatively high frequency (consumption in the past month).

In the full sample of 4,278 sibling-pair-years, our estimates imply a *negative* relationship between the older sibling's legal access to alcohol and the younger sibling's alcohol consumption.[4] The preferred specification implies that the number of binge drinking days (5 or more drinks) in the past 30 days reported by younger siblings decreases by 0.34 days at the cutoff. This is a substantial effect given that the average younger sibling in the sample reports 1.14 binge drinking days in the past month. While this estimate is statistically significant (95% CI of [-0.650, -0.025]), results from similar specifications which differ in terms of outcomes or control variables are often less precise. However, we do consistently find a moderate negative effect across a wide range of specifications. Further, our preferred estimate is well into the left tail of a distribution of placebo discontinuities estimated at older sibling ages that are close, but not equal to, 21 years.

Our estimates are almost always precise enough to rule out the relatively large positive effects reported in prior work. Notably, we compare our estimates to those from Eisenberg et al. (2014) (henceforth EGW) since they utilize what is arguably the gold-standard research design (randomized college roommates) on this question, and their estimates are smaller than

[4]In most cases we only observe alcohol consumption for one sibling (the sibling that is a NLSY97 respondent). However, we estimate the discontinuity in older sibling alcohol consumption in a separate sample of NLSY97 respondents who have younger (usually not NLSY-respondent) siblings. Results are similar to the existing literature utilizing this research design, implying increases in consumption of 20-30%.

nearly all other estimates in the related literature.[5] Among 64 total specifications which can be compared to EGW, 59 confidence intervals (95%) exclude the EGW point estimate.[6]

We view our results as suggestive evidence that the causal relationship in alcohol consumption between siblings in this age range is negative. This is admittedly counterintuitive. However, a large literature has established that a series of negative outcomes spike at the MLDA (e.g., Carpenter and Dobkin, 2017, 2015; Lindo et al., 2013; Carrell et al., 2011; Carpenter and Dobkin, 2009) and it is plausible that younger siblings update their beliefs about the costs of alcohol consumption after observing their older siblings experience these negative consequences.[7] Unfortunately, direct tests for this mechanism are not possible due to data constraints, but we do demonstrate that our estimates vary in several key dimensions in ways that are consistent with a true negative effect.

First, the negative spillover effects are concentrated among sibling pairs that have higher socio-economic status, that report non-Black and non-Hispanic race/ethnicity, and where the older sibling is a male. We show that these are the same subgroups where the "first-stage" discontinuity in older sibling consumption is largest.[8] Second, negative effects are also concentrated among sibling pairs who are the same gender. It is reasonable to assume that these sibling pairs would be more exposed to each other's behavior, and this assumption is supported in the time-use literature (Dunifon et al., 2017). Our estimates are also especially negative for measures of excessive consumption (binge drinking). Prior work suggests that negative effects of alcohol consumption experienced at the MLDA are driven by these types of consumption (Carpenter et al., 2016), and it is reasonable to expect that any spillover effects would be most apparent on this margin as well. Finally, we also estimate spillover effects on older siblings when a younger sibling turns 21. This can be seen as a falsification test for the proposed mechanism since an older sibling is less likely to learn from a younger sibling's alcohol consumption. Point estimates in this sample are close to zero and much more likely to be positive.

Three specific concerns which may threaten the interpretation of our results are worth highlighting. First, an older sibling's legal access may directly affect the access of the younger sibling (e.g., if the older sibling purchases alcohol for the younger). However, this effect would bias

[5]Notably Gaviria and Raphael (2001), Duncan et al. (2005), Lundborg (2006), Fletcher (2012), Guo et al. (2015), and Altonji et al. (2016) all report similar or larger peer effects of alcohol consumption in their preferred specifications.

[6]EGW's key comparable result is that being assigned a roommate who binge drank in 30 days before move-in leads to a 19% increase in the probability of any binge drinking by the respondent in a second 30-day period roughly 8 months later.

[7]Some specific negative outcomes that have been studied in the literature are likely too rare to explain our spillover effects. However, despite the lack of quantitative evidence it is likely that other less-severe and more common negative outcomes also spike at age 21.

[8]These "first-stage" results rely on a separate sample of NLSY97 respondents who have younger (usually not NLSY-respondent) siblings.

our negative estimates toward zero. Second, parents may compensate for an expected spillover effect with a change in parenting style when the older sibling turns 21 (e.g., by monitoring younger siblings more closely), which would likely negatively bias our results. The NLSY97 includes information on parenting styles which allows us to provide evidence against this concern. Third, it is possible that younger siblings in our sample are not sufficiently aware of the alcohol consumption of their older siblings (e.g., if they do not spend much time together). To further support our focus on this peer group we use the American Time Use Survey (ATUS) to demonstrate that siblings in the relevant age range who live together spend substantial amounts of time together.

We contribute to three separate literatures. First, we add to a limited number of studies which provide quasi-experimental estimates of endogenous peer effects in alcohol consumption. The most similar work is Fletcher and Marksteiner (2017), who estimate endogenous peer effects in alcohol consumption with a different identification strategy (randomized assignment to an alcohol cessation program) in a very different population (adult spouses in which one spouse is a heavy drinker). Second, we provide what we believe to be the first quasi-experimental estimates of spillovers in alcohol consumption between siblings. The existing economics literature on this topic has focused on other outcomes (e.g., smoking as in Harris and López-Valcárcel (2008)) or has relied on structural assumptions (Altonji et al., 2016), while a substantial literature outside of economics has documented the strong correlation in consumption between siblings (e.g., Duncan et al., 2001; Fagan and Najman, 2005; Trim et al., 2006; Van Der Vorst et al., 2007; Whiteman et al., 2013). Third, we contribute to a growing literature on sibling spillovers more generally, where a growing body of evidence has demonstrated the importance of sibling influences in a variety of important domains including health outcomes (Breining, 2014; Ho, 2017; Cawley et al., 2019; Daysal et al., 2019), fertility (Heissel, 2021), education (Goodman et al., 2015; Joensen and Nielsen, 2018; Karbownik and Özek, 2019; Altmejd et al., 2021), and military service (Bingley et al., 2021).

2 Data

The NLSY97 is a longitudinal survey of 8,984 American youths who were between the ages of 12 and 17 at the time of the first wave of the survey in 1997 (Bureau of Labor Statistics, 2019).[9] Designed and administered by the Bureau of Labor Statistics, the NLSY97 includes a wide variety of questions related to family processes, education, employment, health, and family formation, among other topics.

[9]Waves are annual from 1997-2011, and biennial beginning in 2013.

The NLSY97 has two key features which make it uniquely suitable for our analysis. First, the survey includes detailed information on the composition of each respondent's household, including the relationship of each household member to the respondent, and the age of each household member (including the respondent) in months.[10] Second, information on the alcohol consumption of respondents is obtained in all waves. Specifically, we use the following outcomes, all reported for the past 30 days prior to the survey: the number of days on which any drinks were consumed (drinking days), the number of days on which 5+ drinks were consumed (binge days), and the binary versions (any/none) of drinking days and binge days.[11]

Each wave of the NLSY97 also includes a detailed set of covariates at the respondent and household levels which are also potentially related to alcohol consumption and therefore will serve as control variables in our analysis. These include the race and gender of the respondent and siblings, educational attainment and enrollment of respondents, geography (urban/rural, census region), household size, AFQT score of the respondent, an indicator for whether or not the respondent has children, and an indicator for whether the respondent worked in the past year.[12]

For reasons that we will describe in the next section, we will primarily focus on 2,614 NLSY respondents who have only one older sibling in their household, and where that older sibling is between the ages of 19 years & 0 months (228 months of age) and 23 years & 0 months old (276 months of age). Alternative samples drop either the requirement that the siblings *currently* live together,[13] the requirement that the two siblings are the oldest two siblings, or both. Key subgroups of interest are defined based on parental educational attainment, family income, race/ethnicity (recorded in the data as "Black", "Hispanic", or "Non-Black, Non-Hispanic"), whether the siblings report the same gender, the gender of the older sibling, the age difference between the siblings, and the younger sibling's previous (as of the prior survey wave) drinking experience. Summary statistics are shown in Table 1.

The NLSY97 sampling process first identified a large number of households and then iden-

[10]Specifically, the public use version of the NLSY97 includes the exact date of the interview along with the month of birth for the respondent and all members of the respondent's household. This allows us to estimate the age in months of the respondent and all siblings who ever co-resided with the respondent during the survey, regardless of whether those older siblings are themselves NLSY97 respondents. The NLSY97 also collects a very limited set of information on close relatives such as siblings who live outside of the respondent's household. This information is unfortunately too limited to make use of these siblings in our analyses.

[11]Additional measures of alcohol consumption used to define subgroups, but not used as outcomes, include whether or not the respondent had ever consumed alcohol as of the previous year and whether or not the respondent had ever binge consumed alcohol as of the previous year.

[12]This is not an exhaustive list of potentially relevant control variables included in the NLSY97. A large number of relevant questions in the NLSY97 are either asked too intermittently or have too many missing values to be useful in our analysis.

[13]They must have lived together during at least one NLSY97 wave in order for us to observe the sibling and the sibling's age in months.

tified all individuals in each household who were age-eligible for the survey. Because of this, a sample of siblings who are all NSLY97 respondents is available. While this sample will not be used in our main analysis, we do use it to provide descriptive evidence of between sibling correlation in alcohol consumption in the NLSY97. Table A1 shows results from a regression of the younger sibling's past month alcohol consumption on the older sibling's past month alcohol consumption for multiple measures of consumption in a sample of sibling pairs that are both NLSY97 respondents.[14] These results are not causally interpretable and are not directly comparable to our sample of interest. However, they show that in this sample, much like in prior literature described previously, there is a very strong correlation in alcohol consumption between siblings.

Among a set of measures of past month alcohol consumption in the NLSY97, we focus on measures of the excessive alcohol consumption in our main analyses: The number of binge drinking days (5+ drinks) in the past month, and an indicator for any binge drinking days in the past month. This helps make our results more comparable to the existing literature, which typically focuses on measures of binge drinking, and more policy relevant, since excessive drinking is more likely to result in the various negative outcomes that are associated with alcohol consumption. The focus on a small subset of the available outcomes also helps to reduce problems of multiple testing. However, results for all outcomes are presented for certain analyses in order to demonstrate robustness.

3 Methods

We implement a reduced form regression discontinuity design in which the age of the older sibling is the running variable and the effect of the older sibling turning 21 (gaining legal access to alcohol) acts as an instrument for the older sibling's alcohol consumption. The MLDA was first used as an exogenous source of variation in alcohol consumption in a RDD by Carpenter and Dobkin (2009) to study the effect of alcohol consumption on mortality and has since been used to study a wide range of other outcomes.[15] So long as no other factors related to the outcome are changing discontinuously at the cutoff, this approach will provide causally interpretable results. Many of the aforementioned studies have provided convincing evidence of both a strong first stage (large, discontinuous increases in consumption at age 21) and of the credibility of

[14]Households with 3 or more sibling NLSY97 respondents are excluded for simplicity and the sample is limited to individuals under the age of 23.

[15]Including criminal activity (Carpenter and Dobkin, 2015; Hansen and Waddell, 2016), crime victimization (Chalfin et al., forthcoming), morbidity (Carpenter and Dobkin, 2017), marijuana consumption (Yörük and Yörük, 2011; Crost and Guerrero, 2012; Crost and Rees, 2013; Yörük and Yörük, 2013), the consumption of other illegal drugs (Deza, 2015), risky sexual behavior (Yörük and Yörük, 2015), and mental health (Yörük and Yörük, 2012).

this research design (e.g., establishing that observable covariates do not change discontinuously at the cutoff).

Many NLSY97 respondents have more than one older sibling. As pointed out by Dahl et al. (2014), this complicates estimation of peer effects with a RDD since it is unclear how to define the running variable. To avoid these complications, we define the peer group as siblings who currently reside in the same household and consider the effect of the oldest sibling in the peer group on the second oldest sibling. This ensures that we have a large sample of sibling pairs in which we observe the information necessary to implement the RDD (older sibling age in months and younger sibling alcohol consumption) and allows us to avoid the complications involved with defining the running variable in peer groups with more than two members.

