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The Effect of Minimum Legal Drinking Age on the Incidence of First Pregnancy and Its Outcome

by

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Working Paper No. 2011-6R2

September 19, 2014

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The effect of Minimum Drinking Age laws on pregnancy, fertility, and alcohol consumption

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Abstract

Analysis of micro-level data reveals that changes in the minimum legal drinking age (MLDA) could induce changes in the intensity and location of alcohol consumption, sexual behavior, and teen fertility. Effects on teen fertility vary across different populations. Among 15-20 year-old non-poor whites, less restrictive legal access to alcohol decreases the probability of first pregnancy and abortion. For this group, easier legal access to alcohol likely increases the alcohol consumption in bars. For black and poor white young women, the results are sensitive to the alcohol consumption restrictions measure. A decrease in the MLDA increases the probability of first pregnancy and abortion. Yet, using a more precise measure that accounts for the MLDA and the woman's age, these results generally no longer hold.

JEL classification code: J13; J18

Keywords: Minimum Legal Drinking Age (MLDA), Pregnancy, Fertility, Sexual Behavior, Alcohol Consumption, Discrete Hazard

1. INTRODUCTION

Numerous studies associate teenage alcohol consumption with increased motor vehicle accident mortality, sexually transmitted diseases, date rape, and other risk-taking behaviors with long-term consequences.[1] I analyze the effect of alcohol consumption restrictions measured in terms of the state minimum legal drinking age (MLDA) on teen pregnancy and abortion rates. Understanding the causes of teen childbearing is crucial for designing effective public policy. As noted by Fletcher and Wolfe (2009), among other adverse consequences, teen childbearing lowers human capital accumulation for teen mothers and affects their earnings.

The causal effect of teen alcohol use on teen pregnancy is thought to be mediated through risky sexual behavior (Dee 2001, Sen 2003). Several studies report that sexually active teens under the influence of alcohol are less likely to use contraception (Markowitz et al. 2005) and are consequently more likely to experience an unintended pregnancy. If easy availability of alcohol leads to a higher likelihood of unintended pregnancy, then this increase in the number of pregnancies could result in a relatively larger increase in the number of abortions than live births, as unintended pregnancies are more likely to be terminated than planned pregnancies (Finer and Henshaw 2006). Strict restrictions on legal alcohol availability, on the other hand, should be associated with a decrease in the number of pregnancies, births, and/or abortions. This hypothesis so far has received mixed empirical support. Dee (2001) reports that an increase in the MLDA to 21 years – which represents a decrease in the availability of alcohol – reduces state level birth rates among black 15-19 year-old teens, but does not affect their white peers.

Some studies suggest that risky sexual behavior depends on the intensity of alcohol intake per unit of time and the context in which the alcohol is consumed (O'Hare 2005). For example, moderate alcohol consumption in a bar has different implications than binge drinking at a home party. If more restrictions on alcohol consumption increase the likelihood of more intense drinking in a private setting where intimacy is more likely to occur, then there could be a relatively larger number of unintended pregnancies and abortions in the presence of a high MLDA and a decrease when restrictions are relaxed. These are the opposite effects that many policy-makers might expect.

I test the relationship between the drinking eligibility restrictions and the fertility of young women using exogenous variations in the MLDA across states in the 1970s and 1980s.[2] There are several contributions of my paper to the existing alcohol/teenage fertility literature. Unlike a few related studies that examine the relationship

[1] For more details, see review studies by Wagenaar and Toomey (2002) and Newbury-Birch (2009).
[2] Reduction in the MLDA across country in the early 1970s was alongside a decrease in the voting age; increases in the MLDA in the 1980s were compelled by the federal law requirements.

between alcohol consumption restrictions and fertility of 15-19 year-old teens using state level aggregate data, I use micro-level data from the 1979 cohort of the National Longitudinal Survey of Youth (NLSY). The micro-level panel permits reconciling the timing of pregnancy with the state MLDA and allows me to explore heterogeneous effects by individual characteristics. Such precision is not possible with annually aggregated fertility data. Secondly, in contrast to the literature focused on births or abortions, I model the effect on the probability of first pregnancy and the probability of termination of that pregnancy. The focus on first incidence is novel and reasonable, given that the target group of MLDA is teens and most of them who get pregnant experience this as a first time event. Thirdly, in separate analyses, I use two measures of the legal access to alcohol (the state's MLDA and an individual's legal eligibility for alcohol consumption/possession), which allows me to assess both the sensitivity and the consistency of results across the two measures. Finally, I analyze the effect on different subpopulation-age groups which in some instances is not feasible with aggregate data.

Following the existing literature, I focus on the MLDA restrictions for beer, as it is the most popular alcoholic beverage among youth (Coate and Grossman 1988).[3] Prior to 1988, the MLDA restrictions were regulated at the state-level, with the lowest age requirement set at 18 and the highest at 21 years of age. Between 1970 and 1975, the number of states with a MLDA below 21 years increased from 18 to 39 states. However, under the threat of losing Federal highway funding, by July 1988 the MLDA was raised and set uniformly across the United States to 21. The changes in the MLDA for beer by state from 1970 to 1990 are summarized in supplementary Appendix A. Additionally, in 1973, after the Supreme Court ruling in *Roe v. Wade,* abortions became legal on the national level, significantly reducing the cost of unintended pregnancy for women nationwide.[4]

The empirical evaluation relies on a discrete-time hazard model using the micro-level monthly fertility data. I assess the effect of the change (decrease or increase) in the MLDA and the effect of becoming legally eligible to drink on the probability of becoming pregnant for the first time separately for 15-20, 15-17, 18-20, and 21-23 year-old white, black, and Hispanic women. Given the information on how the first pregnancy ended, the age at pregnancy, and the MLDA restrictions in the state of residence at the time of pregnancy conception, I evaluate the effect of changes in the legal access to alcohol on the probability of first pregnancy ending in an abortion.

[3] If binge drinking is driven by non-beer alcoholic beverages then the MLDA for beer might not be the best measure. For the incidence of first pregnancy, the analysis of the MLDA for liquor yields, generally, qualitatively similar results (some estimates for black women become statistically insignificant).
[4] The abortion laws are discussed in Gold (2003).

Identifying possible effects of the changes in the MLDA is of high priority in light of recent debate regarding the effects of the MLDA.[5]

The results indicate that changes in alcohol consumption restrictions have heterogeneous effects across different population subgroups. The statistical power of the estimates also varies across the considered measures of alcohol consumption restrictions: (i) the MLDA in the state of residence, which is a relatively parsimonious proxy measure of access to alcohol, typically used in the literature due to the aggregated level of the data used, and (ii) a woman's legal right to drink alcohol given both the state's MLDA and that woman's age, a relatively more precise measure of the variable of interest.

I find that for some women the less restrictive access to alcohol reduces the probability of unwanted pregnancy. Specifically, compared to their non-eligible peers, non-poor white women who can drink legally have lower probability of becoming pregnant for the first time and also lower probability of terminating that pregnancy, indicating that there are fewer unwanted pregnancies among women who can drink legally. (A decrease in the MLDA below 21 has a similar negative effect on both probabilities, albeit the estimates are less precise). Relying on the NLSY questions on the frequency of going to bars among woman who have consumed alcohol, I provide supportive evidence that is consistent with a proposition that for this population subgroup reaching the legal age for alcohol consumption opens an alternative less risky venue for alcohol consumption (i.e. drinking at a bar) to drinking at someone's house.[6]

The results for poor-whites, blacks, and Hispanics are mixed, making not only the interpretation, but also the identification of potential channels challenging.[7] Using a generic measure of access – the state's MLDA – I find that a decrease in the MLDA from 21 to 18, 19, or 20 years increases the probability of becoming pregnant for the first time among black and poor white women and increases the probability of termination of the first pregnancy. This might be consistent with a conventional expectation of an increase in the incidence of risk taking under the influence of alcohol leading to negative events (e.g., unwanted pregnancies) in states with the MLDA below 21. However, for these population groups, these conclusions do not hold any longer once the alcohol restrictions measure is constructed more precisely based on both the MLDA and the woman's age.

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[5] In recent years, the Amethyst Initiative, a public movement calling for a re-examination of the MLDA of 21, was supported by the leadership on many campuses.

[6] Wechsler et al. (2000) also reports some evidence in favor of this hypothesis that underage students tend to drink less often, yet consume more drinks per occasion; they are also more likely to drink in private settings.

[7] The Hispanics sample is the smallest, yielding the least reliable estimates particularly in abortion models, which additionally might suffer from the reporting bias.

