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# Driving, Dropouts, and Drive-Throughs: Mobility Restrictions and Teen Human Capital*

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**Abstract**

We provide evidence that graduated driver licensing (GDL) laws, originally intended to improve public safety, impact human capital accumulation. Many teens use automobiles to access both school and employment. Because school and work decisions are interrelated, the effects of automobile-specific mobility restrictions are ambiguous. Using a novel triple-difference research design, we find that restricting mobility significantly reduces high school dropout rates and teen employment. We develop a multiple discrete choice model that rationalizes unintended consequences and reveals that school and work are weak complements. Thus, improved educational outcomes reflect decreased access to leisure activities rather than reduced labor market access.

*Keywords:* mobility restrictions, human capital, teen employment, graduated driver licensing, multiple discreteness

*JEL Codes:* I20, J24, J22, C35, R48

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# 1 Introduction

Throughout their teenage years, individuals make decisions regarding human capital accumulation that can permanently alter their lifetime economic trajectories. Of particular import is the decision of whether or not to complete high school. Additional years of high school education have been shown to increase adult earnings (Angrist and Krueger 1991) and lifetime wealth (Oreopoulos 2007), as well as reduce rates of teen pregnancy (Black, Devereux, and Salvanes 2008) and the incidence of adolescent crime (Anderson 2014). Policymakers, educators, and parents invest substantial time and money in efforts to shape such human capital decisions.

However, understanding the effects of policies aimed at influencing early human capital accumulation is complicated by the interrelated nature of teen choices regarding work, schooling, and leisure. Policies that are intended to directly impact teen employment decisions may also unintentionally affect schooling decisions (and vice versa) if the two activities are strong complements or substitutes. Furthermore, policies that impact the set of leisure activities available to teens may have indirect effects on both schooling and employment decisions.

We study a policy targeted at teen car safety that was not intended to impact human capital accumulation and show that, nonetheless, it affected both high school dropout decisions and teen employment. We employ quasi-experimental variation in teen driving laws within a difference-in-differences-in-differences research design to identify the overall effects of teen mobility restrictions on educational attainment and employment. We then develop and estimate a structural framework that rationalizes these findings. The framework separates the direct effects of the policy from indirect effects due to activities being substitutes or complements. Distinguishing these channels not only clarifies and enriches our results, but is also broadly useful in designing future policies to better target teen behavior.

Specifically, we investigate graduated driver's licensing (GDL) laws, which aim to reduce automobile accidents by limiting teen access to driving. GDL laws typically increase the minimum age at which teens can access full-privilege driver's licenses and create an intermediate licensing level that restricts nighttime driving and/or restricts the number of passengers who may ride with a teen driver. We combine variation in the timing of GDL laws across states with variation in compulsory schooling laws to create a novel triple-difference research design, which allows us to identify the total effect of teen mobility restrictions on high school retention. Compulsory schooling laws make it very costly for targeted teens to drop out of high school, potentially masking the consequences of restricted mobility on school attendance. We use differences in compulsory attendance ages to compare outcomes in states where GDL laws change for teens whose dropout behavior is unrestricted with outcomes in states where GDL laws change but teens face a legal requirement to stay in school. The timing of these policy changes (GDL laws were adopted by many states in the late 1990s, whereas the increase in minimum school-leaving ages began largely in the early 2000s) provides a unique opportunity to observe the effects of teen mobility restrictions when students have the option to (legally) drop out of school.

Using microdata from the Current Population Survey's Annual Social and Economic Supplement, we find that GDL laws decrease the probability of high school dropout for 16-year-old teenagers by 1.1pp (a 28% reduction at the mean) only in state-years where these teens are legally able to leave high school. A benefit of this triple-difference design is that it also provides a natural placebo test. We find no evidence of an effect of GDL laws on teens for whom school attendance is compulsory, which suggests that our identification strategy successfully isolates exogenous policy variation.