The reduced form RD is then implemented by estimating the following equation via OLS:

$$alc_{2,h,t} = \gamma_1 1\{age_{1,h,t} \geq 21\} + f(age_{1,h,t}) + X'_{2,h,t}\gamma_2 + X'_{1,h,t}\gamma_3 + W'_{h,t}\gamma_4 + \theta_{2,h} + \mu_{2,h,t} \quad (1)$$

where subscripts denote sibling (2 = younger, 1 = older), household (h), and the survey wave (t), the outcome is some measure of the younger sibling's past month alcohol consumption, the reduced form effect of older sibling's legal access to alcohol on the younger sibling's consumption is estimated by $\widehat{\gamma_1}$, $f(age_{1,h,t})$ is a flexible polynomial in the running variable (e.g., older sibling age fully interacted with the cutoff dummy $1\{age_{1,h,t} \geq 21\}$), $X_{2,h,t}$ is a vector of younger sibling covariates, $X_{1,h,t}$ is a vector of older sibling covariates, $W_{h,t}$ is a vector of household h level covariates, and $\theta_{2,h}$ is a younger sibling fixed effect.

We separately estimate the increase in older sibling drinking at age 21 in a similar equation (where the outcome is the alcohol consumption of the older sibling, $alc_{1,h,t}$, and younger sibling characteristics are removed from the regression). The sibling pairs used in that analysis are not the same as those used to estimate the reduced form effect.[16]

To construct the running variable, we use the birth month of the older sibling and the month of the relevant interview (younger sibling's interview in the reduced form, older in the first stage). This implies that our running variable is rounded *up*, and that the cutoff indicator will be mismeasured for some sibling pairs in which the older sibling is exactly 21 years (252 months) old. Following the recommendations in Dong (2015), we address this misclassification bias with a "donut RD" specification that excludes sibling pairs in which the older sibling is exactly 21 years (252 months) old. Following the existing literature on MLDA-based RDDs,

[16]Sibling pairs in the reduced form sample are those in which the second oldest sibling the household is a NLSY97 respondent. Sibling pairs in the first stage are those in which the oldest sibling is a NLSY97 respondent. Although a two-sample two-stage least squares is theoretically possible, sample sizes are likely to be too small for it to be informative.

the sample is limited to sibling pairs in which the older sibling is between the ages of 19 and 23—i.e., the youngest older sibling in our sample is 19 years and 0 months old (228 months of age) and the oldest is 23 years and 0 months old (276 months of age). Standard errors are cluster robust at the younger sibling level to account for correlation in the error term among observations from the same individual.

Additional models test the sensitivity of our results in several of the dimensions mentioned above (bandwidth, correction for rounding-induced bias, inclusion of covariates, inclusion of fixed effects, and order of the running variable polynomial). Notably, we implement the continuity-based RD framework developed by Cattaneo et al. (2019), including mean-squared error optimal bandwidth selection and bias-corrected robust confidence intervals.

The main assumption required for a causal interpretation of $\widehat{\gamma_1}$ is that no unobserved confounding factors change discontinuously at the cutoff. We "test" this assumption to the extent possible by estimating models similar to equation 1, where outcomes are predicted values from separate regressions which predict the alcohol consumption measures using a range of covariates.

A second required assumption in RDDs more generally is that the running variable is not manipulated. Although this is not technically a concern in our setting (since age is not manipulable), a related problem can arise if rates of nonresponse to alcohol consumption questions change discontinuously at the cutoff. This is primarily a concern in the first stage where an older sibling may be more willing to report alcohol consumption once they reach age 21. We test this assumption by demonstrating visually that the density of older sibling age is smooth through the cutoff and by testing formally for a discontinuity in the density at the cutoff.

Finally, to interpret $\widehat{\gamma_1}$ as the causal effect *of older sibling alcohol consumption* (as opposed to older sibling legal access), we require an exclusion restriction: the older sibling's legal access must influence younger sibling alcohol consumption *only* through an increase in the older sibling's consumption. We discuss and provide suggestive evidence in favor of this assumption in section 5.

4 Main Results

4.1 Design

We begin by demonstrating that older sibling drinking behavior changes discontinuously at age 21. Results using count and binary measures of drinking days and binge days in the past month are shown in Table 2. Similar to results from previous work using the same identification strategy in different samples,[17] there is a large discontinuous increase in alcohol consumption at age 21. This effect is apparent in all models shown in Table 2. In preferred models (donut specification with a linear function of the running variable, controls,[18] and individual level fixed effects), past month binge drinking and drinking days increase by 0.45 days and 1.45 days at the cutoff, respectively. Both increases are statistically significant at the 5% level. Increases on the extensive margin of consumption for binge drinking and drinking (from the same models) are 5.7pp and at 8.3pp respectively, which are also both statistically significant at the 5% level. Figure 1 plots mean values of our four outcomes among older siblings in each month-of-age bin along with fitted lines and regression coefficients from the corresponding first stage regressions.

Figure A1 graphs the distribution of the older sibling's age in months, for the first stage sample. While age is not manipulable, it is possible for nonresponse to change discontinuously at the cutoff and such a response could potentially affect the interpretation of the results presented in Table 2. Figure A1 suggests that this is not occurring, given that the distribution is relatively smooth through the cutoff at 21 years of age.

In Table A2 and Figure 2 we present results which test for discontinuities in sibling pair characteristics at the cutoff that could potentially confound the causal effect of interest. We use a set of covariates[19] to predict each of our four (younger sibling) alcohol consumption outcomes and then use a regression analogous to our reduced form specification to test for a discontinuity in each of those predictions at the cutoff (standard errors are calculated via bootstrap). All discontinuities are statistically indistinguishable from zero.

4.2 Reduced form

Table 3 shows results from equation 1, estimated in the full sample of sibling pairs who reside in the same household and are the oldest siblings in the household, for all four measures of alcohol consumption. Focusing on the preferred model (the first column) which includes the previously defined vector of controls and individual level fixed effects, all point estimates are negative and large (e.g., younger sibling past month binge days decrease by 0.337 day or 26.9% of the mean just before the cutoff). Assuming that no unobserved confounder changes discontinuously when the older sibling turns 21, this implies that the older siblings' increase in alcohol consumption

[17] See the previous section for citations.

[18] Month and year of the survey, the race and gender of the respondent and siblings, the age of the respondent, educational attainment and enrollment of respondents, geography (urban/rural, census region), household size, AFQT score of the respondent, an indicator for whether or not the respondent has children, and an indicator for whether the respondent worked in the past year.

[19] Specifically: age, gender of both siblings, educational enrollment, highest completed education, work status, indicators for whether the household lives in an urban area, census region dummies, AFQT score, household size, and interview month/year. (All variables refer to the younger sibling's information unless otherwise noted.)

at the cutoff has a causal negative effect on the alcohol consumption of younger siblings on this margin.

Point estimates are statistically significant for only one of the four considered outcomes in our preferred specification, suggesting that these estimates should be interpreted with caution. However, the outcome for which our results are strongest (count of binge drinking days) is also arguably the most important—as it represents a particularly risky and problematic type of consumption. Further, point estimates are negative and at least modestly sized for all outcomes in the preferred specification, and nearly all estimates in Table 3 as a whole. As a percentage of the mean of the outcome just before the cutoff, coefficients in the preferred specification imply that binge drinking decreases by 9.2% at the extensive margin, while drinking (any amount) decreases by 5.9% at the intensive margin and 6.7% at the extensive margin.

Corresponding binned scatter plots for all four outcomes are shown in Figure 3. These figures include coefficients from a regression of each outcome variable on the age of the older sibling (in months, centered at 21 years old), an indicator for older sibling age ≥ 21, and their interaction (i.e., they correspond to a specification without controls or fixed effects). The figures show that there is no significant change in the alcohol consumption of younger sibling when their older sibling turns to 21, suggesting (as also shown in Table 3) that the size and precision of our negative estimates are somewhat sensitive to specification choices.

To further strengthen our results, we perform a series of placebo tests estimating discontinuities at older sibling age values near but not equal to 21. Specifically, for each monthly age bin up to 12 months on either side of the cutoff, we re-estimate equation 1, using the preferred donut-RD specification with a linear polynomial of the running variable, controls, and fixed effects. Results are plotted in Figure 4, with the estimate at the actual cutoff value represented by a blue vertical line. While this test cannot be used to conduct formal inference, the results are reassuring—the true estimate is in the far left tail of both placebo distributions.

4.3 Comparison to prior literature

While we believe that the negative effects we estimate are plausible, our sample sizes are relatively small and the estimates are not always statistically significantly different from zero. However, given the uniformly positive effects estimated in the related literature (e.g., Gaviria and Raphael, 2001; Duncan et al., 2005; Clark and Lohéac, 2007; Lundborg, 2006; Fletcher, 2012; Eisenberg et al., 2014; Guo et al., 2015; Altonji et al., 2016; Fletcher and Marksteiner, 2017) an our interpretation of our estimates as null results is interesting in its own right.

To emphasize this, we compare our results to similar estimates from Eisenberg et al. (2014)

(EGW). We choose this study for two reasons. First, their estimates are smaller than nearly all other estimates in the related literature. Therefore, if our results are sufficiently precise to rule out the EGW point estimate, the same is true for a series of other leading estimates in the literature.[20] Second, EGW utilize what we see as the best existing identification strategy on this question: randomized assignment to college roommates.

EGW's key comparable result is that being assigned a roommate who binge drank in the 30 days prior to a baseline survey fielded in August of 2009 (just before the students moved in to their shared rooms) leads to a 19% increase in the probability of any binge drinking by the respondent in a second 30-day period roughly 8 months later. In Figure 5 we scale our preferred estimate for the same outcome by the control (immediately pre-cutoff) mean, obtaining a 95% CI of [-27.6%,9.2%] in our preferred specification and ruling out the EGW point estimate (red line) in all four specifications.

Since we estimate the direct effect of the peer's alcohol consumption, while EGW estimate the effect of exposure to a peer who binge drank in the past, we do not view these results as contradictory. Instead, if college roommates and co-resident older siblings are similar peer influences, these estimates would imply that EGW's results are explained at least in part by roommate characteristics other than drinking behavior. As mentioned previously, this is critically important for policy. Only endogenous peer effects—those which result directly from the peer's alcohol consumption—indicate that the costs of alcohol consumption are socially multiplied.

Although the EGW treatment (being assigned a roommate who did not binge drank vs. one who did) may seem larger than the effects of the MLDA on older-sibling alcohol consumption, timing implies that the difference is small. The EGW treatment is a 100% increase in exposure to peer binge drinking measured roughly 8 months in the past, while we measure a contemporaneous spillover effect. Especially given the trajectory of alcohol consumption in this age range (as shown by, e.g., the slope of the fitted line on the left-hand side of panel (c) in Figure 1), it is likely that many of the "control" peers in EGW (no prior binge drinking) would have binge drank by the time the outcomes were measured 8 months later. Therefore, the contemporaneous treatment vs. control difference in peer binge drinking in the EGW sample is likely much smaller than 100%.

[20] Notably Gaviria and Raphael (2001), Duncan et al. (2005), Lundborg (2006), Fletcher (2012), Guo et al. (2015), and Altonji et al. (2016) all report similar or larger peer effects of alcohol consumption in their preferred specifications.

5 Interpretation

Why might an increase in older sibling alcohol consumption at the MLDA have *negative* effects on younger sibling consumption? In this section we first discuss several potential mechanisms for this result and, alternatively, several possible exclusion restriction violations. We then present a variety of suggestive evidence which we believe supports an interpretation of our results as the causal effect of sibling alcohol consumption.

5.1 Potential mechanisms and alternative interpretations

In an influential review of the economic literature on social interactions, Manski (2000) suggests that economic peer effects can operate through one of three channels: constraints, preferences, or expectations. Here we use this taxonomy to organize a discussion of potential mechanisms for the sibling spillover effects presented in section 4.2.