Overall, the heterogeneity of results indicates that both the effectiveness of restrictions and the mechanism that links alcohol consumption patterns and risky sexual behavior differ across subgroups of women. The sensitivity of results to the choice of the measures of alcohol consumption restrictions could mean that the MLDA alone might not be sufficient to capture alcohol consumption behavior and one should account for the location and legality of alcohol consumption as well.

2. LITERATURE REVIEW

Teen demand for alcohol is relatively responsive to changes in alcohol consumption restrictions. Most studies conclude that a decrease in restrictions on alcohol consumption including changes in the MLDA, as well as a decrease in prices and taxation that affects prices of alcoholic beverage, lead to an increase in teen alcohol consumption (Grossman et al. 1994, Coate and Grossman 1988, Dee and Evans 2003, Markowitz and Tauras 2009).[8]

Despite a positive association between alcohol use and risky sexual behavior among teenagers reported in numerous studies, the causal nature of the relationship remains unknown.[9] Results drawn primarily from aggregate data can only assume that occasions of alcohol use and risky sex coincide; event specific studies, in contrast, provide some evidence in favor of an association for young heterosexuals (Donovan and McEwan 1995). Some studies report that alcohol use may lower contraception use among sexually active teens, and, therefore, increase the probability of an unplanned pregnancies (e.g., Grossman and Markowitz 2005; Markowitz et al. 2005; Hingson et al. 1990, and Rees et al. 2001).

A few recent studies based on aggregate data have emphasized the relationship between alcohol consumption restrictions and teen fertility. Using a "difference-in-difference-in-difference" model, Dee (2001) finds that the nationwide increases in the MLDA to 21 reduced the birth rate among black 15-19 year-old teens by roughly 5.5 percent; the effect on white teens is mostly statistically insignificant and "implausibly" signed. Sen (2003) investigates the effects of beer taxes and other alcohol-related policies, including the MLDA, on teen abortions and births. The MLDA does not appear to have a robust, statistically significant impact on either outcome. One should be cautious with the interpretation of these results, as Sen's study relies on four years of data (1985, 1988, 1992, and 1996), and only the period from 1985 to 1988 involves variation in the MLDA.

[8] For exceptions see Dee (1999) and Kaestner (2000). The literature review on the topic is in Wagenaar et al. (2009) and on the effects of MLDA - in Wagenaar and Toomey (2002).
[9] See Leigh and Stall (1993) and Rashad and Kaestner (2004) for a literature review.

Both Dee (2001) and Sen (2003) use state-level fertility data. The main disadvantage of this approach in studying the effect of MLDA on teen fertility is that it does not permit a thorough analysis of the effects of external factors at the time of pregnancy on individual decision making. Results presented by Dee (2001) only partially support the hypothesis that a low MLDA leads to higher childbearing rates among teenagers and indicate that the response differs across races. Additionally, aggregate data on abortions and pregnancies might not be informative for this kind of analysis for three reasons. First, abortion data are not available by state, year, age group, and race. Therefore, unlike Dee (2001), one neither can construct abortion and pregnancy rates by race nor estimate separate models for each race. Second, a lack of uniform reporting requirements across states introduces a substantial number of missing values in the abortion data collected by the Center for Disease Control and Prevention (CDC). This affects the quality of pregnancy rates, calculation of which relies on abortion data. Third, CDC's abortion data reflect abortions by the state of occurrence and not by the state of residence. The latter is more preferred if one wants to study the effect of changes in the state's law on the rates in the same state.

3. EMPIRICAL ANALYSIS

3.1 Data

I use the 1979 cohort of the NLSY that consists of a nationally representative random sample of young men and women who were 14 to 22 years old in 1979 and oversamples of young blacks, Hispanics, poor whites, and members of the military. The use of NLSY is advantageous as it contains detailed retrospective fertility and mobility histories, which permit tracking the timing of the first pregnancy and its outcome (birth, abortion, miscarriage/stillbirth) in the 1970s-1980s for 6,283 women and identifying the state of residency and the state MLDA for each woman at the time of that pregnancy.[10]

The identification of pregnancy incidence relies on the reported number of pregnancies and their outcomes. Jones and Darroch Forrest (1992) note that abortions are underreported in the NLSY data, especially in the earlier survey years. Results in Udry et al. (1996) suggest that underreporting of abortion is higher among nonwhite women. I address this potential issue by estimating models separately for non-black, non-Hispanic women (whites), non-Hispanic blacks (blacks), and Hispanics.

To pinpoint the timing of events and make my analysis as precise as possible, I convert the NLSY data set into a panel where a unit of observation is a person-month. Each woman enters the data set in the month when she turns

[10] The National Survey of Family Growth (NSFG) contains no data on the retrospective mobility history. Therefore, the timing of past pregnancies cannot be matched to the state of residence or the state policy.

15 years old. For every month after the entry, I know whether she became pregnant or not, her state of residence, and the MLDA in that state.[11] Once she turns 21 years old she exits the data set, as past this point drinking age restrictions are not binding.[12] Further restrictions include exclusion of women with incomplete fertility history, women serving in military, and women who became pregnant before their 15th birthday. The final sample includes 399,528 monthly observations during the period 1972-1985 on 5,549 women. About 60% of my sample is white women, one-quarter is black, and the rest are Hispanics.

The person-month data set can be viewed as transition data where women can move from one state (being not pregnant) to another (becoming pregnant). Table 1 presents a narrative history of the pregnancy occurrence and its outcome for women age 15-20 and for comparison for women age 21-23 described in terms of hazard and survival functions. The examination of survival probabilities reveals that 95% of 5,549 women at risk of first pregnancy did not become pregnant at age 15, 80% – did not experience a first pregnancy by their 18th birthday, and slightly more than half of the sample did not get pregnant by their 21st birthday. About 75% of all first pregnancies that occurred to 15-20 year-old women ended in a live birth and 16% were terminated.[13]

The race specific incidence shows that 54% of the black sample became pregnant for the first time between ages 15 to 20 years; the pregnancy occurrence among poor whites and Hispanics was 47% compared to 32% for women in the non-poor white sample. The inspection of composition of pregnancies aggregated by race, age, and the MLDA, reported in supplementary Appendix C, reveals that the majority of all first time pregnant 15-17 year-olds were black women; among all 18-20 year-olds – non-poor white women. The share of pregnancies among poor whites and Hispanics are rather stable across age groups. An additional stratification by the MLDA shows that, with an exception of Hispanics, a disproportionally higher share of first pregnancies occurs in states where the MLDA is set below 21. These states have a higher share of abortions, but only among poor whites and Hispanics.

3.2 Model Specification: Discrete-Time Hazard

The fertility history data identify the month and year when first pregnancy occurs. This suggests grouping observations into discrete (monthly) time intervals. I use the discrete-time hazard model which accommodates the nature of the observed data and the research question.

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[11] The timing of first pregnancy is identified by month and year of conception variables created by the NLSY.
[12] For a falsification test, an extended data set includes 21-23 year olds. As expected, changes in the MLDA do not affect first childbearing of 21-23 year-old women (supplementary Appendix D, Table D-1).
[13] The comparison of birth and abortion ratios across NLSY, NSFG, and the national statistics is in supplementary Appendix B.

Becoming pregnant for a first time, at time t is a non-repeatable event which is intrinsically conditional on not experiencing the event at any time prior to t. Therefore, for each woman the pregnancy status can be represented as a string of zeros (indicating not pregnant) followed by a one (indicating first pregnancy). If a woman did not have the first pregnancy prior to age 21 then the dependent variable is a string of zeros implying that the event has yet to be experienced. The binary nature of the dependent variable and the specification of the log-likelihood function allow one to model the hazard probability as a probit function.

The presence of several age cohorts in my data set determines two notions of time: calendar time and age-time. For each woman I observe the age when she had her first pregnancy which also corresponds to a certain calendar time t. I normalize age by expressing it in terms of months since birth minus 180 so it corresponds to months since age 15. To accommodate both notions of time, in addition to the index for calendar time t, I introduce the age counter τ that represents the time that a woman spends at risk of first pregnancy measured in months. The function $g(\tau)$ is parameterized as a cubic polynomial and it captures the effect of age (and the duration dependency) on the hazard probability after accounting for other covariates. The effect of calendar time is captured by a full set of calendar time fixed effects.