That restricting teen mobility improves educational attainment is surprising because reducing access to an activity should have a weakly negative effect on participation, ceteris paribus (particularly in rural areas or cities with few transportation alternatives). However, mobility restrictions might also limit access to alternative activities, such as employment or leisure. The indirect effects on high school completion stemming from changes in access to alternative activities could dominate the direct effect, depending on the magnitude of those indirect effects and substitution patterns between activities. Thus, the sign of the total impact of mobility restrictions on critical human capital accumulation during formative teen years is ex ante ambiguous. Our positive estimate on high school retention suggests that these indirect margins are likely very important to the underlying teen decision-making process.[1]

We next investigate the impact of GDL laws on teens' human capital decisions surrounding labor force participation. We apply our triple-difference approach to teen employment outcomes, which provides some evidence on whether our findings on high school dropout are attributable in part to reduced access to employment opportunities.

[1] Note, however, that the strictest variant of GDL law, which completely disallows unsupervised 16-year-old driving, does not cause a corresponding decline in the probability of high school dropout. These two results together suggest that limiting teen driving may improve educational outcomes by reducing access to alternative activities (such as leisure or employment), but these positive effects diminish if teen access to driving is completely removed.

We find that GDL laws reduce 16-year-old labor force participation by 1.7pp (a 7% reduction at the mean) only in state-years where those teens are unrestricted by compulsory schooling laws. This results strongly suggests an indirect channel linking teen's decisions regarding school and work. However, it is unclear how much of this reduction in teen labor force activity reflects a direct effect from GDL laws restricting teens' ability to commute to jobs, or indirect effects caused by changes in school-going or leisure activities.

To distinguish these channels, we turn to a multiple discrete choice model in which teens choose to participate in school, work, both activities, or neither activity. We allow GDL laws to differentially impact each of these options. The model identifies the complementarity or substitutability of schooling and work using exclusion restrictions on compulsory schooling laws and labor market conditions. Model estimates reveal that employment is not a strong substitute for high school attendance; in fact, they are weak complements. Thus, we conclude that very little of the reduction in high school dropout behavior can be attributed to the changes in labor market access caused by GDL laws. Counterfactual simulations reveal that improved high school retention reflects decreased access to leisure activities (including, for example, risky behaviors).[2] On the other hand, the observed reduction in teen labor force participation can be attributed almost entirely to increased difficulty in commuting to work opportunities.

This paper offers several contributions. The first is an important insight into the determinants of educational attainment among teenagers. Sixteen-year-olds, despite not being adults de jure, make meaningful human capital decisions that will impact their lifetime trajectories in the labor market. Policies that influence the economic environments in which these teens live can shape those decisions in unexpected ways. We show that GDL laws are one such policy, adding to a growing literature investigating how various non-education policies can impact high school dropout behavior (Cohodes et al. 2016; Lovenheim, Reback, and Wedenoja 2016; Miller and Wherry 2018; Groves 2020; Kennedy 2020).[3] By limiting teen mobility, policy-makers inadvertently impacted the monumental decision of whether or not to complete a high school degree.

This finding contributes to the broader literature on the determinants of high school

[2] This finding complements related literature showing that GDL laws reduce the likelihood of risky behaviors by teens (Deza and Litwok 2016; Deza 2019).

[3] Lovenheim, Reback, and Wedenoja (2016) show that school-based health centers reduce teen childbearing but have no impact on high school dropout rates. Nonetheless, Medicaid expansions for children and pregnant women do generate significant reductions in high school dropout rates (Cohodes et al. 2016; Miller and Wherry 2018; Groves 2020). Kennedy (2020) finds that targeted mobility restrictions in the form of "No Pass, No Drive" laws do not impact high school graduation rates.

dropout behavior, which has consistently shown that leisure activities (and especially risky behaviors) are strongly correlated with the decision to drop out (Bray et al. 2000; Koch and McGeary 2005; Crispin 2017).[4] In a comprehensive analysis of the effects of compulsory schooling laws on educational attainment, Oreopoulos (2007) concludes that, "it is very difficult to reconcile substantial returns to compulsory schooling with an investment model of school attainment. The results are more consistent with the possibility that many adolescents ignore or heavily discount future consequences when deciding to drop out of school." Our findings are in line with this conclusion and show that GDL laws, through reduced access to leisure activities, improve educational attainment for teens.