In our context, there are two clear constraints on a younger sibling's alcohol consumption that could be influenced by their older sibling's legal drinking status. First, there is the potential for an "access" effect. An older sibling's ability to legally purchase alcohol could ease the access constraint faced by the younger sibling even if the older sibling's consumption remains unchanged. We cannot test for this effect in our data, since we do not observe how (or how easily) NLSY97 respondents obtain alcohol. However, we note that an access effect would bias our negative reduced form estimates toward zero. Second, when an older sibling's alcohol consumption (or access to alcohol) increases, parents may respond by monitoring the younger sibling more closely. We can test for such responses in the NLSY97 data, and results presented in appendix A suggest that this does not occur.

Preferences act as a mechanism for peer effects when the utility that one person derives from a behavior depends on the behavior of their peer. Preferences are commonly hypothesized as mechanisms for positive peer effects in alcohol consumption—drinking together is more fun. It is more difficult to see how such a story could explain a *negative* peer effect in alcohol consumption.

Expectations are perhaps the most promising class of mechanisms in our context. A peer effect operates through expectations when there is uncertainty about the costs or benefits of some behavior, and peers can reduce that uncertainty by observing each other's experiences. Alcohol consumption is a costly behavior, and this is especially true for the types of excessive consumption that respond most strongly to the MLDA (Carpenter et al., 2016). Indeed, we know that several negative outcomes spike at the MLDA. It is reasonable to expect that adolescents are uncertain about these costs, and that they would learn about them by observing role models like older siblings.

Many of the specific negative outcomes that have been studied in the MLDA literature (e.g., mortality (Carpenter and Dobkin, 2009), hospital admissions (Carpenter and Dobkin, 2017). criminal behavior (Carpenter and Dobkin, 2015), etc.) are too rare to plausibly explain the negative spillover effects that we estimate. However, we argue that other less severe and more common negative outcomes also likely spike at age 21. Notably, prior work provides evidence that academic performance decreases at the MLDA (Carpenter and Dobkin, 2015; Lindo et al., 2013), and this is an example of a less severe and likely more commonplace effect of excessive alcohol consumption that could drive our results. To name a few other hypothetical examples, older siblings whose risky drinking behavior increases at age 21 might get in more trouble at home (or in work or school), get less sleep, or spend lots of time lying on the couch nursing a hangover.

A second important caveat to our proposed expectations mechanism is that it relies heavily on the assumption that the younger sibling is able to observe their older sibling's behavior and outcomes. We provide two pieces of suggestive evidence for this. First, our main results focus on siblings who reside in the same household and, as discussed further in section 6, our reduced form estimates attenuate when we relax this restriction. Second, in appendix B we outline a descriptive analysis which uses data from the ATUS to demonstrate that sibling pairs similar to those in our NLSY97 sample spend substantial amounts of time together.

Unfortunately, data on adolescent expectations about the *relevant* consequences of alcohol consumption are hard to come by. The NLSY97 and some other surveys do contain survey questions on more severe and long-term risks of alcohol consumption, such as liver disease, heart disease, and alcoholism. However, it is unlikely that a younger sibling would update their beliefs about those risks based on their older sibling's *contemporaneous* experiences.[21] Instead, empirically testing the expectations mechanism in our context would require survey questions on expectations about smaller and more immediate costs of alcohol consumption. To our knowledge, these do not exist in a data set with sample size and information (sibling age in months) sufficient for our purposes.

In lieu of direct evidence for our proposed mechanism, we turn in the next subsection to an analysis of heterogeneity in sibling spillover effects at the MLDA.

[21] Even if such risk perceptions were of interest, the NLSY97 only asks these questions in a small number of survey waves (restricting our sample size), and other potential data sources have similar issues. Notably, the National Health Interview Survey (NHIS) has few respondents under age 21 and the National Longitudinal Study of Adolescent Health (Add Health) lacks the detailed household information necessary to construct our running variable.

5.2 Heterogeneity

Prior work on the MLDA has uncovered consistent patterns of heterogeneity, both in the first stage and for various negative consequences of excessive alcohol consumption. Specifically, the increase in (own) alcohol consumption at age 21 has been shown to be driven by excessive consumption (binge drinking) among males (Carpenter et al., 2016). Increases in mortality (Carpenter and Dobkin, 2009), and to a lesser extent morbidity (Carpenter and Dobkin, 2017) are also concentrated among males, while the mortality effects are further concentrated among whites (Carpenter and Dobkin, 2009). Similarly heterogeneous sibling spillover effects in our setting would provide support for our exclusion restriction. If our results represent true causal effects of older sibling alcohol consumption we would expect our negative reduced form estimates to be concentrated in the subgroups and outcomes where the first stage is strongest.

We begin in Table 4 by re-estimating our first stage (older sibling consumption) results from section 4.1 in subgroups defined by parental education (all parents completed high school vs. one or more did not complete high school), household income ($<$ vs. $\geq$ median of \$53,515[22]), employment status, gender, and race (recorded in the NLSY97 as Black vs. Hispanic vs. non-Black and non-Hispanic). Similar to prior work, we find that the increase in alcohol consumption at the MLDA is driven by males. We additionally find that the increase is driven by families with high socioeconomic status (high parental education and household income), by non-Black and non-Hispanic older siblings, and by older siblings who are not employed.

Next, in Table 5 we repeat the same exercise for our reduced form results. Alignment with the first stage heterogeneity from Table 4 is striking. Younger siblings that are non-Black and non-Hispanic, have high-SES, or have male older siblings all exhibit larger and more precisely estimated reductions in alcohol consumption when their older sibling turns 21. The only subgroup result with an analogue in Table 4 where the first stage and reduced form results do not align is older sibling employment status.[23] As previously discussed in section 4.2, we also note that the negative reduced form effects are strongest for measures of excessive alcohol consumption where the first stage effect has been concentrated in the prior literature. We believe that these patterns support an interpretation of our reduced form estimates as the causal effect of older sibling alcohol consumption. It is unlikely that a spurious result or some violation of the exclusion restriction would show up in exactly the subgroups and margins of alcohol

[22]Note that this median is calculated from the reduced form sample. The goal of this section is to determine whether the first stage effects are driven by the same subgroups as the reduced form effects, so we want to define the subgroups similarly in both samples. Since the reduced form sample is smaller, we prioritize balancing the low and high income subgroup sizes in that sample.

[23]The first stage is much stronger among non-employed older siblings, but the reduced form effect is slightly stronger among sibling pairs where the older sibling is employed.

consumption where the first stage result is strongest.

Table 5 also includes three additional sets of subgroups not present in Table 4: sibling gender match, sibling age difference, and the prior drinking experience of the younger sibling (measured in the previous survey year). These subgroups are of interest because it is natural to expect that an expectations mechanism would be more prominent when siblings share the same gender (e.g., if such siblings spend more time together, which has been shown by, e.g., Dunifon et al. (2017)), are closer in age (for the same reason), and when the younger sibling has less experience with alcohol (since such younger siblings would likely have more uncertainty about the costs of alcohol consumption).

We find that negative spillover effects are indeed concentrated among same-gender sibling pairs. Patterns for subgroups defined by prior drinking experience, and age difference, are less clear. One possible explanation for this is that these characteristics may be strongly correlated with each other, or with other key factors which predict alcohol consumption. If the age difference between siblings is large, then the younger sibling is likely to have less prior alcohol consumption simply because they are younger. This complicates the interpretation of these subgroup estimates.

5.3 What happens when a younger sibling turns 21?

The NLSY97 data also allows us estimate spillover effects on older siblings when a younger sibling turns 21. This is useful because the expectations mechanism we have proposed is less relevant when a younger sibling turns 21. Older siblings likely have much less uncertainty about the consequences of alcohol consumption by the time their younger siblings turn 21, and younger siblings are less likely to serve as a role model. For these reasons, this exercise can be seen as a falsification test for the proposed mechanism.

Table A3 implements this falsification test using several different specifications and outcomes. Since it is less common for sibling pairs in this age range to live together (here the younger sibling is between the ages of 19 and 23), sample sizes under our typical sample definitions are relatively small in this analysis. For this reason we also present these results with alternative samples that remove either the requirement that the siblings currently live together, the requirement that the siblings are the oldest two siblings in the household, or both.

Point estimates are nearly always positive. Although the estimates are somewhat imprecise, we view this as additional suggestive evidence in support of our main results.

6 Robustness

Our main analyses focus on sibling pairs who currently reside in the same household, are the oldest two siblings in that household, and where the older sibling is between the ages of 19 and 23. In this section, we explore the robustness of our results to different sample selection criteria, and to various specification choices such as the inclusion of controls or FEs.

6.1 Alternative samples

Table 6 presents results that remove either the requirement that the siblings currently live together, the requirement that the siblings are the oldest two siblings in the household, or both. The first panel corresponds to Table 3, which uses our main sample. Point estimates are broadly consistent (small, negative, not always statistically significant) across these samples but are closer to zero in the alternative samples. We view this as reassuring since a true spillover effect is likely to be larger when the siblings live together (siblings who do not live together likely spend less time with eachother) and when the peer is the only older sibling present (if there are multiple older siblings the behavior of any one older sibling may become less influential).

6.2 Comparing many specifications

We visually summarize the overall robustness of our results in a plot of 64 specifications (most of which have been presented in other parts of this paper), which vary by the inclusion of fixed effects, the inclusion of controls, the order of the running variable polynomial, and the sample, in Figure A2 and Figure A3. Each regression in Figure A2 uses an indicator for any binge drinking as the outcome, and each coefficient is scaled by the corresponding "control" mean (mean of the outcome just below the cutoff)—facilitating comparisons with the EGW point estimate of +19%. Among 64 total specifications, 58 scaled estimates are negative, though the magnitudes vary and confidence intervals usually do not rule out a null effect. Among 64 total specifications, 59 confidence intervals (95%) exclude the EGW point estimate. Figure A3 shows similar results with the count of binge drinking days as the outcome.

One additional reassuring pattern in this figure is that the handful of outlier CIs (in both figures) which include large positive effects *all* include quadratic polynomials in the running variable. Visual inspection of various binscatters in this paper suggest that the relationship between the outcome and running variable is likely linear in our data. Further, higher-order polynomials in RD designs are known to increase the risk of detecting spurious effects (Gelman and Imbens, 2019).

6.3 Fixed effects

Figure A2 and (to a lesser extent) Figure A3 highlight that our results are somewhat sensitive to the inclusion of younger sibling FEs. Models with FEs generally have more negative point estimates and tighter confidence intervals. While the improved precision is expected, sensitivity of the point estimates to the inclusion of FEs may appear surprising given that FEs should not be necessary for identification in our research design, and that adding FEs does not change the estimation sample (as shown in, e.g., Table 3). However, as explained in detail by Miller et al. (forthcoming), FE estimates are identified only by groups that have variation in treatment (here, by younger siblings who show up in our sample at least once before their older sibling turns 21 and at least once after).

In Table A4 we demonstrate that such "switchers" make up roughly two-thirds of the corresponding regression sample used in our main analyses (siblings who live together and are the two oldest siblings in the household), and that these switchers differ meaningfully in some dimensions from the remainder of the sample ("non-switchers"). Given prior results (Table 5) showing that treatment effects in this context are heterogeneous, we take this as suggestive evidence that the sensitivity of our main results to the inclusion of FEs is driven by treatment effect heterogeneity. Since FEs also buy us improvements in precision, and we have no *a priori* reason to prefer estimates for the combined sample over the switcher sample, we consider the FE estimates to be preferred.

Table A5 shows characterstics of similar switcher and non-switcher subsets of the alternative sample that *does not* require the siblings to live together at the time of the sample.[24] A common reason for sibling pairs in our main sample to appear only when the older sibling is under 21 (i.e., to be non-switchers) is that the older sibling moves out too soon. It is therefore not suprising that switchers make up a much larger proportion of this alternative sample (without the "same household" restriction). Reassuringly, Figure A2 and Figure A3 also demonstrate that results in this alternative sample are less sensitive to the inclusion of FEs.