Empirically the discrete-time (monthly) hazard probability of first pregnancy is given by

$$\Pr[1^{st}\_pregnancy_{ist} = 1 | \text{ not pregnant before t}, X_{ist}] =$$

$$\Phi(\beta_0 + \delta\ \mathbf{eligibility\_restriction_{ist}} + \beta' X_{ist} + \theta_1 age_{it} + \theta_2 age_{it}^2 + \theta_3 age_{it}^3 + \gamma_s + \eta_t), \qquad (1)$$

where *1^{st}_pregnancy* is a binary indicator of the pregnancy status, Φ – the standard cumulative normal distribution function, and i indexes individuals, s – state of residence, t – calendar time that corresponds to a combination of month and year and ranges from April 1972 to September 1985. For estimation purpose, η_t corresponds to a set of calendar year and calendar month dummies. The latter is included to allow for a seasonality effect. Standard errors are clustered by state.

I use two measures of eligibility restrictions (two separate models) to capture the effect of the MLDA on teen fertility: a dummy indicating whether the MLDA in a state of residence is set below age 21 and an indicator of whether a teen can legally drink in the state of residence, given her age and the MLDA in her state of residence. The former variable is consistent with the literature; the latter variable is more appropriate for the micro-level data as it is adjusted for the "grandfather clause," a provision that exempts teens who were previously eligible to drink from new eligibility requirements, and, therefore, provides a more precise measure of person-specific eligibility. The differences between the two measures are substantial, especially among women age 15-20 as the underage women (i.e. 15-17) can reside in both states with the MLDA below 21 and the MLDA 21, but they are always "not eligible". Table 2 provides the descriptive statistics for variables included in the model. Over 70% of women age 15-20 in the data set were residing in states with the MLDA below 21, but only one third of them can drink legally. Although both measures indicate an "easier access" to alcohol, the incidence of first pregnancy among eligible women is less frequent than among women facing the MLDA below 21. These differences might result in the different estimates (including differences in the statistical power) for the effect of the policy change on the probability of the first pregnancy.

The vector *X* included in (1) contains controls for individual and family characteristics: race, religion in which a female was raised, the Armed Forces Qualifications Test (AFQT) scores, mother's education, and indicators for the marital status and for presence of both parents in the household at age 14.[14] An addition of contraceptive use variable yields mostly qualitatively similar estimates.[15]

As mentioned earlier, I estimate separate equations for white, black, and Hispanic 15-20 year-old women. Additionally, whites are disaggregated into poor and non-poor white subsamples, where poor subsample consists of the supplemental poor white and the cross-sectional poor white samples as identified by the respondent's sample identification code. Next, alcohol consumption patterns among 15-17 year olds might differ from patterns among 18-20 year olds, who are directly affected by the MLDA variations. The access to alcohol for those under 18 is illegal under any MLDA regime, but they can gain access through older "legal" peers, successful use of fake IDs or the lack of the law enforcement.[16] To take this into account, I estimate models by race separately for 15-17 and 18-20 year olds. I test the credibility of the model by repeating the estimation for women age 21-23. If the model is correct, then eligibility restrictions should not affect the dependent variable for this age group, as it is not subject to the MLDA restrictions. Finally, Fertig and Watson (2009) suggest that the MLDA restrictions might affect the composition of the pool of women who become pregnant. Since the outcome of the first pregnancy and the alcohol

[14] A father's presence, as well as the time that parents spend with their children can affect risky behaviors (e.g., See (2014) finds a decrease in teen risky behaviors due to parental time supervision). However, comprehensive intertemporal data on the household composition and time allocation is not available.
[15] Delaney et al (2013) report a strong association between alcohol consumed by students and drinking by their older siblings. The pregnancy and abortion results are robust to the inclusion of an indicator for the presence of older siblings.
[16] Wagenaar and Tomey (2002) conclude that 44%-97% of outlets in purchase-attempt studies sold alcohol to the underage without a request of age identification. Similarly, Wagenaar and Wolfson (1994) find that the law enforcement actions against outlets selling to minors are also rare.

consumption eligibility restrictions in a state at the time of conception of that pregnancy are known, I explore the compositional aspect by estimating the probability of abortion (relative to a live birth) among women age 15-20 who are pregnant for the first time.[17] This analysis is more exploratory, as abortion data are rather scarce. These models are estimated as the conditional on pregnancy probability models using a specification similar to (1).[18] Calendar month dummies in these specifications are omitted; calendar year and state fixed effects are included.

Empirical results are discussed in the next section. The discussion is focused on the "average marginal effects" calculated using a discrete first-difference approach. Probit estimates for all considered age groups are reported in supplementary Appendix D. The effects of personal characteristics on studied probabilities have generally expected signs, but as they are of secondary interest, the discussion is omitted. All results presented here are robust to inclusion of controls for other alcohol consumption policy variables such as the state beer tax, the administrative per se license suspension law, and the illegal per se law (for the majority of states, the latter corresponds to the blood alcohol content 10% or more), and age-specific linear trends.[19]

4. RESULTS

The discrete change method refers to a change in the individual predicted probability of the outcome event due to a change in the dichotomous explanatory variable (Long 1997). The effect of a decrease in the MLDA for a woman *i* in a given subpopulation-age group (Δ_i), equals the difference between the predicted probability of first pregnancy while in a state with the MLDA below 21 years and the predicted probability of first pregnancy while in a state with the MLDA of 21:

$$\Delta_i = \text{predicted Pr}[\text{1st\_pregnancy}_{ist} = 1 \mid X_{ist,}\ \&\ \text{MLDA} = 18, 19 \text{ or } 20] - \text{predicted Pr}[\text{1st\_pregnancy}_{ist} = 1 \mid X_{ist,}\ \&\ \text{MLDA} = 21] \quad (2)$$

[17] Further disaggregation into 18-20 and 15-17 age groups substantially reduces sample sizes and for the latter group produces noisy point estimates that lack credibility.
[18] A small number of miscarriages/stillbirths in the data set is insufficient to estimate the multinomial probability model. Instead, I estimate a probit model for abortion using a sample of pregnancies ending only in live births and abortions.
[19] Due to sample sizes, the inclusion of the state-specific linear trends is feasible only in the pregnancy model. The resulting estimates of discrete changes maintain the sign, but generally have larger standard errors. The sensitivity of results varies across the two measures of the policy change. For example, specifications with the "legally eligible" dummy produce qualitatively similar results; specifications with a noisier measure (the MLDA dummy) produce mixed results.

Then the effect is averaged across observations in a given subpopulation-age group: $\overline{\Delta} = \sum_{i=1}^{N_R} \Delta_i / N_R$, where N_R is the number of women in a subpopulation-age group R. To identify the effect of an increase in the MLDA one should reverse the sign of Δ_i. In a similar manner, I calculate the average change in the predicted probability of first pregnancy due to becoming legally eligible to drink. The robust standard errors for the average effects are obtained using the delta-method.

The change in the predicted probability of first pregnancy due to a decrease in the MLDA by subpopulation-age group is reported in Table 3. 15-20 year-old black and poor white women tend to have lower probabilities of becoming pregnant for the first time when the MLDA is 21. A decrease in MLDA to 18 or 19 or 20 years significantly *increases* average monthly probability of first pregnancy by 0.6 percentage points among poor whites. The increase in the probability for blacks is smaller (0.3 percentage points). These are substantial effects given that the base monthly probabilities for this age group are 0.5% and 0.8%, respectively. In contrast, Hispanics tend to have higher probability of becoming pregnant for the first time when the MLDA is 21 and lowering of the MLDA is associated with a 1.0 percentage point *reduction* in the probability. The estimated effect of the MLDA on pregnancy probabilities among non-poor whites is also negative, but not statistically significant at the conventional confidence level.

The disaggregation of 15-20 year old sample into 15-17 and 18-20 age groups yields similar results in terms of the direction of the effect, but with more variation in the statistical power of the estimates. As expected, given the age groups subject to the alcohol restrictions, the results for the 18-20 age group are generally larger, and the results for the 15-17 age group are generally statistically insignificant (the latter are reported in supplementary Appendix D, Table D-2). For black women, a decrease in the MLDA results in a marginally statistically significant increase of 0.4 percentage points in the probability of first pregnancy for age 15-17, and 0.2 percentage points statistically insignificant increase for 18-20.[20] This indicates that for black women, models with the MLDA proxy do not capture well the policy of interest.

The effects of MLDA on pregnancy resolution are reported in Table 4. A low MLDA at the time of first pregnancy has a negative but statistically insignificant effect on that pregnancy's outcome for 15-20 year-old non-

[20] In other words, the policy's direct effect on black women age 18-20 is smaller and statistically insignificant than the potential indirect effect of the policy on women age 15-17, which is not only larger but also statistically significant.

poor white women. For poor white and Hispanic women, a decrease in the MLDA substantially increases the probability of abortion (a change of 28.1 and 24.7 percentage points respectively), and these effects are statistically significant.[21] A similar positive, but numerically smaller effect (7.5 percentage points) is observed for the first time pregnant black women.