We also provide insight into teen employment decisions, which are both directly impacted by mobility restrictions and provide a mechanism through which driving restrictions might impact educational attainment. Our framework reveals that GDL policies directly limit access to employment for 16-year-olds. However, estimates show that school and work are complements, so it is not reduced work access that increases high school retention.[5] This finding complements existing evidence that restrictions to mobility can impact the labor supply of non-teen groups (Amuedo-Dorantes, Arenas-Arroyo, and Sevilla 2020; Black, Kolesnikova, and Taylor 2014) and updates the literature investigating the link between education and employment for teens (e.g., Eckstein and Wolpin 1999).[6]

Finally, we develop a structural framework for policy analysis that incorporates our triple-difference research design. We show that this model distinguishes mechanisms, separating direct from indirect (substitution) effects. We also show how the model can be adapted to contexts where there is no "outside option" unaffected by the policy of interest by using additional restrictions to set identify a normalizing parameter. Our model retains a primary focus on identifying policy parameters while adding structure to gain additional insight and interpretation; relatively few papers combine quasi-experimental research design with discrete choice models for policy evaluation (an exception is Li 2018).[7] Moreover, the model provides an alternative to, and ultimately reinforces, the

[4] Anderson (2014) and Bell, Costa, and Machin (2016) also find evidence of a reversed causal link: increasing the minimum legal dropout age has a significant and negative effect on violent and property crime arrest rates for teens.

[5] Argys, Mroz, and Pitts (2019) study the related question of whether GDL laws are responsible for the secular decline in US teen employment. They find that GDL laws can explain about half of the drop in teen labor force participation. This result is broadly consistent with our employment findings despite the fact that we employ a different research design, data sample, and observation window.

[6] The evidence on the impact of working while in high school has largely shown that part-time employment while in school is not detrimental to academic success (Montmarquette, Viennot-Briot, and Dagenais 2007; Dustmann and Soest 2008).

[7] An extensive literature applies dynamic structural modeling to human capital accumulation. Given

design-based (i.e., reduced form) approach. These two methods are complementary.

We describe the background and context for our study and detail data sources in Section 2. In Section 3, we describe the triple-difference research design. Section 4 presents our main results on education outcomes as well as an array of robustness checks employing alternate model specifications[8], alternate estimators[9], and alternate data sets.[10] In this section we also explore heterogeneity across various subgroups and heterogeneity by differing levels of mobility restrictions imposed by GDL laws. In Section 5, we turn to an investigation of teen employment outcomes to explore potential mechanisms. Section 6 unites education and employment decisions within a structural model to differentiate the various effects of GDL laws on teen activities and Section 7 concludes.

## 2 Context and Data

High teen driving fatality risk in the United States in the 1980s led to the implementation of a number of policies targeted at improving both car safety and limiting teen driving. One such policy that has been widely adopted starting in the mid 1990s is the graduated driver's license (GDL). GDL laws: (i) limit full privilege licenses to older (>16) teens and (ii) create an intermediate licensing level that restricts nighttime driving and/or restricts the number of passengers who may ride with a teen driver. Such restrictions made substantial progress in reducing fatal teen car accidents, which declined by 55% from 2004–2013 (Dee, Grabowski, and Morrisey 2005; Shults, Olsen, and Williams 2015). Further, these restrictions appear to decrease fatalities primarily by decreasing teen driving rather than improving the quality of teen driving, implying restricted mobility (Gilpin 2019; Karaca-Mandic and Ridgeway 2010).

We develop a database of pertinent state-level GDL laws in the 50 states and DC from several sources, including the Federal Highway Administration's (FHWA) Highway Statistics and the Insurance Institute for Highway Safety (IIHS) covering the years 1990 to 2017.[11] Figure 1a shows counts of the number of states with various types of

our repeated cross-sectional data, our approach instead grows out of product choice models from industrial organization (e.g., Berry, Levinsohn, and Pakes 1995; Gentzkow 2007; Goolsbee and Petrin 2004).

[8]We provide evidence of parallel pre-trends using an event study specification to support our identifying assumptions.

[9]We address the new literature on staggered adoption difference-in-differences models and show that our results are robust to a variation of the imputation estimator of Borusyak, Jaravel, and Spiess (2021).