6.4 Bandwidth and estimation

All results discussed thus far have relied on a bandwidth of 24 months (i.e., restricting the sample to sibling pairs in which the older sibling is between the ages of 19 years and 0 months and 23 years and 0 months). This follows prior literature utilizing MLDA RDDs, which nearly universally chooses this bandwidth (e.g., Carpenter and Dobkin, 2009; Deza, 2015; Hansen and Waddell, 2016; Carpenter et al., 2016).

[24] The sample used in the bottom panel of Table 6 and denoted as "Sample 4" in Figure A2 and Figure A3.

We demonstrate the robustness of our main results to the choice of bandwidth in two ways. First, Figure 6 graphs point estimates and confidence intervals from the preferred model[25] for 36 separate bandwidths (single month increments from 12 to 48 months). Results are stable across bandwidth choices, although (expectedly) less precise as bandwidth shrinks. Second, we verify that our results persist when using the "continuity" framework for estimation and inference in RDDs developed by Cattaneo et al. (2020). This approach involves local polynomial estimation of the discontinuity, a data-driven bandwidth selection approach which is mean squared error optimal, and bias-corrected confidence intervals. Table A6 presents the results of the continuity-based RD models with the triangular kernel. Column (1) and (3) use MSE-optimal bandwidths, while column (2) and column (4) use our ad-hoc 24-month bandwidth. All columns include the MSE-optimal estimates and bias-corrected robust 95% confidence intervals where the standard errors are cluster robust at individual level. Consistent to Table 3, all coefficients are negative with moderate magnitudes. For models with individual fixed effects, the coefficients are large and statistically significant compared to models without fixed effects, a pattern also present in Table 3.

7 Conclusions

We focus on a population which allows for the estimation of causally interpretable endogenous peer effects in alcohol consumption under relatively weak assumptions: sibling pairs close to the MLDA in the United States. This setting is helpful for two reasons. First, the peer group (sibling pairs) is not chosen. Second, the average alcohol consumption of young adults has been shown to increase discontinuously at the MLDA. This allows for the use of a RDD. While somewhat imprecise, estimates suggest that alcohol consumption of younger siblings decreases when the older sibling gains legal access to alcohol. We are consistently able to rule out large positive effects reported in the prior literature.

Several limitations are worth mentioning. First, as in any RDD, external validity is a concern. Our results apply only to a certain peer group, adolescent siblings residing in the same household where the older sibling is near the MLDA. However, our results are less limited in this way than a typical MLDA-based RDD, given that the running variable and the outcome are taken from different individuals (ages range from 12-20 years for younger siblings in our sample whose older sibling is within one month of the cutoff). Second, an older sibling's legal drinking status could *potentially* affect younger sibling consumption indirectly, e.g., via parental

[25] Donut RD specification including a vector of controls, a linear polynomial in the running variable fully interacted with the cutoff dummy, and individual level fixed effects

responses or access effects. However, one key set of confounders (parental responses) are, to some extent, ruled out based on observable information in the data, and another (access effects) are likely to bias our results towards zero.

Our results emphasize the important distinction between contextual (interpretable as the causal effect of exposure to the peer) and endogenous (interpretable as the causal effect of the peer's behavior) peer effects. A key rationale for policy makers to understand peer effects in alcohol consumption and other costly behaviors, is the potential for the costs of those behaviors—or the benefits of interventions which successfully decrease the prevalence of the behaviors—to be socially multiplied. This will occur only if the peer effect in question is an endogenous peer effect. Many studies on peer effects, notably the leading work on spillovers in risky health behaviors based on randomly assigned college roommates, are only able to identify contextual peer effects. The existence of contextual peer effects in a population does not necessarily imply the existence of endogenous peer effects and policy makers should exercise caution in acting on results which identify only the former.

An important goal for future work will be to understand the differences between the growing set of well-identified results on peer effects in alcohol consumption. The research designs used in our work, Fletcher and Marksteiner (2017), and the series of papers utilizing randomly assigned college roommates (Duncan et al., 2005; Eisenberg et al., 2014; Guo et al., 2015) all differ in at least two key dimensions: The peer group studied and the nature of the variation in alcohol consumption exploited by the researcher. Further, the interpretation of the results differs since studies based on randomly assigned roommates identify only contextual peer effects. Future work should aim to understand to what extent these three factors explain the differences in these results and to understand the nature of spillovers in alcohol consumption in more generalizable contexts.

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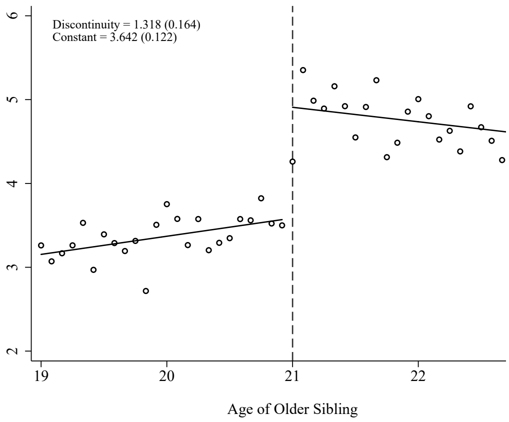
Figures and Tables

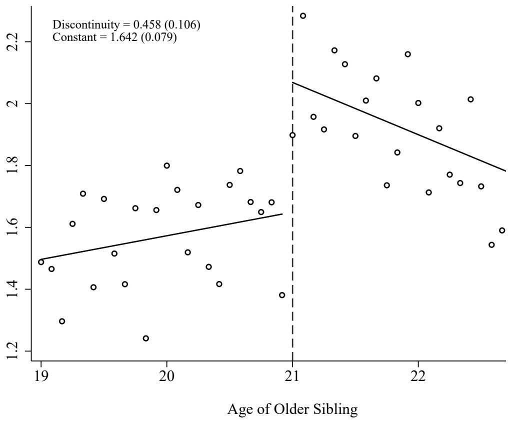
Figure 1: Discontinuities in Older Sibling Alcohol Consumption

(a) Count of Drinking Days



(b) Prob. of Any Drinking

(c) Count of Binge Drinking Days

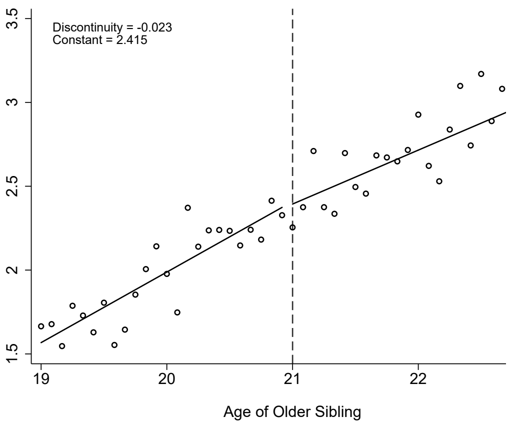


(d) Prob. of Any Binge Drinking

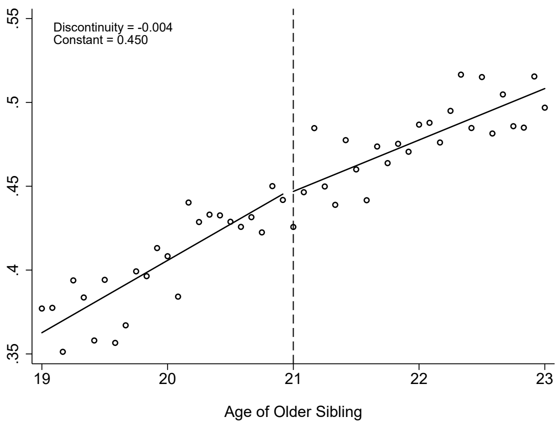
<u>Notes:</u> Each panel shows mean alcohol consumption in each older-sibling-age bin (in months), with linear fits estimated separately on each side of the cutoff. Point estimates and standard errors at the top left of each panel are from the corresponding OLS regressions of the relevant alcohol consumption measure on age (centered at 21), an age-21+ indicator, and their interaction (estimated with individual level data). The sample includes all NLSY97 respondents in the age range who have 1 or more younger siblings and are the oldest siblings in their household.

Figure 2: Smoothness of Covariates at Cutoff

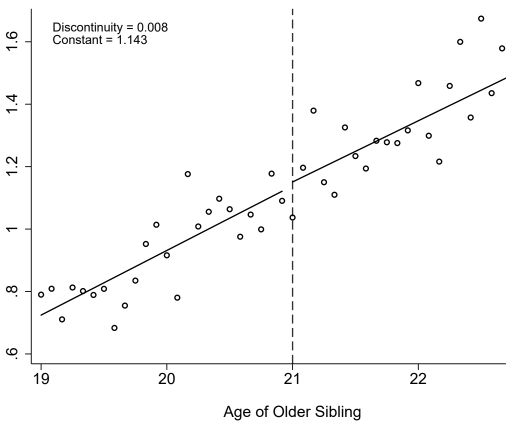
(a) Predicted Count of Drinking Days



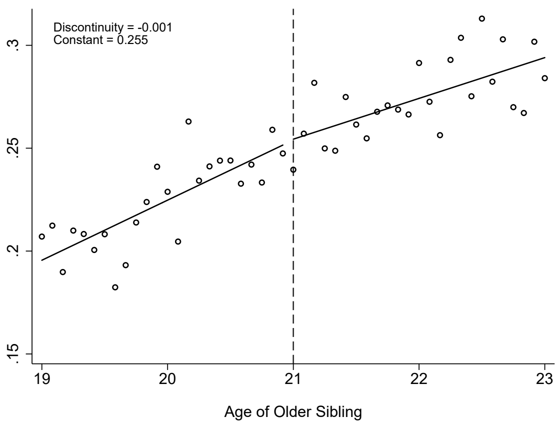
(b) Predicted Prob. of Any Drinking



(c) Predicted Count of Binge Drinking Days



(d) Predicted Prob. of Any Binge Drinking

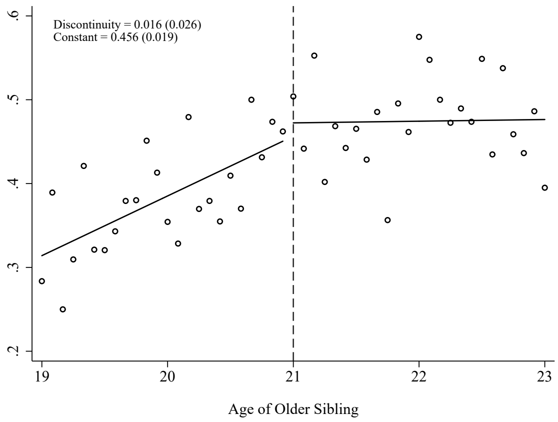


Notes: Each panel shows mean predicted alcohol consumption in each older-sibling-age bin (in months), with linear fits estimated separately on each side of the cutoff (using the binned means). Predicted alcohol consumption measures are fitted values from a regression of the corresonding alcohol consumption measure on age, gender of both siblings, educational enrollment, highest completed education, work status, indicators for whether the household lives in an urban area, census region dummies, AFQT score, household size, and interview month/year. (All variables refer to the younger sibling's information unless otherwise noted.) Point estimates and standard errors at the top left of each panel are from corresponding OLS regressions of the prediction on the running variable, the cutoff indicator, and their interaction (estimated with individual level data). Standard errors are calculated via bootstrap. The sample includes all NLSY97 respondents in the age range who are the second oldest siblings in their household.