The results described above do hint that the MLDA below 21 years tend to be associated with an increase in the number of unwanted pregnancies (as revealed by the increased incidence of abortions or alternatively the reduced incidence of live births) among some groups of women. However, as discussed in the previous section, being in a state with the MLDA below age 21 is a relatively noisy measure of access to alcohol consumption. Carpenter and Dobkin (2009) find a large and immediate increase in drinking among young adults after becoming legally eligible to drink. Therefore, the results from a specification that includes an indicator for whether a woman can legally drink, which are reported in Table 5, are more informative.

For non-poor white women ages 15-20 and 18-20, being legally eligible to drink decreases the probability of becoming pregnant for the first time and also decreases the probability of terminating that pregnancy (the largest samples available for the analysis). As expected, the effects are larger for the 18-20 age group. For poor whites, the eligibility status does not appear to have a statistically significant effect on either the incidence of first pregnancy or its outcome. The estimated average marginal effect for the abortion model has substantial standard errors (particularly for the 18-20 age group), but a low precision is expected given the sample size. The results for blacks are mixed: the legal access to alcohol reduces the probability of becoming pregnant for the first time and increases the probability of abortion. However, for the latter I fail to reject the null of zero effect. Results for Hispanics are not reported, as point estimates are excessively large and, therefore, not reliable, especially in the 18-20 sample.

The differences in results across the two measures of the access lead to several conclusions. First, given that the two measures produce quantitatively and qualitatively different results, the MLDA measure should be used cautiously, and a more precise measure that identifies the individual legal ability to consume alcohol should be used instead. Second, an increase in the alcohol access via a change in the legal eligibility status decreases the probability of unwanted pregnancy among non-poor women ages 15-20 and 18-20. For this population subgroup, the additional reassuring factor is the consistency in the statistical power of the estimates, across age groups for a given measure of

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[21] The result for Hispanics is peculiar, as a decrease in the MLDA reduces the probability of pregnancy and increases that of abortion. A potential explanation is that the latter might suffer from a reporting bias. This will be the case, if the underreporting of abortions, which generally is observed among Hispanics, is systematic in the states with the more stringent laws (i.e. MLDA is 21), compared to the states with a more liberal set of laws.

the policy variable in both pregnancy and abortion models. Third, the differences in results between non-poor and poor whites indicate that the effect is heterogeneous by income. Similar differences across income groups for black and Hispanic women, if present, could explain the results.[22] Aggregation of poor and non-poor women in one group might affect the quality of inference when such differences are substantial. For example, the analysis of a combined poor and non-poor white sample yields smaller estimates for the average effects of becoming legally eligible to drink that also generally have larger estimated p-values (the estimate for 18-20 age group in pregnancy model even becomes statistically insignificant).

In addition to a potential noisy MLDA measure, another explanation for different results across different policy measures and population groups is that the alcohol use patterns and the factors that drive them might differ substantially across population subgroups. To test for this, I analyze the probability of bar attendance among 18-20 year-old women. The analysis relies on the alcohol use location data from the 1982-85 NLSY surveys.[23] The probability of going to bars is modeled as a function of the eligibility restrictions, personal and family characteristics similar to equation (1), and state, calendar year and month fixed effects. Table 6 shows that changes in the alcohol availability have heterogeneous effects on the probability of going to bars across population groups; the estimates, to some extent, are also sensitive to the choice of the eligibility restriction. Specifically, the MLDA below 21 is associated with an increase in the reported bar attendance among non-poor whites and Hispanics, but it *decreases* attendance among poor white and black women. Becoming legally eligible to drink only statistically significantly increases the probability of going to bars among non-poor whites. Although the statistical power of estimates for other population groups is low, the estimates for poor whites and Hispanics maintain consistency in terms of the sign. This exercise does not directly establish a link between the legal access and the alcohol use venue, but assuming that one of the reasons people go to bars is to drink alcohol these results are useful. If an easy access to alcohol increases bar attendance among non-poor whites (as shown by both measures) then it is reasonable to suggest that the legal access increases alcohol use in bars for that group, supporting the location hypothesis that predicts a decrease in unwanted pregnancies due to a shift in the drinking venue from house parties to bars. In contrast, for black and poor white women, I do not find statistical evidence of such a link. It is possible that for these

[22] The NLSY does not distinguish between poor and non-poor blacks or Hispanics.
[23] These questions were not asked prior to 1982. The thorough analysis of bar attendance among the underage is not feasible due to the aging of cohorts (the youngest cohort was 17 in 1982). Supplementary appendix E shows alcohol use and bar attendance trends. The trends confirm that drinking varies across races with the highest share among non-poor white women. Not surprisingly, among those ages 18-20 who report drinking, the percentage of women going to bars is substantially higher among those legally eligible to drink.

two groups the changes in the hazard and the resolution of the first pregnancy due to a change in the MLDA might reflect the effect of other factors, which are correlated with the MLDA but are not included in the model.

Finally, results presented here on the effects of MLDA cannot be easily compared to those reported in Dee (2001), since I study the hazard of first pregnancy and its outcome using individual level data and Dee studies state level birth rates that are not limited to the first time pregnant women. Nonetheless, similar to Dee, I do not find a statistical association between changes in the MLDA and the fertility of some white women.

5. DISCUSSION

The majority of teens who get pregnant experience it as a first time event. Given that teen childbearing is associated with high social costs, an effective public policy that could reduce some of these costs requires a detailed understanding of the factors that affect the incidence of pregnancy among teens. This study uses a reduced-form evaluation that relies on individual level data from the NLSY to test whether increasing alcohol availability (measured in terms of a decrease in the MLDA below 21 years and in terms of becoming legally eligible to drink alcohol) leads to a change in teen pregnancy rate or pregnancy outcome. The results indicate that easier access to alcohol has heterogeneous effects on different population subgroups. In addition, these effects are sensitive to the alcohol access measure used in the estimation.

For non-poor white less restrictive legal access to alcohol decreases the probability of first pregnancy and decreases the probability of first pregnancy abortion, indicating that when there is relatively easier access to alcohol there are fewer unwanted pregnancies. While changes in the MLDA change the cost of alcohol consumption, they could also change the context and intensity of alcohol intake. Relatively high MLDA could induce binge-drinking, and could increase drinking in private gatherings (relative to drinking in a public location), where risky sexual behavior (including unprotected sex) is more likely to occur. The results for non-poor whites are consistent with a mechanism where alcohol restrictions are perhaps not necessarily too effective in preventing non-poor white women from drinking, and less restrictive legal alcohol access results in a change in the location of alcohol consumption from a private to a public setting. In other words, the change in the access to alcohol rules changes the cost of the alcohol consumption in a public location relatively more than in a private location. The supplementary analysis of the bar attendance data provides reassuring evidence of such channel and confirms that, for this population subgroup, an increase in the legal access to alcohol increases alcohol use in bars, which could explain the observed decrease in unwanted pregnancies. For the first pregnancy model (which relies on a large sample) either measure of alcohol restrictions produces estimates that have the same sign and similar magnitude. However, the MLDA measure is noisy, which is reflected in the statistical power of the estimates. For the abortion model, the MLDA measure produces results that are both noisy and have a smaller magnitude, which suggests that the MLDA might not be a good proxy.

For poor white and black women the results are mixed. Taken in isolation, a decrease in the MLDA from 21 years to 18, 19, or 20 years increases the probability of first pregnancy and also has a positive effect on the probability of termination of that pregnancy among black and poor white women. This is consistent with a mechanism where lower MLDA reduces the cost of alcohol consumption, without a significant substitution between drinking at a private and at a public location. If this is the case, then some of the unintended negative consequences of changes in the MLDA in these sub-populations could perhaps be mitigated by educating young adults about the potential effects of excessive alcohol consumption. However, the conclusions based on the MLDA measure alone are premature, since the results are not robust when a less noisy measure of restrictions that accounts for the MLDA and a woman's age is used. Given that the results differ across considered fertility outcomes, measures of eligibility restrictions, and population subgroups, more research is needed to identify all mechanisms through which alcohol consumption restrictions affect teen childbearing rates among poor white, black, and Hispanic women.

ACKNOWLEDGMENTS

I sincerely thank Thomas A. Mroz, Paul W. Wilson, Sumner La Croix, Sang-Hyop Lee, Alan Barreca, Michael Sinkey, and Petru S. Stoianovici for their advice and comments.