[10]We replicate our main findings using school-district level data from the National Center for Education Statistics' Common Core of Data.

[11]IIHS data begins coverage in 1995. We use FHWA data for the years before 1995, and to rectify conflicts

GDL laws over time. Prior to 1995, fewer than ten states limited full privilege licenses to those older than 16 or had nighttime driving restrictions. But by 2010, forty-seven states had placed increased restrictions on teenage driving. Much of the adoption of GDL laws occurred between 1996 and 2003.

To verify that GDL laws had a binding effect on teen automobile use, we link our GDL law dataset to information from the US Department of Transportation's Fatality Analysis Reporting System (FARS). We use the rate of fatal car accidents involving a teen driver as a proxy for the prevalence of teen driving and estimate the effect of increasing the minimum full-privilege driving license age on teen accident rates. We find that the GDL driving restrictions reduced the rate of fatal car accidents for drivers aged 16 by 27 percent. This result indicates that teens are significantly less likely to be involved in a fatal car accident when they cannot access an unrestricted driver's license and that GDL laws significantly restricted teen driving. We discuss this verification exercise in detail in Appendix A.

In order to identify the effects of GDL laws on teen human capital decisions, we will implement a triple-difference identification strategy that interacts variation in GDL laws with variation in state-specific compulsory schooling (CS) laws. Specifically, we will use the mandated school-leaving age (the minimum age at which a teen is legally allowed to drop out of school) to create a "control" group of teens who are exposed to GDL restrictions but who cannot respond by dropping out of high school due to the local CS laws. We extend the school-leaving age data from Anderson (2014) (which covers 1980–2008) up to 2017. For 2009–2011, 2013–2015, and 2017, we draw on the National Center for Education Statistics' (NCES) State Education Reforms tables and fill in the intervening years for states with no changes. For states with a change in the minimum school-leaving age, we verified the timing of the change in legal databases.[12] Figure 1b shows counts of the number of states with different minimum school-leaving ages from 1990 to 2017. Over this time period 25 states changed their minimum school-leaving age, in most cases from 16 to either 17 or 18.

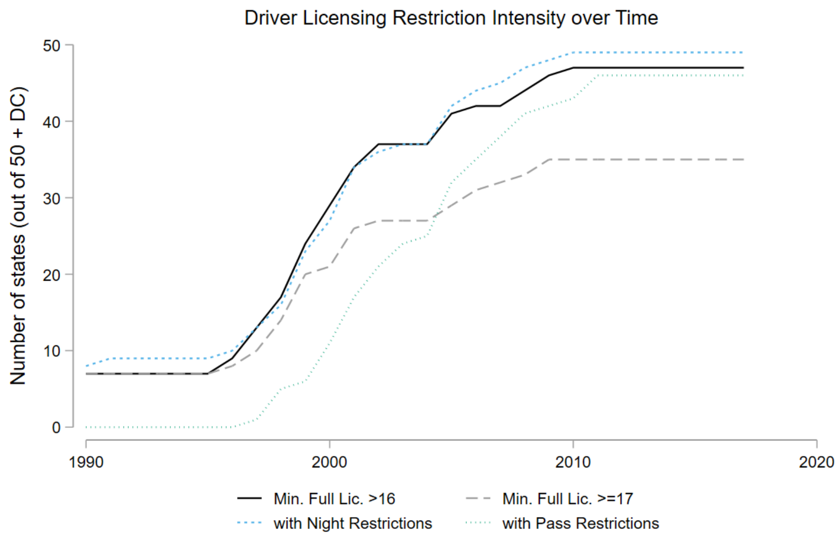
We link the data on each state's GDL and CS laws to individual-level data on schooling and work decisions in the Current Population Survey (CPS) Annual Social and Economic Supplement (ASEC).[13] The CPS ASEC data is from an annual survey of U.S. households conducted in March of each year and provides person-level information on a variety of

between the two datasets. The GDL data is similar to that used in Severen and Benthem (2019).