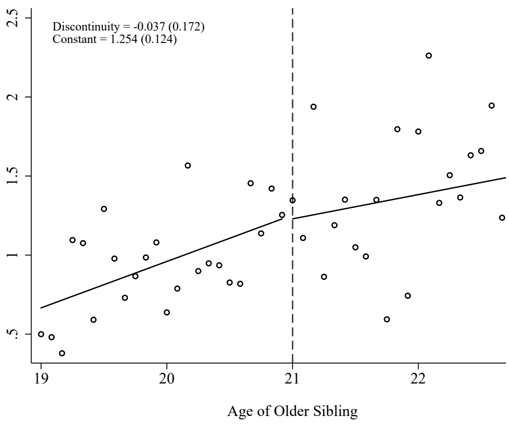
Figure 3: Reduced Form

(a) Count of Drinking Days

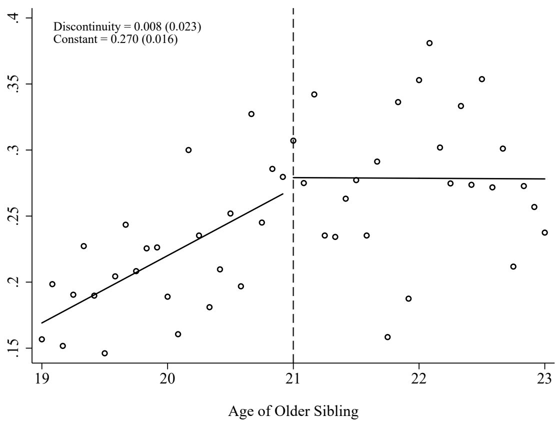
(b) Prob. of Any Drinking



(c) Count of Binge Drinking Days



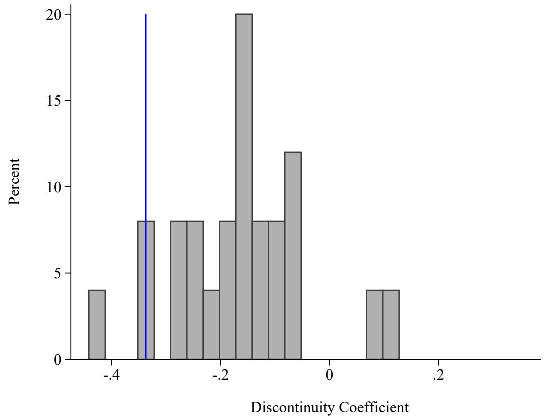
(d) Prob. of Any Binge Drinking



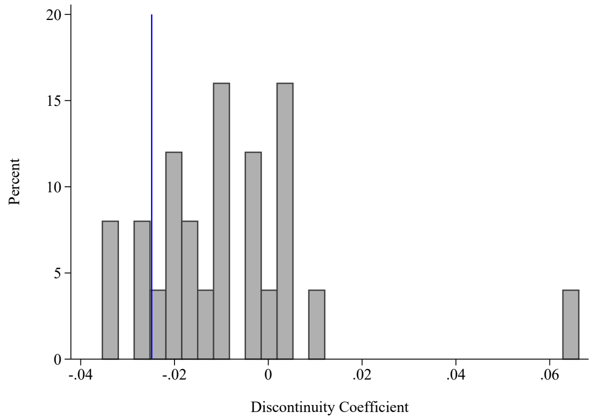
Notes: Each panel shows mean alcohol consumption in each older-sibling-age bin (in months), with linear fits estimated separately on each side of the cutoff. Point estimates and standard errors at the top left of each panel are from corresponding OLS regressions of the younger sibling's alcohol consumption on the running variable, the cutoff indicator, and their interaction (estimated with individual level data). The sample includes all NLSY97 respondents in the age range who are the second oldest siblings in their household.

Figure 4: Placebo Discontinuities

(a) Count of Binge Drinking Days

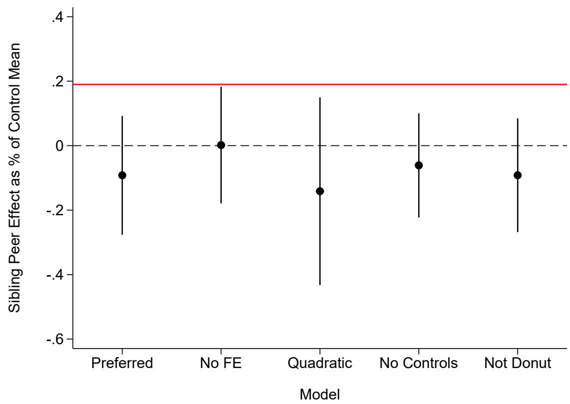


(b) Prob. of Any Binge Drinking



Notes: Figure shows the distribution of point estimates from a series of placebo discontinuity estimates, at running variable values near but not equal to the cutoff. Each underlying regression uses the preferred specification in Table 3 (donut, linear, controls, FE) and a different cutoff age (in months) which varies from one year below to one year above age 21. Actual estimate is shown with a vertical blue line.

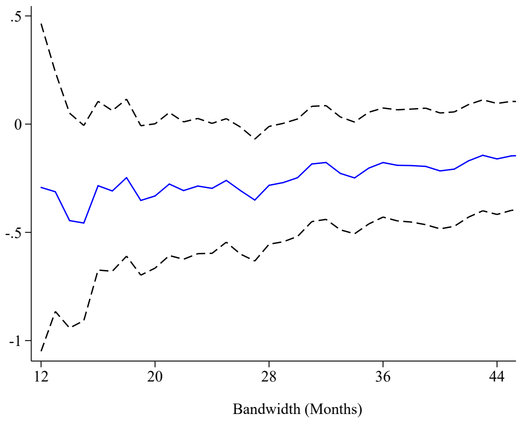
Figure 5: Comparison with Eisenberg et al. (2014)



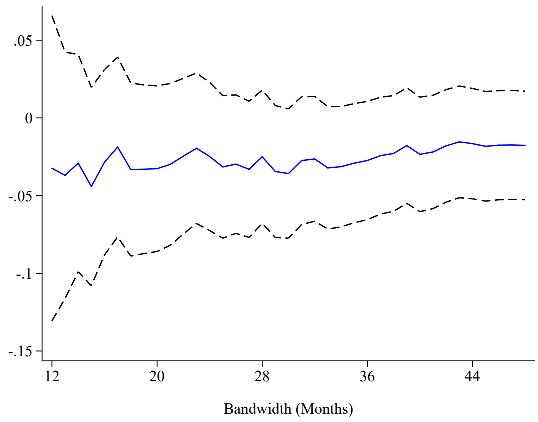
Notes: Figure shows point estimates and confidence intervals from various specifications in Table 3 alongside the main estimate from EGW (red line). The outcome in each regression (including EGW's) is an indicator for any binge drinking days in the past month.

Figure 6: Robustness to Different Bandwidths

(a) Count of Binge Drinking Days



(b) Prob. of Any Binge Drinking



Notes: Figure shows point estimates and confidence intervals from the preferred specification in Table 3 (donut, linear, controls, FE) estimated with different bandwidths (in months).

Table 1: NLSY97 Sample Summary Statistics

	Full Sample mean/sd	RDD Sample mean/sd
Younger Sibling's Past Month Drinking:		
Drinking Days	2.41	2.31
	(4.98)	(4.76)
Any Drinking Days	0.41	0.42
	(0.49)	(0.49)
Binge Days	1.02	1.14
	(3.03)	(3.25)
Any Binge Days	0.23	0.24
	(0.42)	(0.43)
Younger Sibling's Characteristics:		
Age	18.72	17.99
	(3.97)	(1.72)
Female	0.47	0.48
	(0.50)	(0.50)
Race: Black	0.28	0.26
	(0.45)	(0.44)
Race: Hispanic	0.26	0.24
	(0.44)	(0.43)
In High School	0.53	0.55
	(0.50)	(0.50)
In College	0.15	0.20
	(0.35)	(0.40)
Urban	0.78	0.77
	(0.41)	(0.42)
Worked Past Yr	0.66	0.71
	(0.47)	(0.46)
Household Income	$63,484.53	$61,559.45
	(58,841.17)	(55,247.96)
AFQT score	42,842.18	44,616.99
	(28,720.84)	(29,150.67)
Older Sibling's Characteristics:		
Age	22.15	20.87
	(5.29)	(1.17)
Female	0.47	0.47
	(0.50)	(0.50)
Worked Past Yr	0.38	0.39
	(0.49)	(0.49)
Currently Enrolled In School	0.53	0.50
	(0.50)	(0.50)
N	15,230	5,702

Notes: The full sample consists of all NLSY respondents with older siblings living in the same household. Our analysis sample ("RDD") consists of co-resident siblings who are the oldest two siblings in the household, where the younger sibling is a NLSY97 respondent and the older sibling is between the ages of 19 and 23.

Table 2: Discontinuities in Older Sibling Alcohol Consumption

	Count of Drinking Days				
	(1)	(2)	(3)	(4)	(5)
Age 21+	1.447***	1.585***	1.834***	1.339***	1.328***
	(0.152)	(0.169)	(0.241)	(0.133)	(0.146)
Constant	4.801***	3.205***	4.658***	3.720***	4.785***
	(0.727)	(0.502)	(0.733)	(0.083)	(0.723)
Observations	16682	16682	16682	21376	17049
	Any Drinking Days				
Age 21+	0.083***	0.090***	0.105***	0.083***	0.079***
	(0.013)	(0.013)	(0.019)	(0.011)	(0.012)
Constant	0.545***	0.434***	0.536***	0.585***	0.556***
	(0.057)	(0.041)	(0.058)	(0.008)	(0.056)
Observations	16682	16682	16682	21376	17049
	Count of Binge Drinking Days				
Age 21+	0.454***	0.587***	0.678***	0.404***	0.405***
	(0.100)	(0.111)	(0.161)	(0.089)	(0.095)
Constant	2.406***	1.496***	2.282***	1.667***	2.437***
	(0.463)	(0.322)	(0.464)	(0.051)	(0.469)
Observations	16601	16601	16601	21268	16964
	Any Binge Drinking Days				
Age 21+	0.057***	0.068***	0.073***	0.058***	0.050***
	(0.012)	(0.013)	(0.020)	(0.011)	(0.012)
Constant	0.427***	0.305***	0.423***	0.342***	0.422***
	(0.053)	(0.041)	(0.054)	(0.007)	(0.053)
Observations	16601	16601	16601	21268	16964
Fixed Effects	X		X	X	X
Quadratic			X		
Controls	X	X	X		X
Donut	X	X	X	X	

Notes: Controls include the month and year of the survey, the gender and age of the respondent and siblings, race of respondents, educational attainment and enrollment of respondents, geography (urban/rural, census region), household size, AFQT score of the respondent, an indicator for whether or not the respondent has children, and an indicator for whether the respondent worked in the past year. All models are estimated in a sample of NLSY97 respondents who are the oldest siblings currently residing in their household who are between the ages of 19 and 23. All models include cluster robust standard errors at individual level. Age is centered at 21 years. +, *, **, and *** denote statistical significance at the 10%, 5%, 1%, and 0.1% levels, respectively.

Table 3: Reduced Form

	(1)	(2)	(3)	(4)	(5)
	Count of Drinking Days				
Sibling 21+	-0.137	0.098	-0.288	0.156	-0.058
	(0.249)	(0.249)	(0.431)	(0.229)	(0.241)
Constant	2.577	4.555***	2.505	2.389***	2.885
	(2.023)	(0.830)	(2.008)	(0.150)	(1.890)
Observations	4282	4282	4282	5527	4373
	Any Drinking Days				
Sibling 21+	-0.031	-0.009	-0.059	0.004	-0.035
	(0.028)	(0.028)	(0.046)	(0.025)	(0.027)
Constant	0.828***	0.657***	0.840***	0.459***	0.904***
	(0.171)	(0.089)	(0.173)	(0.016)	(0.163)
Observations	4282	4282	4282	5527	4373
	Count of Binge Drinking Days				
Sibling 21+	-0.337*	-0.176	-0.372	-0.195	-0.297+
	(0.159)	(0.169)	(0.267)	(0.161)	(0.153)
Constant	1.512	2.846***	1.440	1.313***	1.567
	(1.485)	(0.599)	(1.465)	(0.107)	(1.383)
Observations	4278	4278	4278	5521	4369
	Any Binge Drinking Days				
Sibling 21+	-0.025	0.000	-0.038	-0.017	-0.025
	(0.025)	(0.025)	(0.040)	(0.022)	(0.024)
Constant	0.408*	0.439***	0.402*	0.282***	0.443*
	(0.183)	(0.078)	(0.185)	(0.015)	(0.184)
Observations	4278	4278	4278	5521	4369
FE	X		X	X	X
Quadratic			X		
Controls	X	X	X		X
Donut	X	X	X	X	

Notes: Controls include the month and year of the survey, the gender and age of the respondent and siblings, race of respondents, educational attainment and enrollment of respondents, geography (urban/rural, census region), household size, AFQT score of the respondent, an indicator for whether or not the respondent has children, and an indicator for whether the respondent worked in the past year. All models are estimated in a sample of NLSY97 respondents who have exactly one older sibling in their household, and where that older sibling is between the ages of 19 and 23. All models include cluster robust standard errors at individual level. Sibling age is centered at 21 years. +, *, **, and *** denote statistical significance at the 10%, 5%, 1%, and 0.1% levels, respectively.