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Table 1 – Timing of the first pregnancy and its outcome aggregated by years of age for 15-20 and 21-23 year-old women

Age in years	Teens at risk	Pregnant for the 1st time	Pregnancy ended in: a live birth	Pregnancy ended in: an abortion	Hazard probability of the 1st pregnancy	Survival probability of the 1st pregnancy
15	5,549	257	201	44	0.05 (0.00)	0.95 (0.00)
16	5,292	406	324	57	0.08 (0.00)	0.88 (0.00)
17	4,886	438	326	75	0.09 (0.00)	0.80 (0.01)
18	4,448	473	346	72	0.11 (0.00)	0.72 (0.01)
19	3,975	459	347	71	0.12 (0.01)	0.63 (0.01)
20	3,516	329	230	52	0.09 (0.00)	0.57 (0.01)
Women for whom the MLDA is not binding anymore						
21	3,187	314	234	40	0.10 (0.01)	0.52 (0.01)
22	2,873	276	223	35	0.10 (0.01)	0.47 (0.01)
23	2,597	251	185	31	0.10 (0.01)	0.42 (0.01)

Note: Standard errors are in parentheses.

Table 2 – Descriptive statistics for 15-20 year-old women, 1972-1985

Variable	Mean	SD	Observations	
			person-month	person
Time variant variables				
Legally eligible to drink (adjusted for the grandfather clause)	0.32	0.47	395,036	5,544
MLDA is 18, 19 or 20 years	0.72	0.45	395,036	5,544
Currently married	0.11	0.31	399,528	5,549
Used contraception before 1st pregnancy	0.44	0.50	275,904	3,832
Time invariant variables				
Non-poor white	0.40	0.49	399,528	5,549
Poor white	0.19	0.39	399,528	5,549
Black	0.25	0.43	399,528	5,549
Hispanic	0.17	0.37	399,528	5,549
Raised in a Baptist family	0.29	0.45	398,376	5,533
Raised in other religion	0.33	0.47	398,376	5,533
Raised as an Atheist	0.03	0.18	398,376	5,533
Raised in a Catholic family	0.35	0.48	398,376	5,533
AFQT score below the mean	0.55	0.50	381,600	5,300
Two-parent household at age 14	0.68	0.47	398,880	5,540
Mother's education (years)	10.77	3.19	378,288	5,254

Note: Data sources for the MLDA: Wagenaar (1981), O'Malley and Wagenaar (1990), and the National Highway Traffic Safety Administration website. If a person resides outside of the U.S. then for the corresponding time periods missing values are assigned for both alcohol consumption policy restrictions and state dummies.

Table 3 – The average predicted probability of first pregnancy and the average effect of a **decrease** in the MLDA, 15-20 and 18-20 year-old women

	15-20 age group				**18-20 age group**			
	Non-poor white	**Poor white**	**Black**	**Hispanic**	**Non-poor white**	**Poor white**	**Black**	**Hispanic**
Prob. of *1st pregnancy* if MLDA=**21**	0.006	0.005	0.008	0.015	0.009	0.003	0.010	0.047
Prob. of *1st pregnancy* if MLDA<**21**	0.005	0.011	0.011	0.006	0.006	0.018	0.012	0.006
Discrete change due to a decrease	**-0.001**	**0.006****	**0.003***	**-0.010***	**-0.003**	**0.015****	**0.002**	**-0.040**$^{+}$
in the MLDA ($\bar{\Delta}$)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.02)
Number of person-month observations	125,341	51,652	63,846	44,715	56,400	21,550	25,614	18,347
Number of persons	2,020	935	1,221	809	1,782	712	868	640

Note: ** significant at 1%; * at 5%; $^{+}$ at 10%. Robust standard errors adjusted for state level clusters are in parentheses. Standard errors for the monthly average predicted probabilities are available upon request. All regressions include controls for religion in which a female was raised, the AFQT scores, mother's education, whether both parents were present in the household at age 14, an indicator for being currently married, a cubic polynomial for age as well as state, calendar year, and calendar month fixed effects. Results are generally qualitatively robust to inclusion of the use of contraception before the first pregnancy (exception 18-20 Hispanics, for whom a marginally statistically significant estimate becomes insignificant). Changes in the MLDA do not appear to affect first childbearing of 21-23 year-old women: for all considered population groups I fail to reject the null of zero effect.

Table 4 –The average conditional probability of abortion and the average effect of a **decrease** in the MLDA, 15-20 year-old women

	15-20 age group			
	Non-poor white	**Poor white**	**Black**	**Hispanic**
Cond. probability of *abortion* if MLDA=**21**	0.361	0.035	0.094	0.098
Cond. probability of *abortion* if MLDA<**21**	0.276	0.316	0.169	0.345
Discrete change due to a	**-0.085**	**0.281****	**0.075***	**0.247***
decrease in the MLDA ($\overline{\Delta}$ **)**	(0.08)	(0.06)	(0.03)	(0.11)
Sample size	541	302	393	274

Note: ** significant at 1%; * at 5%; $^{+}$ at 10%. Robust standard errors adjusted for state level clusters are in parentheses. Standard errors for the conditional monthly average predicted probabilities are available upon request. All samples are limited to pregnancies ending in live births or abortions. All regressions include controls for religion in which a female was raised, the AFQT scores, mother's education, whether both parents were present in the household at age 14, an indicator for being currently married, a cubic polynomial for age as well as state and calendar year fixed effects. The results are robust to inclusion of the use of contraception use variable. With an exception of poor white sample for which a discrete change cannot be established, the point estimates for age group 18-20 are generally noisier, although qualitatively similar to the ones reported here.

Table 5 – The average effect of **becoming legally eligible** to drink on the probability of first pregnancy and the probability of abortion, 15-20 and 18-20 year-old women

	15-20 age group					
	1st pregnancy			***Abortion***		
	Non-poor white	**Poor white**	**Black**	**Non-poor white**	**Poor white**	**Black**
Av. probability of *outcome* if **cannot legally drink**	0.006	0.008	0.011	0.337	0.229	0.152
Av. probability of *outcome* if **can legally drink**	0.004	0.009	0.009	0.233	0.143	0.154
Discrete change due to becoming	**-0.001****	**0.000**	**-0.002**$^{+}$	**-0.105***	**-0.087**	**0.002**
legally eligible to drink ($\bar{\Delta}$ **)**	(0.00)	(0.00)	(0.00)	(0.05)	(0.07)	(0.08)
Sample size	125,341	51,652	63,846	541	302	393

	18-20 age group					
	1st pregnancy			***Abortion***		
	Non-poor white	**Poor white**	**Black**	**Non-poor white**	**Poor white**	**Black**
Av. probability of *outcome* if **cannot legally drink**	0.009	0.008	0.014	0.448	0.218	0.104
Av. probability of *outcome* if **can legally drink**	0.006	0.011	0.011	0.194	0.285	0.214
Discrete change due to becoming	**-0.003****	**0.003**	**-0.004***	**-0.254****	**0.066**	**0.110**
legally eligible to drink ($\bar{\Delta}$ **)**	(0.00)	(0.00)	(0.00)	(0.08)	(0.25)	(0.12)
Sample size	56,400	21,550	25,614	310	84	130

Note: ** significant at 1%; * at 5%; $^{+}$ at 10%. Robust standard errors adjusted for state level clusters are in parentheses. Standard errors for the average predicted probabilities are available upon request. For the abortion model, samples are limited to pregnancies ending in live births or abortions. All regressions include controls as described in notes under Tables 3-4 respectively for the pregnancy model and the abortion model. An inclusion of the contraception use variable yields qualitatively similar results. Exceptions for age 18-20: the estimate for poor whites in the hazard model and the estimate for blacks in the abortion model become larger and statistically significant; for age 15-20: the estimate for blacks in the hazard model becomes statistically insignificant. Results for Hispanics are not reported as some point estimates are prohibitively large and not reliable.