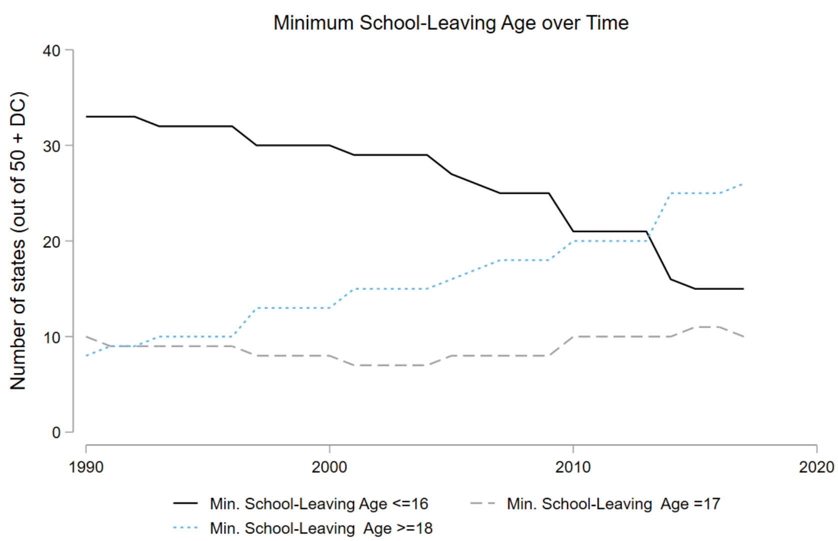
[12] A precise accounting of these changes is available from the authors upon request.

[13] When linking these datasets, we assign a GDL law to a year if that law was in effect by December of that year. In Appendix A, we verify that this approach is reasonable.

Figure 1: Teen Driving Restrictions & Minimum School-Leaving Age from 1990–2017




(a) Graduate Driver Licensing Adoption




(b) Minimum Legal School-Leaving Age

demographics, household controls, and teen outcomes. Importantly, the survey asks all participants aged 16–24 if they were enrolled in high school or college during the previous week, and, if so, whether they were enrolled full- or part-time.[14] We use these responses

[14]Students on holiday or seasonal vacation at the time of the survey were instructed to answer “yes" to this question.

to construct a single indicator variable, $NotInSchool_i$, which equals 1 if individual $i$ is not enrolled in any amount of either high school or college in the week preceding the survey. We use this measure as a proxy for high school dropout, although it will also incorporate measurement error from those teens who have already completed a high school degree and chose not to enroll in college. CPS ASEC participants were also surveyed on labor force participation and employment status in the preceding week.

To construct our final estimation sample, we limit the linked GDL law, CS law, and CPS data to individuals aged 16 at the time of the ASEC survey. This serves dual purposes: (1) our measure of high school dropout, $NotInSchool_i$, should incorporate less noise for this age group as they are very unlikely to have already completed high school; (2) this is the age that is most impacted by the implementation of the GDL laws. Between 1990–2017, 40 states switched from allowing 16-year-old teens to obtain full driver's licenses to restricting this privilege to older teens.[15]

Table 1: Summary Statistics on Individuals Aged 16

| | Mean | Std. Dev | Min | Max |
|---|---|---|---|---|
| Individual Characteristics: | | | | |
| Female | 0.49 | 0.50 | 0 | 1 |
| White | 0.78 | 0.42 | 0 | 1 |
| Black | 0.15 | 0.36 | 0 | 1 |
| Asian | 0.02 | 0.15 | 0 | 1 |
| Other Race | 0.05 | 0.22 | 0 | 1 |
| Hispanic | 0.16 | 0.37 | 0 | 1 |
| Mother Edu $\geq$ B.A. | 0.24 | 0.43 | 0 | 1 |
| Father In Household | 0.76 | 0.43 | 0 | 1 |
| Receives SNAP* Benefits | 0.12 | 0.33 | 0 | 1 |
| Outcome Variables: | | | | |
| $NotInSchool = 1$ | 0.038 | 0.19 | 0 | 1 |
| $InLaborForce = 1$ | 0.233 | 0.42 | 0 | 1 |
| Treatment Variables: | | | | |
| Minimum Unrestricted Driving Age | 16.9 | 0.72 | 15 | 18 |
| Minimum School-Leaving Age | 16.9 | 0.91 | 16 | 18 |
| State-level Characteristics: | | | | |
| 3-Month Unemployment Rate | 6.45 | 1.98 | 2.50 | 14.2 |
| Log Minimum Wage | 1.91 | 0.11 | 1.71 | 2.41 |

* SNAP = Supplemental Nutrition Assistance Program
Source: CPS ASEC Data on individuals aged 16 linked to GDL and CS data, BLS unemployment data, and state minimum wage data. This data includes 75,196 individual observations.