Table 4: Discontinuities Older Sibling Alcohol Consumption in Subgroups, Binge Days

	Parental Education		Household Income	
	$<$ HS	$\geq$ HS	$<$ Median	$\geq$ Median
Age 21+	0.318^{+}	0.546***	0.142	0.971***
	(0.184)	(0.153)	(0.146)	(0.237)
Constant	2.359*	2.889**	3.032***	1.763
	(1.055)	(1.022)	(0.672)	(1.798)
Observations	3992	8085	8651	5009
	Older Sibling's Employment Status		Older Sibling's Sex	
	Working	Not Working	Male	Female
Age 21+	0.486***	0.828**	0.592***	0.319**
	(0.112)	(0.315)	(0.171)	(0.108)
Constant	2.605***	3.864*	3.204***	1.599**
	(0.513)	(1.933)	(0.782)	(0.490)
Observations	14471	2130	8127	8474
	Race			
	Black	Hispanic	Non-Black, Non-Hispanic	
Age 21+	0.072	0.294	0.656***	
	(0.129)	(0.245)	(0.146)	
Constant	0.777	2.382^{+}	2.783***	
	(0.583)	(1.407)	(0.621)	
Observations	3911	3139	9551	

Notes: Controls include the month and year of the survey, the gender and age of the respondent and siblings, race of respondents, educational attainment and enrollment of respondents, geography (urban/rural, census region), household size, AFQT score of the respondent, an indicator for whether or not the respondent has children, and an indicator for whether the respondent worked in the past year. All models are estimated in the corresponding subgroup of a sample of NLSY97 respondents who are the oldest siblings currently residing in their household who are between the ages of 19 and 23. The median household income is $53,515. All models include cluster robust standard errors at individual level. Age is centered at 21 years. +, *, **, and *** denote statistical significance at the 10%, 5%, 1%, and 0.1% levels, respectively.

Table 5: Reduced Form in Subgroups, Binge Days

	Parental Education		Household Income	
	< HS	≥ HS	< Median	≥ Median
Sibling 21+	-0.124	-0.417^{+}	0.143	-1.090^{+}
	(0.277)	(0.217)	(0.311)	(0.645)
Constant	-1.695	2.791	0.145	-12.152**
	(1.476)	(2.454)	(2.055)	(4.047)
Observations	1045	2206	875	875
	Older Sibling's Employment Status		Older Sibling's Sex	
	Working	Not Working	Male	Female
Sibling 21+	-0.638*	-0.495*	-0.662**	0.064
	(0.294)	(0.221)	(0.245)	(0.208)
Constant	8.474*	1.463	-6.995*	-0.268
	(4.300)	(1.458)	(2.725)	(1.087)
Observations	1637	2641	2266	2012
	Sibling Sex Composition		Sibling Age Difference	
	Same	Opposite	< 30 Months	≥ 30 Months
Sibling 21+	-0.616**	-0.040	-0.290	-0.439*
	(0.231)	(0.223)	(0.261)	(0.202)
Constant	-0.194	0.614	13.177***	1.284
	(1.812)	(1.716)	(3.087)	(1.583)
Observations	2379	1899	1966	2312
	Experience with Alcohol (younger sibling in prior year)		Experience with Alcohol (younger sibling in prior year)	
	Ever Drinkers	Never Drinkers	Ever Binge Drinkers	Never Binge Drinkers
Sibling 21+	-0.488*	-0.166	-0.644	-0.195^{+}
	(0.243)	(0.117)	(0.560)	(0.112)
Constant	-0.325	0.750	0.869	1.005
	(1.845)	(0.483)	(2.909)	(0.672)
Observations	2511	1492	1069	2783
	Race			
	Black	Hispanic	Non-Black, Non-Hispanic	
Sibling 21+	-0.294	-0.225	-0.438^{+}	
	(0.279)	(0.275)	(0.254)	
Constant	0.494	-0.239	3.262	
	(0.959)	(1.703)	(2.465)	
Observations	1072	909	2297	

Notes: Controls include the month and year of the survey, the gender and age of the respondent and siblings, race of respondents, educational attainment and enrollment of respondents, geography (urban/rural, census region), household size, AFQT score of the respondent, an indicator for whether or not the respondent has children, and an indicator for whether the respondent worked in the past year. All models are estimated in the corresponding subgroup of a sample of NLSY97 respondents who have exactly one older sibling in their household, and where that older sibling is between the ages of 19 and 23. The median household income is $53,515 in this sample. All models include cluster robust standard errors at individual level. Sibling age is centered at 21 years. +, *, **, and *** denote statistical significance at the 10%, 5%, 1%, and 0.1% levels, respectively.

Table 6: Alternative Samples

	Drinking Days	Any Drinking Days	Binge Drinking Days	Any Binge Drinking Days
	Two oldest siblings living in the same household			
Sibling 21+	-0.137	-0.031	-0.337*	-0.025
	(0.249)	(0.028)	(0.159)	(0.025)
Constant	2.577	0.828***	1.512	0.408*
	(2.023)	(0.171)	(1.485)	(0.183)
Observations	4282	4282	4278	4278
	Two oldest siblings			
Sibling 21+	0.026	-0.025	-0.252+	-0.024
	(0.229)	(0.026)	(0.147)	(0.023)
Constant	5.618**	0.659***	2.205*	0.580*
	(1.861)	(0.195)	(1.069)	(0.247)
Observations	5100	5100	5094	5094
	Two siblings living in the same household			
Sibling 21+	-0.230	-0.015	-0.202+	-0.026
	(0.186)	(0.020)	(0.121)	(0.018)
Constant	2.232	0.451***	0.564	0.296**
	(1.709)	(0.104)	(1.139)	(0.099)
Observations	6988	6988	6975	6975
	Two siblings			
Sibling 21+	-0.079	-0.025	-0.107	-0.020
	(0.161)	(0.016)	(0.101)	(0.015)
Constant	2.354+	0.190*	0.248	0.525***
	(1.218)	(0.077)	(0.820)	(0.074)
Observations	9975	9975	9958	9958

Notes: The first panel is estimated in a sample of NLSY97 respondents who have exactly one older sibling in their household, and where that older sibling is between the ages of 19 and 23. The second panel is estimated in a sample of NLSY97 respondents who are the second oldest siblings, where their older sibling is between the ages of 19 and 23, and the older sibling may not currently reside in the same household as the respondent. The third panel is estimated in a sample of NLSY97 respondents who have one or more older siblings in their household where the closest older sibling to the respondent is between the ages of 19 and 23. The last panel is estimated in a sample of NLSY respondents who have one or more older siblings where the closest older sibling to the respondent is between the ages of 19 and 23, and the closest older sibling may not currently reside in the same household as the respondent. Controls include the month and year of the survey, the gender and age of the respondent and siblings, race of respondents, educational attainment and enrollment of respondents, geography (urban/rural, census region), household size, AFQT score of the respondent, an indicator for whether the respondent has children, and an indicator for whether the respondent worked in the past year. All models include cluster robust standard errors at individual level. Sibling age is centered at 21 years. +, *, **, and *** denote statistical significance at the 10%, 5%, 1%, and 0.1% levels, respectively.

Appendices

A Evidence against offsetting parental responses

As described in section 5.1 parental responses are one key pathway through which the older sibling's legal access to alcohol could directly (i.e., not via an endogenous peer effect) affect the younger sibling's consumption of alcohol. The NLSY97 includes two separate questions which can provide some suggestive evidence on the role that parental responses play in explaining the results described above. First, all respondents are asked to score the degree to which they are monitored by their parents (including, if applicable, parents they live with and parents they do not live with) on a scale from 0-16, with higher scores indicating closer monitoring. Second, respondents are asked to classify their parents' parenting styles (again, including if applicable parents who do and do not live with the respondent) as either uninvolved, permissive, authoritarian, or authoritative.

In the first panel of Table A7 we present results for models similar to equation 1 with the parental monitoring score for the parents that the respondent lives with as the outcome. If the respondent lives with two parents, the outcome is their average score. In the second panel of Table A7, similar results are presented with an indicator for whether or not at least one parent was reported to be either authoritarian or authoritative.

The survey questions underlying these outcomes are asked only in survey rounds between 1997 and 2000. Luckily, a substantial portion of the sibling-pair-years that meet our sample restriction requirements are in this time period. However, some are not, and sample sizes are therefore smaller than the previously described full sample results in Table 3. Models in Table A7 are the same as those in Table 3 in order to demonstrate the robustness of the results.

In each of the 5 models for each of the three outcomes, point estimates for a discontinuity in parental behavior at the cutoff are either negative (suggesting parents become less strict or engage in less monitoring), not statistically significant, or both. In the preferred model (1) neither point estimate is economically significant. We interpret this as suggestive evidence that parental responses are not driving the observed decrease in younger sibling alcohol consumption at the cutoff.

B Time use of siblings near the MLDA

As described in section 5.1, a concern in our setting is that younger siblings may not be sufficiently exposed to changes in older sibling alcohol consumption that occur at the MLDA. This might be the case if, for example, siblings in this age range do not spend meaningful amounts of time together.

While a small literature on the shared time use of siblings does exist (e.g., Dunifon et al., 2017; Wikle and Hoagland, 2020), there does not appear to be any work on the age ranges relevant for our analysis. We therefore address this concern with a simple descriptive analysis which demonstrates that siblings at ages near the MLDA spend substantial amounts of time together—even after excluding time with parents, and especially when focusing on same-gender sibling pairs.

The ATUS is a repeated cross-sectional survey, which regularly samples a subset of Current Population Survey (CPS) respondents and solicits a detailed time-diary for one day's worth of activities (Hofferth et al., 2020). These time diaries detail the minute-level activities of respondents, including what activity was being performed, where, and with whom. Since the ATUS sample is drawn from the CPS, the two surveys can be linked so that the household structure of each ATUS respondent is observable. We use this information to select all respondents from the 2003-2019 waves of the ATUS who lived with *only one* older sibling who was 23 years old or younger. We then use the information on who activities were performed with to characterize the strength of these sibling relationships and provide suggestive evidence regarding the potential for younger siblings to be exposed to changes in older sibling alcohol consumption. Summary statistics for our analysis sample of 2,795 ATUS respondents are shown in Table A8.

We calculate average time spent per day with an older sibling. For comparison, we also calculate average time spent per day with friends.[26] Finally, we present these means in several different subsets of activities. First, we move from all activities reported in the time-diary to activities performed while not at work or school—under the assumption that such "discretionary" time is more relevant for the behavior we are interested in. Second, we further restrict our focus to time spent not at work or school and without any parents present for the same reason. Finally, we break down this last category of discretionary and unsupervised time into the main activity groups defined by ATUS.[27] Results are shown in Figure A4.