Table 6 – The effects of alcohol consumption restrictions on the probability of going to bars among 18-20 year-old women

	Non-poor white	Poor white	Black	Hispanic
Panel A: MLDA				
Probability of *going to bars* if **MLDA=21**	0.587	0.724	0.727	0.344
Probability of *going to bars* if **MLDA<21**	0.756	0.420	0.406	0.517
Discrete change due to a decrease in the MLDA ($\overline{\Delta}$)	**0.170****	**-0.304****	**-0.321****	**0.173***
	(0.04)	(0.09)	(0.06)	(0.09)
Sample size	1,399	308	426	391
Panel B: Legal eligibility				
Probability of *going to bars* if **cannot legally drink**	0.627	0.550	0.357	0.434
Probability of *going to bars* if **can legally drink**	0.756	0.504	0.498	0.454
Discrete change due to becoming legally eligible to drink ($\overline{\Delta}$)	**0.129***	**-0.046**	**0.140**	**0.020**
	(0.05)	(0.13)	(0.16)	(0.09)
Sample size	1,399	308	426	391

Note: ** significant at 1%; * at 5%; $^{+}$ at 10%. Robust standard errors adjusted for state level clusters are in parentheses. Standard errors for the average predicted probabilities are available upon request. The dependent variable equals 1 if a respondent attended bars in the past month, 0 otherwise; the sample is limited to women who admitted drinking in the past month. All regressions include controls for religion in which a female was raised, the AFQT scores, mother's education, whether both parents were present in the household at age 14, an indicator for being currently married, age, age-squared, and state, calendar year and month fixed effects.

Appendix A: State MLDA for beer, 1970-1988 **(SUPPLEMENTARY CONTENT FOR ONLINE PUBLICATION)**

State	on 1/1/1970	Changes in the MLDA in the period 1/1/1970 –1/1/1988
Alabama	21	7/1/1975 - 19; 10/1/1985 - 21[gc]
Alaska	21	9/1/1970 - 19; 11/1/1984 - 21
Arizona	21	8/1/1972 - 19; 1/1/1985 - 21[gc]
Colorado	18	7/1/1987 - 21
Connecticut	21	10/1/1972 -18; 7/1/1982 -19; 10/1/1983-20; 9/1/1985-21[gc]
Delaware	21	7/1/1972 - 20; 1/1/1984 - 21[gc]
DC	18	9/1/1986 - 21[gc]
Florida	21	7/1/1973 - 18; 10/1/1980 - 19; 7/1/1985 - 21[gc]
Georgia	21	7/1/1972 -18; 9/1/1980 -19; 9/1/1985 -20; 9/1/1986 - 21
Hawaii	20	3/1/1972 - 18; 10/1/1986 - 21
Iowa	21	4/1/1972-19; 7/1/1973-18; 7/1/1976 -19[gc]; 9/1/1986 - 21[gc]
Idaho	20	7/1/1972 - 19; 4/1/1987 - 21
Illinois	21	9/1/1973 - 19; 1/1/1980 - 21
Kansas	18	7/1/1985 - 21[gc]
Louisiana	18	3/1/1987 - 21
Maine	20	6/1/1972 - 18; 10/1/1977 - 20; 7/1/1985 - 21[gc]
Maryland	21	7/1/1974 -18; 7/1/1982 - 21[gc]
Massachusetts	21	3/1/1973 - 18; 4/1/1979 - 20; 6/1/1985 - 21[gc]
Michigan	21	1/1/1972 - 18; 12/1/1978 - 21
Minnesota	21	6/1/1973 - 18; 9/1/1979 - 19[gc]; 9/1/1986 - 21[gc]
Mississippi	18	10/1/1986 - 21
Montana	21	7/1/1971 -19; 7/1/1973 -18; 1/1/1979 -19; 4/1/1987- 21
Nebraska	20	6/1/1972 - 19; 7/1/1980 - 20[gc]; 1/1/1985 - 21[gc]
New Hampshire	21	6/1/1973 - 18; 5/1/1979 - 20; 6/1/1985 - 21[gc]
New Jersey	21	1/1/1973 - 18; 1/1/1980 - 19[gc]; 1/1/1983 - 21[gc]
New York	18	12/1/1982 - 19; 12/1/1985 - 21
North Carolina	18	10/1/1983 - 19; 9/1/1986 - 21
Ohio	18	8/1/1982 - 19; 7/1/1987 - 21[gc]
Oklahoma	18	9/1/1983 - 21
Rhode Island	21	3/1/1972 -18; 7/1/1980 -19; 7/1/1981 -20; 7/1/1984 - 21
South Carolina	18	1/1/1984 - 19; 1/1/1985 - 20; 9/1/1986 - 21
South Dakota	19	7/1/1972 - 18; 7/1/1984 - 19; 4/1/1988 - 21
Tennessee	21	5/1/1971 - 18; 6/1/1979 - 19; 8/1/1984 - 21[gc]
Texas	21	8/1/1973 - 18; 9/1/1981 - 19; 9/1/1986 - 21
Vermont	21	7/1/1971 - 18; 7/1/1986 - 21[gc]
Virginia	21	7/1/1974 - 18; 7/1/1981 - 19; 7/1/1985- 21[gc]
West Virginia	18	7/1/1983 - 19; 7/1/1986 - 21
Wisconsin	18	7/1/1984 - 19; 9/1/1986 - 21[gc]
Wyoming	21	5/1/1973 - 19; 7/1/1988 - 21

Note: The MLDA in Arkansas, California, Indiana, Kentucky, Missouri, Nevada, New Mexico, North Dakota, Oregon, Pennsylvania, Utah, and Washington was 21 years during 1/1/1970-1/1/1988. Source: Wagenaar (1981), O'Malley and Wagenaar (1990), and the National highway traffic safety administration online reports.

[gc] Indicates that the law change includes a "grandfather clause."

Appendix B: Comparison of abortion data across NLSY and NSFG surveys **(SUPPLEMENTARY CONTENT FOR ONLINE PUBLICATION)**

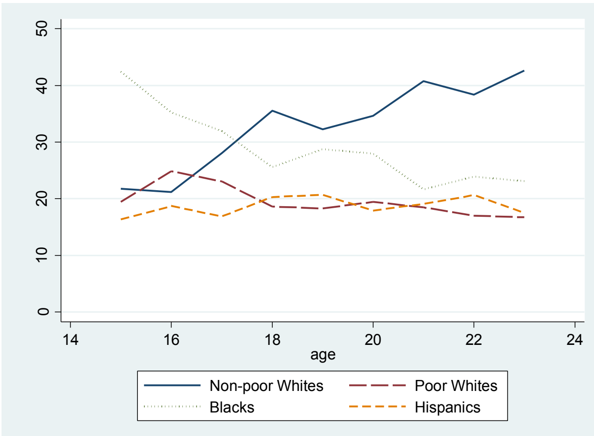
Panel A	**NLSY**					**NSFG [a]**				
Age at 1st pregnancy	Total # of 1st pregn.	1st pregn. ended in: Birth	1st pregn. ended in: Abortion	Birth ratio	Abortion ratio	Total # of 1st pregn.	1st pregn. ended in: Birth	1st pregn. ended in: Abortion	Birth ratio	Abortion ratio
15	257	201	44	78.2	17.1	726	483	164	66.5	22.6
16	406	324	57	79.8	14.0	1041	731	182	70.2	17.5
17	438	326	75	74.4	17.1	1318	934	223	70.9	16.9
18	473	346	72	73.2	15.2	1473	1023	268	69.5	18.2
19	459	347	71	75.6	15.5	1334	957	214	71.7	16.0
20	329	230	52	69.9	15.8	1165	846	149	72.6	12.8
21-23	841	642	106	76.3	12.6	2752	2021	336	73.4	12.2
Average 15-20				**75.2**	**15.8**				**70.2**	**17.3**
Average 15-19				**76.2**	**15.8**				**69.8**	**18.2**
National statistics 15-19 (AGI) [b]				**51.6**	**34.6**				**51.6**	**34.6**

Panel B	**NLSY**			**NSFG [a]**		
	White [c]	*Black*	*Hispanic*	*White*	*Black*	*Hispanic*
Total # 1st pregnancies among 15-20	1183	737	442	3970	2860	227
of which ended in birth (%)	69.1	82.6	78.5	65.8	76.8	73.1
of which ended in abortion (%)	20.5	9.5	13.1	20.6	12.2	14.5
Total # 1st pregnancies among 21-23	488	192	161	1947	687	118
of which ended in birth (%)	73.2	77.6	84.5	73.1	74.2	73.7
of which ended in abortion (%)	15.0	9.9	8.7	12.8	10.5	12.7

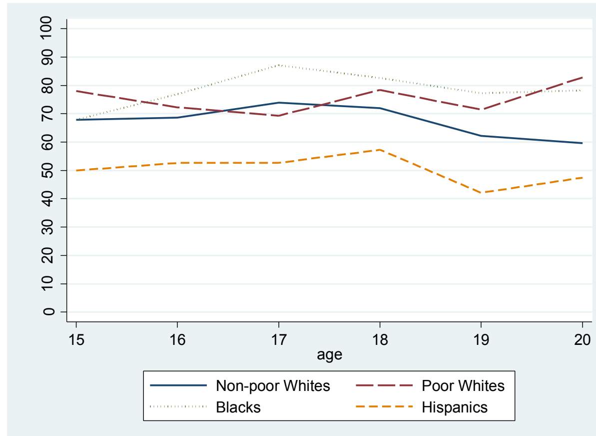
Note: [a] Pooled 1976, 1982, 1988, 1995 years. For comparability, the following restrictions are applied to the NSFG surveys: 1) only first pregnancies and their outcomes are considered (reflects 33%-37% of all pregnancy records); 2) age at conception is 15-23; 3) conception occurred between 1972 and 1988. Due to restrictions 2) and 3), between 40-50% of all first pregnancy records from the NSFG 1982-1995 surveys are used; for the 1976 survey this number is 14%. [b] Averages for 1972-1985 time period. This number is not directly comparable to the corresponding NLSY or NSFG statistics as it reflects the share of *all* terminated pregnancies among women age 15-19, not the share of first pregnancies that are terminated. [c] Includes a poor white sample.