The average respondent reports spending roughly 1 hour and 50 minutes with an older sibling during their diary-day, just under one hour of which is discretionary and unsupervised time, when alcohol consumption would be most likely to occur. Since a substantial portion of the average diary-day in our sample is spent either at work, at school, or with parents (see Table A8), this is a meaningful amount of time. Moreover, the activities that siblings engage in together during this discretionary and unsupervised time (right panel of the figure) are broadly

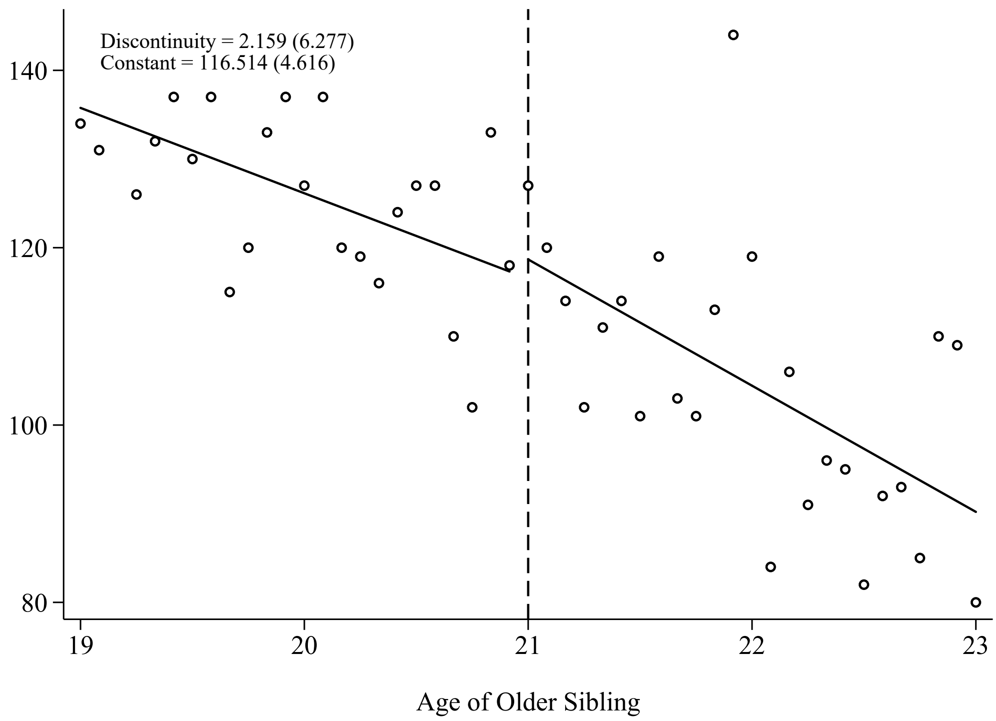
[26]We consider an activity to be performed with an older sibling (or similarly, with a friend) if the so-called "who" record in the ATUS data lists at least one older sibling (or friend).

[27]Note that it is not possible to directly observe alcohol consumption in the ATUS.

similar to the activities engaged in with friends. While it is clear that respondents spend more time with friends (especially within discretionary and supervised time), we argue that this is strong evidence that the sibling pairs we study in our main analysis are a meaningful peer group for the behavior of interest.

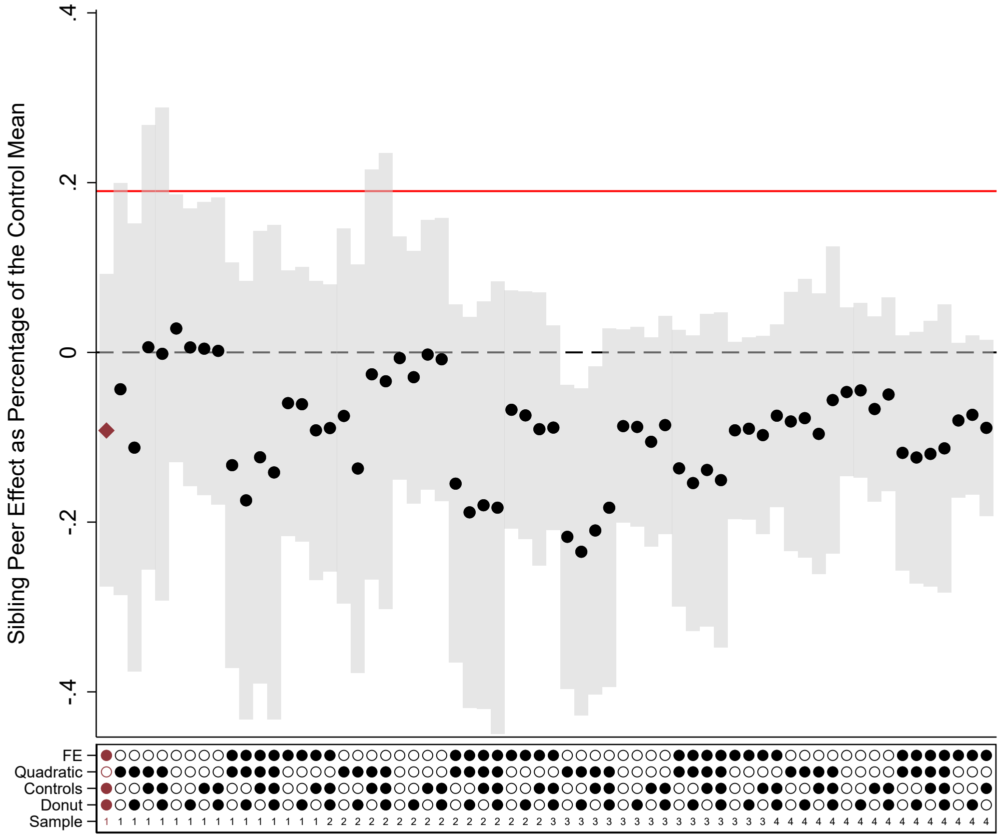
C Appendix Figures and Tables

Figure A1: Density of Running Variable



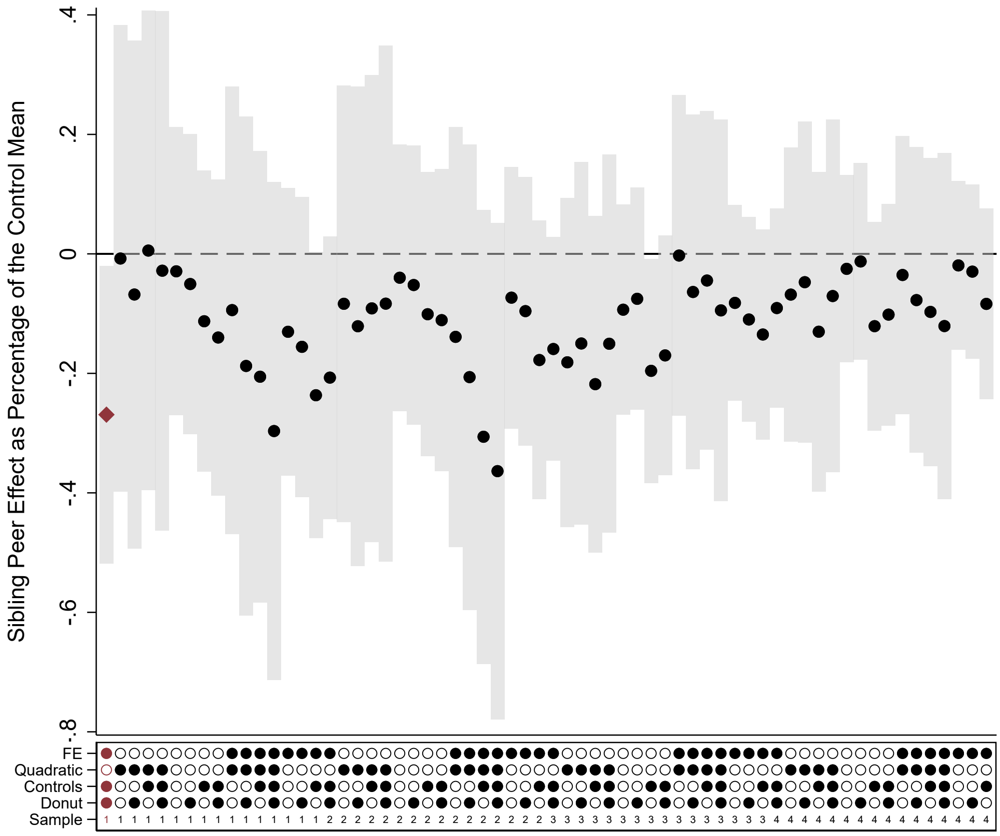
Notes: Y-axis shows the count of sibling-pairs with each running variable value (age in months of older sibling). Point estimates and standard errors are from a regression of the number of observations in each bin on the centered-at-21 running variable, the treatment cutoff, and their interaction (estimated with individual level data).

Figure A2: Robustness to Alternative Specifications
(Outcome = Prob. of Any Binge Drinking)



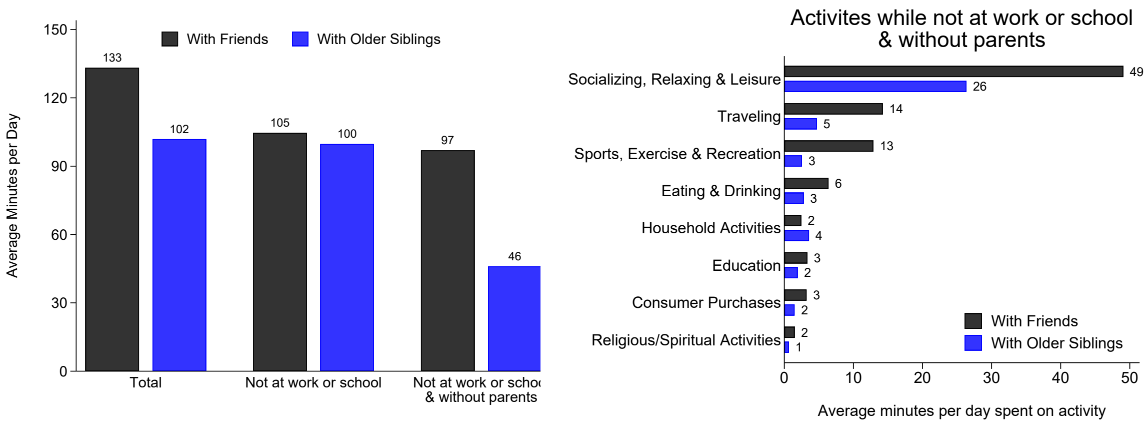
Notes: Figure shows point estimates and confidence intervals from various specifications in Table 3, all with an indicator for any binge days as the outcome, and all scaled by the pre-cutoff mean (constant in the corresponding regression on the centered-at-zero running variable, the treatment cutoff, and their interaction). Red line shows the point estimate from Eisenberg et al. (2014) for comparison. "Sample" refers to different subsets of sibling pairs, defined by (i) whether they live together, and (ii) whether they are the oldest two siblings in the household (or family). 1 = same household, oldest two siblings in family. 2 = same household, peer is closest older sibling but not necessarily the only older sibling. 3 = siblings do not necessarily live together, siblings are the oldest two siblings in the family. 4 = siblings do not necessarily live together, peer is closest older sibling but not necessarily the only older sibling.

Figure A3: Robustness to Alternative Specifications
(Outcome = Count of Binge Drinking Days)



Notes: Figure shows point estimates and confidence intervals from various specifications in Table 3, all with the count of binge drinking days as the outcome, and all scaled by the pre-cutoff mean (constant in the corresponding regression on the centered-at-zero running variable, the treatment cutoff, and their interaction). "Sample" refers to different subsets of sibling pairs, defined by (i) whether they live together, and (ii) whether they are the oldest two siblings in the household (or family). 1 = same household, oldest two siblings in family. 2 = same household, peer is closest older sibling but not necessarily the only older sibling. 3 = siblings do not necessarily live together, siblings are the oldest two siblings in the family. 4 = siblings do not necessarily live together, peer is closest older sibling but not necessarily the only older sibling.

Figure A4: Time Spent with Older Siblings in the ATUS



Notes: Descriptive statistics are from a sample of 2,795 respondents in the 2003-2019 waves of the ATUS who have 1 older siblings under the age of 23 in their household. Each panel shows the average (across respondent) time per day spent with either friends (black, left bars in each pair in the left panel and top bars in each pair in the right panel) or older siblings (blue, right bars in each pair in the left panel and bottom bars in each pair in the right panel) in various categories.