Appendix C: Composition of first pregnancies in the dataset by age, race, and the MLDA (**SUPPLEMENTARY CONTENT FOR ONLINE PUBLICATION**)

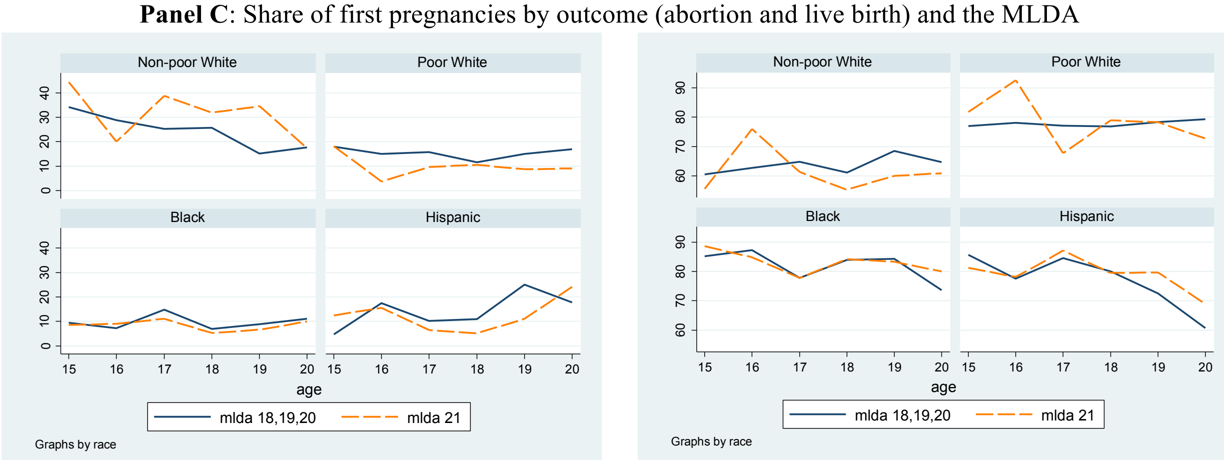
Panel A: Distribution of first pregnancies by age and race



Panel B: Share of first pregnancies occurred in states with the MLDA 18, 19 or 20 years



Panel C: Share of first pregnancies by outcome (abortion and live birth) and the MLDA



Appendix D: Probit estimates **(SUPPLEMENTARY CONTENT FOR ONLINE PUBLICATION)**

Table D-1: Probit coefficient estimates and the average effect of a decrease in the MLDA on the outcome for 15-20 and 21-23 age groups

	15-20 year-old				***21-23 year-old***			
	Non-poor white	**Poor white**	**Black**	**Hispanic**	**Non-poor white**	**Poor white**	**Black**	**Hispanic**
MLDA is 18, 19 or 20	-0.09	0.32^{**}	0.14^{+}	-0.41^{**}	0.08	0.18	-0.12	0.02
	(0.08)	(0.09)	(0.08)	(0.12)	(0.09)	(0.15)	(0.12)	(0.22)
Baptist family	0.05	0.16^{*}	0.06	0.28^{*}	0.08	-0.11	-0.11	-0.27
	(0.05)	(0.08)	(0.05)	(0.11)	(0.08)	(0.13)	(0.13)	(0.34)
Other religion	0.00	0.03	0.07	-0.02	0.05	0.01	-0.22^{+}	-0.40^{**}
	(0.04)	(0.06)	(0.05)	(0.09)	(0.06)	(0.10)	(0.11)	(0.15)
Atheist family	0.13^{*}	0.31^{**}	0.24^{**}	-0.09	0.27^{**}	-0.10	0.21	0.36
	(0.05)	(0.09)	(0.08)	(0.18)	(0.08)	(0.17)	(0.23)	(0.22)
AFQT score below the mean	0.15^{**}	0.23^{**}	0.19^{**}	0.19^{**}	0.09^{+}	0.28^{**}	0.23^{**}	0.33^{**}
	(0.03)	(0.05)	(0.04)	(0.04)	(0.05)	(0.07)	(0.07)	(0.08)
Mother's education (years)	-0.04^{**}	-0.02^{+}	-0.02^{**}	0.00	-0.02^{*}	0.01	0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Two-parent house at 14	-0.11^{**}	-0.06	-0.20^{**}	-0.16^{**}	-0.11^{+}	-0.09	-0.06	-0.22^{*}
	(0.04)	(0.06)	(0.04)	(0.04)	(0.06)	(0.07)	(0.08)	(0.09)
Currently married	0.75^{**}	0.70^{**}	0.52^{**}	0.94^{**}	0.73^{**}	0.62^{**}	0.51^{**}	0.78^{**}
	(0.04)	(0.06)	(0.13)	(0.05)	(0.05)	(0.09)	(0.08)	(0.07)
Constant	-2.62^{**}	-3.03^{**}	-2.54^{**}	-5.45^{**}	-42.85^{*}	-12.01	24.09	-50.97^{**}
	(0.24)	(0.29)	(0.20)	(0.37)	(17.10)	(29.13)	(24.23)	(25.21)
Observations	125,341	51,652	63,846	44,715	43,847	14,237	17,262	12,544
Discrete change due to in the MLDA ($\bar{\Delta}$)	**-0.001**	**0.006****	**0.003***	**-0.010***	**0.001**	**0.004**	**-0.003**	**0.001**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)

Note: **significant at 1%; * at 5%; $^{+}$ at 10%. Standard errors clustered by state. Person-month observations. All models include state, calendar year, and calendar month fixed effects as well as a cubic polynomial for age. An excluded category for religion is women raised in Catholic families. The dpendent variable equals 1 if pregnant; 0 otherwise.

Appendix D (continued)

Table D-2: Probit coefficient estimates and the average effect of a decrease in the MLDA on the outcome for 15-17 and 18-20 age groups

	15-17 year-old				***18-20 year-old***			
	Non-poor white	**Poor white**	**Black**	**Hispanic**	**Non-poor white**	**Poor white**	**Black**	**Hispanic**
MLDA is 18, 19 or 20	-0.12	0.03	0.18^{+}	-0.18	-0.14	0.70^{**}	0.07	-0.93^{**}
	(0.13)	(0.13)	(0.10)	(0.17)	(0.11)	(0.15)	(0.20)	(0.28)
Baptist family	0.16^{+}	0.14	0.19^{*}	0.32^{+}	-0.04	0.19^{*}	-0.01	0.24^{*}
	(0.09)	(0.10)	(0.09)	(0.18)	(0.06)	(0.10)	(0.10)	(0.10)
Other religion	0.04	0.07	0.27^{**}	-0.05	-0.04	-0.02	-0.11	0.00
	(0.06)	(0.07)	(0.09)	(0.14)	(0.05)	(0.09)	(0.10)	(0.11)
Atheist family	0.24^{**}	0.21	0.37^{**}	0.25	0.02	0.48^{**}	0.17	
	(0.08)	(0.15)	(0.12)	(0.23)	(0.07)	(0.12)	(0.14)	
AFQT score below the mean	0.21^{**}	0.22^{**}	0.14^{*}	0.17^{+}	0.10^{**}	0.23^{**}	0.24^{**}	0.18^{**}
	(0.06)	(0.07)	(0.06)	(0.09)	(0.04)	(0.06)	(0.08)	(0.05)
Mother's education (years)	-0.04^{**}	-0.02^{+}	-0.03^{**}	0.00	-0.04^{**}	-0.01	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Two-parent house at 14	-0.08	-0.05	-0.23^{**}	-0.11^{*}	-0.13^{**}	-0.10	-0.17^{**}	-0.20^{**}
	(0.06)	(0.07)	(0.04)	(0.05)	(0.05)	(0.07)	(0.05)	(0.07)
Currently married	0.86^{**}	0.81^{**}	0.67^{*}	1.33^{**}	0.75^{**}	0.69^{**}	0.51^{**}	0.87^{**}
	(0.11)	(0.09)	(0.29)	(0.07)	(0.05)	(0.08)	(0.12)	(0.08)
Constant	-2.58^{**}	-2.83^{**}	-2.78^{**}	-3.15^{**}	-6.02^{*}	-8.91^{+}	-7.04^{+}	-7.33^{*}
	(0.29)	(0.31)	(0.26)	(0.55)	(2.65)	(4.68)	(3.71)	(3.52)
Observations	68,716	28,897	37,694	25,285	56,400	21,550	25,614	18,348
Discrete change due to in the MLDA ($\bar{\Delta}$)	**-0.001**	**0.001**	**0.004^{+}**	**-0.003**	**-0.003**	**0.015^{**}**	**0.002**	**-0.040^{+}**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.02)