Table A1: Correlations in Alcohol Consumption Between Siblings

	Younger Sibling Consumption			
	Count of Drinking Days		Any Drinking Days	
	(1)	(2)	(1)	(2)
Older Sibling Consumption	0.161***	0.136***	0.178***	0.168***
	(0.020)	(0.019)	(0.018)	(0.016)
Constant	3.419***	5.074***	0.485***	0.487***
	(0.918)	(0.997)	(0.113)	(0.091)
Mean	1.938	2.356	0.374	0.421
N	3808	6096	3808	6096
	Count of Binge Drinking Days		Any Binge Drinking Days	
Older Sibling Consumption	0.163***	0.133***	0.166***	0.140***
	(0.026)	(0.021)	(0.018)	(0.016)
Constant	2.043**	2.874***	0.326***	0.434***
	(0.642)	(0.604)	(0.091)	(0.081)
Mean	0.881	1.084	0.198	0.236
N	3806	6096	3806	6096
Lag		X		X

Notes: Each model has the younger sibling's consumption as the outcome and the older sibling's consumption as the covariate of interest. Model 1 uses the contemporaneous consumption of the older sibling and excludes sibling pairs in which the older sibling was interviewed after the younger sibling. Model 2 uses the lagged (prior survey year) consumption. All models are estimated via OLS with cluster robust standard errors at the household level and are limited to households with exactly two NLSY97 respondent siblings, in which the older sibling is 23 years old or younger. All models include a vector of controls for both siblings similar to those described in Section 2. Means of each outcome (for younger siblings) are shown for each model. +, *, **, and *** denote statistical significance at the 10%, 5%, 1%, and 0.1% levels, respectively.

Table A2: Smoothness of Covariates at Cutoff

	Predicted Outcomes			
	Drinking Days	Any Drinking Days	Binge Drinking Days	Any Binge Binge Days)
Constant	2.415***	0.450***	1.143***	0.255***
	(0.091)	(0.010)	(0.060)	(0.009)
Sibling 21+	-0.023	-0.004	0.008	-0.001
	(0.084)	(0.009)	(0.053)	(0.008)
N	4373	4373	4369	4369

Notes: Outcomes are predicted values from a regression of the relevant consumption measure on the month and year of the survey, the race of the respondent, the gender of the respondent and siblings, educational attainment and enrollment of the respondent, geography (urban/rural, census region), household size, AFQT score of the respondent, an indicator for whether or not the respondent has children, and an indicator for whether the respondent worked in the past year. Results shown are from regressions of these predicted values on the age of the older sibling in months, an indicator for whether the older sibling is over 21, and their interaction. Standard errors are bootstrapped. +, *, **, and *** denote statistical significance at the 10%, 5%, 1%, and 0.1% levels, respectively.

Table A3: Effect of Younger Sibling on Older Sibling

	Drinking Days	Any Drinking Days	Binge Drinking Days	Any Binge Drinking Days
	Two youngest siblings living in the same household			
Sibling 21+	-0.117	0.003	0.060	0.055
	(0.467)	(0.034)	(0.258)	(0.036)
Constant	12.259***	0.236	0.620	0.122
	(2.778)	(0.260)	(1.550)	(0.260)
Observations	2380	2380	2333	2333
	Two youngest siblings			
Sibling 21+	-0.013	0.016	-0.016	0.062*
	(0.325)	(0.026)	(0.214)	(0.029)
Constant	10.813**	0.513**	2.064+	0.152
	(3.939)	(0.177)	(1.158)	(0.277)
Observations	3758	3758	3702	3702
	Two siblings living in the same household			
Sibling 21+	0.006	0.007	-0.112	0.001
	(0.227)	(0.017)	(0.136)	(0.018)
Constant	4.968***	0.648***	2.764**	0.353*
	(1.420)	(0.119)	(1.032)	(0.139)
Observations	7557	7557	7398	7398
	Two siblings			
Sibling 21+	-0.056	0.002	0.000	0.011
	(0.163)	(0.013)	(0.101)	(0.013)
Constant	5.380***	0.670***	2.464***	0.362***
	(1.022)	(0.098)	(0.728)	(0.097)
Observations	13367	13367	13145	13145

Notes: The first panel is estimated in a sample of NLSY97 respondents who have exactly one younger sibling in their household, and where that younger sibling is between the ages of 19 and 23. The second panel is estimated in a sample of NLSY97 respondents who are the second youngest siblings, where their younger sibling is between the ages of 19 and 23, and the younger sibling may not currently reside in the same household as the respondent. The third panel is estimated in a sample of NLSY97 respondents who have one or more younger siblings in their household where the closest younger sibling to the respondent is between the ages of 19 and 23. The last panel is estimated in a sample of NLSY respondents who have one or more younger siblings, where the closest younger sibling to the respondent is between the ages of 19 and 23, and the closest younger sibling may not currently reside in the same household as the respondent. Controls include the month and year of the survey, the gender and age of the respondent and siblings, race of respondents, educational attainment and enrollment of respondents, geography (urban/rural, census region), household size, AFQT score of the respondent, an indicator for whether the respondent has children, and an indicator for whether the respondent worked in the past year. All models include cluster robust standard errors at individual level. Sibling age is centered at 21 years. +, *, **, and *** denote statistical significance at the 10%, 5%, 1%, and 0.1% levels, respectively.

Table A4: Switcher and Non-Switcher Characteristics

	Switchers mean (sd)	Non-switchers mean (sd)	p-value
Age difference between the siblings	2.81	3.01	0.00
	(1.36)	(1.65)	
Siblings have same gender	0.55	0.53	0.04
	(0.50)	(0.50)	
Younger Sibling's Characteristics:			
Female	0.47	0.48	0.44
	(0.50)	(0.50)	
Race: Black	0.27	0.24	0.04
	(0.44)	(0.43)	
Race: Hispanic	0.24	0.24	0.75
	(0.43)	(0.42)	
Urban	0.78	0.75	0.01
	(0.41)	(0.43)	
AFQT score	46,037.04	42,094.33	0.00
	(29,391.96)	(28,584.32)	
Household Income	$66,654.55	$52,071.82	0.00
	(57,999.39)	(48,741.99)	
N	3716	1859	
N Sibling Pairs	1180	1423	

Notes: FE estimates of the effect of an older sibling turning 21 are identified only by groups who have variation in this treatment, i.e., younger siblings who show up in our sample at least once before their older sibling turns 21 and at least once after. This table compares these "switcher" sibling pairs to the corresponding "non-switcher" sibling pairs, in our main analysis sample (younger sibling is a NLSY-respondent, older sibling is between the ages of 19 and 23, siblings are the two oldest siblings in the household). The third column displays the p-value for the null hypothesis that the means in the switcher and non-switcher groups are equivalent.

Table A5: Switcher and Non-Switcher Characteristics in an Alternative Sample

	Switchers mean (sd)	Non-switchers mean (sd)	p-value
Age difference between the siblings	2.65	4.03	0.00
	(1.32)	(2.10)	
Siblings have same gender	0.52	0.53	0.68
	(0.50)	(0.50)	
Younger Sibling's Characteristics:			
Female	0.50	0.47	0.18
	(0.50)	(0.50)	
Race: Black	0.25	0.25	0.93
	(0.43)	(0.43)	
Race: Hispanic	0.20	0.28	0.00
	(0.40)	(0.45)	
Urban	0.76	0.76	0.76
	(0.43)	(0.43)	
AFQT score	45,620.70	40,877.99	0.00
	(28,991.13)	(26,712.87)	
Household Income	$53,699.29	$47,828.96	0.09
	(56,351.16)	(48,448.64)	
N	4240	345	
N Sibling Pairs	1788	278	

Notes: FE estimates of the effect of an older sibling turning 21 are identified only by groups who have variation in this treatment, i.e., younger siblings who show up in our sample at least once before their older sibling turns 21 and at least once after. This table compares these "switcher" sibling pais to the corresponding "non-switcher" sibling pairs, in an alternative sample that does not require the siblings to live together (i.e., here the younger sibling is a NLSY-respondent, the older sibling is between the ages of 19 and 23, and the siblings are the two oldest siblings in the family). The third column displays the p-value for the null hypothesis that the means in the switcher and non-switcher groups are equivalent.

Table A6: Continuity Models

	Count of Binge Drinking Days			
Sibling 21+	-0.429***	-0.355**	-0.118	-0.129
	[-0.598,-0.261]	[-0.529,-0.180]	[-0.612,0.372]	[-0.612, 0.597]
N	2874	4124	3931	4124
Bandwidth	16.64	24.00	22.86	24.00
	Any Binge Drinking Days			
Sibling 21+	-0.027*	-0.029*	-0.000	-0.000
	[-0.048,-0.005]	[-0.056,-0.002]	[-0.061,0.075]	[-0.069,0.107]
N	2555	4124	4278	4124
Bandwidth	14.07	24.00	24.59	24.00
FE	X	X		
Quadratic				
Controls	X	X	X	X
Donut	X	X	X	X

Notes: Controls include the month and year of the survey, the gender and age of the respondent and siblings, race of respondents, educational attainment and enrollment of respondents, geography (urban/rural, census region), household size, AFQT score of the respondent, an indicator for whether or not the respondent has children, and an indicator for whether the respondent worked in the past year. All models include the MSE-optimal estimates and bias-corrected robust 95% confidence intervals where the standard errors are cluster robust at individual level. Models 1 and 3 use MSE-optimal bandwidths, and the other models use our ad-hoc 24 month bandwidth. Sibling age is centered at 21 years. +, *, **, and *** denote statistical significance at the 10%, 5%, 1%, and 0.1% levels, respectively.

Table A7: Parenting style changes at the cutoff

	Degree of Parental Monitoring				
	(1)	(2)	(3)	(4)	(5)
Sibling 21+	-0.243	-0.722*	-0.311	-0.421	-0.198
	(0.321)	(0.354)	(0.508)	(0.292)	(0.294)
Constant	11.307***	9.525***	11.337***	8.653***	9.482***
	(2.290)	(1.418)	(2.324)	(0.167)	(2.353)
Observations	1467	1467	1467	1937	1493
	Parents: Authoritarian/Authoritative				
Sibling 21+	-0.010	-0.000	-0.017	0.007	-0.010
	(0.025)	(0.023)	(0.041)	(0.024)	(0.024)
Constant	0.348	-0.135*	0.369	0.262***	0.347
	(0.364)	(0.069)	(0.369)	(0.016)	(0.368)
Observations	4317	4317	4317	5575	4408
FE	X		X	X	X
Quadratic			X		
Controls	X	X	X		X
Donut	X	X	X	X	

Notes: Controls include the month and year of the survey, the gender and age of the respondent and siblings, race of respondents, educational attainment and enrollment of respondents, geography (urban/rural, census region), household size, AFQT score of the respondent, an indicator for whether or not the respondent has children, and an indicator for whether the respondent worked in the past year. All models are estimated in a sample of NLSY97 respondents who are the second oldest siblings currently residing in their household, where the oldest sibling is between the ages of 19 and 23. All models include cluster robust standard errors at individual level. Sibling age is centered at 21 years. +, *, **, and *** denote statistical significance at the 10%, 5%, 1%, and 0.1% levels, respectively.

Table A8: ATUS Sample Summary Statistics

	Mean	SD	Min	Max
Respondent Characteristics:				
Age	17.15	(1.76)	15.0	22.0
Female	0.49	(0.50)	0.0	1.0
In high school	0.58	(0.49)	0.0	1.0
In college	0.18	(0.39)	0.0	1.0
Employed	0.38	(0.49)	0.0	1.0
Surveyed on a weekend or holiday	0.31	(0.46)	0.0	1.0
Age of closest older sibling	19.86	(1.89)	16.0	23.0
Minutes Spent (during ATUS-diary day):				
At home	333.69	(221.19)	0.0	1440.0
At school	168.94	(223.88)	0.0	1020.0
At work	67.23	(163.28)	0.0	890.0
Alone	223.38	(196.42)	0.0	1370.0
With an older sibling	103.71	(171.45)	0.0	1200.0
With a parent	147.03	(192.08)	0.0	1200.0
With a friend	135.97	(208.73)	0.0	1140.0
With anyone else	186.36	(227.81)	0.0	1210.0
N	2,795			

Notes: Summary statistics from time-diaries for all ATUS respondents in the 2003-2019 waves who lived with one older sibling 23 years old or younger. All summary statistics are calculated using survey weights. Data from Hofferth et al. (2020).