Note: **significant at 1%; * at 5%; $^{+}$ at 10%. Standard errors clustered by state. Person-month observations. All models include state, calendar year, and calendar month fixed effects as well as a cubic polynomial for age. An excluded category for religion is women raised in Catholic families. Dependent variable equals 1 if pregnant; 0 otherwise.

Appendix D (continued)

Table D-3: Probit coefficient estimates for abortion model, women age 15-20

	Non-poor white	**Poor white**	**Black**	**Hispanic**
MLDA is 18, 19 or 20	-0.36	2.51**	0.48*	1.55+
	(0.34)	(0.86)	(0.22)	(0.85)
Baptist family	-0.05	0.19	0.30	0.28
	(0.26)	(0.34)	(0.36)	(0.34)
Other religion	0.34*	0.03	0.36	0.76*
	(0.16)	(0.24)	(0.34)	(0.37)
Atheist family	-0.23	-0.20	-0.21	2.14**
	(0.32)	(0.47)	(0.57)	(0.42)
AFQT score below the mean	-0.54**	-0.79**	-0.83**	-1.06**
	(0.18)	(0.19)	(0.26)	(0.22)
Mother's education	0.16**	0.03	0.14**	0.18**
	(0.04)	(0.05)	(0.04)	(0.03)
Two-parent house at 14	0.11	0.43+	0.15	0.12
	(0.15)	(0.23)	(0.20)	(0.46)
Currently married	-1.77**	-1.78**	0.02	-1.45**
	(0.27)	(0.46)	(0.47)	(0.30)
Constant	-2.31**	-4.38**	-2.10*	-2.90*
	(0.79)	(1.15)	(0.91)	(1.38)
Observations	541	302	393	274

Note: ** significant at 1%; * at 5%; + at 10%. Standard errors clustered by state. Samples are limited to women who are pregnant for the first time, excluding pregnancies ending in miscarriages or stillbirths. Dependent variable equals 1 if the 1st pregnancy ended in an abortion, 0 if in a live birth. All models include state and calendar year fixed effects and a cubic polynomial for age; dummies for calendar months are excluded. An excluded category for religion is women raised in Catholic families.

Appendix D (continued)

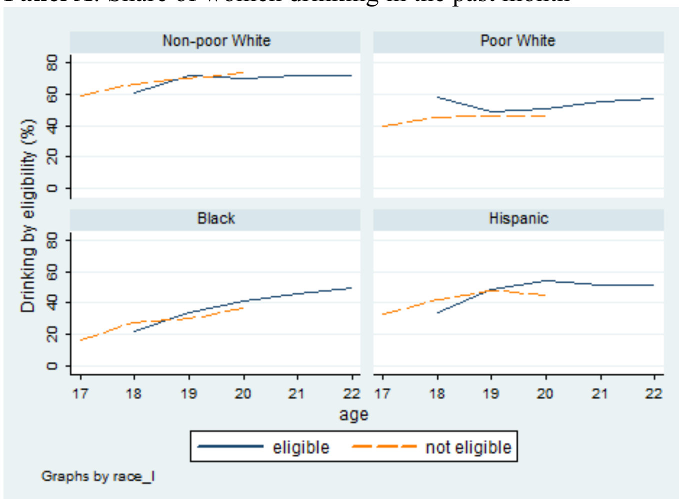
Table D-4: Probit coefficient estimates for models with a "legally eligible" indicator, women age 18-20

	1st pregnancy			Abortion		
	Non-poor white	Poor white	Black	Non-poor white	Poor white	Black
Eligible to drink legally	-0.16**	0.14	-0.12*	-1.38**	0.35	0.66
	(0.06)	(0.11)	(0.05)	(0.53)	(1.33)	(0.80)
Baptist family	-0.04	0.19*	-0.01	-0.28	0.44	-0.70
	(0.06)	(0.10)	(0.10)	(0.38)	(1.27)	(0.52)
Other religion	-0.04	-0.02	-0.11	0.41*	0.08	-0.05
	(0.05)	(0.09)	(0.10)	(0.16)	(0.90)	(0.39)
Atheist family	0.02	0.46**	0.17	0.62	1.83	-1.37*
	(0.07)	(0.13)	(0.14)	(0.60)	(1.80)	(0.69)
AFQT score below the mean	0.10**	0.23**	0.24**	-0.24	-1.88**	-1.39**
	(0.04)	(0.06)	(0.08)	(0.29)	(0.66)	(0.45)
Mother's education	-0.04**	-0.01	-0.01	0.29**	-0.29+	-0.02
	(0.01)	(0.01)	(0.01)	(0.07)	(0.17)	(0.07)
Two-parent house at 14	-0.13**	-0.10	-0.17**	-0.13	0.57	0.29
	(0.04)	(0.07)	(0.05)	(0.18)	(0.69)	(0.17)
Currently married	0.75**	0.68**	0.51**	-2.16**		-0.02
	(0.05)	(0.08)	(0.12)	(0.53)		(0.54)
Constant	-5.70*	-8.73+	-6.49+	-18.53	-107.58	38.13
	(2.65)	(4.76)	(3.71)	(21.86)	(66.79)	(41.33)
Observations	56,400	21,550	25,614	310	84	130

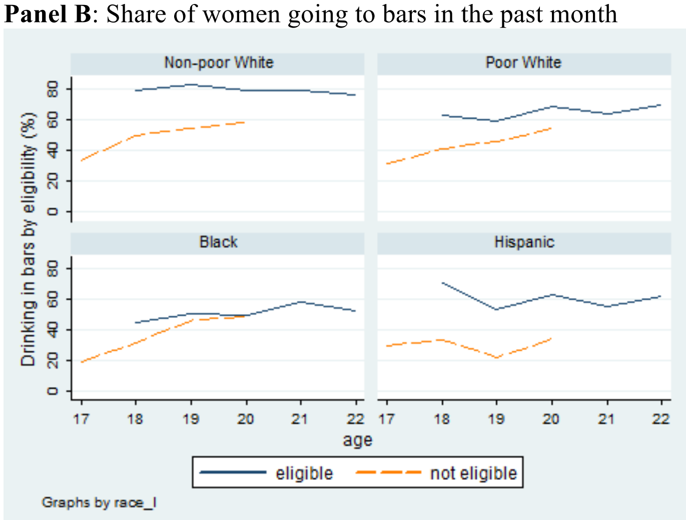
Note: ** significant at 1%; * at 5%; + at 10%. Standard errors clustered by state. All models include state and calendar year fixed effects and a cubic polynomial for age. In addition, the discrete hazard models include calendar month fixed effects. An excluded category for religion is women raised in Catholic families. For the discrete hazard model, the dependent variable equals 1 if pregnant for the first time, 0 otherwise. For abortion models, it equals 1 if the 1st pregnancy ended in an abortion, 0 if a live birth; the sample is limited to pregnant women excluding pregnancies ending in miscarriages/stillbirths. The estimates for age group 15-20 are available upon request.

Appendix E: Trends in alcohol use and bar attendance by eligibility status **(SUPPLEMENTARY CONTEN FOR ONLINE PUBLICATION)**

Panel A: Share of women drinking in the past month



Panel B: Share of women going to bars in the past month



Note: Data are from the 1982-1985 annual NLSY79 surveys. (The questions were added to the survey in 198 the youngest NLSY79 cohort was age 17. Due to the aging of the cohorts the analysis of alcohol consumptio patterns among the underage is not feasible). Going to bars is conditional on the positive alcohol use in the pa month.