[15] In contrast, the GDL laws created binding age limits for 17-year-old drivers in only 14 states.

Finally, in order to control for the local economic environment, we link in data from the Bureau of Labor Statistics (BLS) on the monthly non-seasonally adjusted unemployment rates by state and data from the Federal Reserve Economic Data (FRED) on state minimum wages. We use the BLS data to construct a state-specific unemployment rate in each year as the three-month average of unemployment rates centered around January.[16] From FRED, we take the maximum of the state and federal minimum wage in each year and inflation-adjust to measure the binding real minimum wage in each state-year.

Table 1 reports summary statistics for the final linked estimation sample of 75,196 individuals aged 16. In this sample, 3.8% report not attending any school in the week preceding the survey. This corresponds closely to the national dropout rates reported by the NCES for the 10th and 11th grades: 3.5% and 4.1%, respectively.

# 3 Empirical Strategy

Our primary analyses investigate the relationship between GDL law adoption and teen dropout decisions using a difference-in-differences-in-differences identification strategy. The first difference compares teen dropout behavior before and after the implementation of a GDL law. The second difference leverages the staggered roll-out of the GDL policies and compares teens across states that restricted teen driving in different years (or not at all). Finally, the third difference compares teens across states with different compulsory schooling requirements.

All 50 states have some form of compulsory schooling law in place, which disallows teens to drop out of high school education before reaching a certain age. These age thresholds vary considerably across states and over time (see Figure 1b). We compare teens from states where the minimum school-leaving age is 16 or lower to those where the school-leaving age is 17 or higher to comprise our third difference.

The primary identifying assumption in this type of triple-difference set-up is much weaker than the parallel trends assumption needed for difference-in-differences. Identification in this model allows for differential trends, as long as those differences are evolving similarly across the third difference grouping. Specifically, we assume that in the absence of treatment, the difference in 16-year-old dropout rates between states that adopt GDL laws and states that do not would evolve similarly over time regardless of whether those

[16] For example, the 3-month rate for 1995 is the average of the unemployment rates in December 1994, January 1995, and February 1995.

states had binding compulsory schooling laws or not.[17]

We estimate the following fixed effects triple-difference specification for our sample of 16-year-olds:

$$\begin{aligned} NotInSchool_{ist} = \beta_1 GDL_{st} + \beta_2 CS_{st} + \beta_3 GDL_{st} * CS_{st} \\ + X_i' \nu + Z_{st}' \mu + D_s + D_t + \epsilon_{ist}, \end{aligned} \tag{1}$$

where $GDL_{st}$ is an indicator variable that equals one if the minimum unrestricted driving age in state $s$ in year $t$ is $> 16$ (i.e., 16-year-olds experience mobility restrictions).[18] We capture compulsory schooling laws with $CS_{st}$, an indicator that equals one if the minimum school-leaving age is $\leq 16$ (i.e., 16-year-olds are legally permitted to drop out of school). The vector $X_i$ includes individual-level controls: gender, race/ethnicity indicators, mother's education, presence of father in household, and receipt of SNAP benefits. The variable $Z_{st}$ includes controls for the state's minimum wage and the 3-month average unemployment rate. This specification also includes both state fixed effects to control for time-invariant confounding factors (such as persistent differences in school quality or returns to education across states) and year fixed effects to control for aggregate fluctuations (such as changes in national schooling laws over time).[19]

Because the effect of mobility restrictions on teen dropout behavior is ex ante ambiguous, we first discuss the channels through which GDL laws might impact teen educational attainment. When a state introduces a GDL law that restricts a teen's access to driving, this may have a *direct* effect on the dropout decision if the restriction hinders the teen's ability to commute to school. In particular, for low-income households or teens in rural areas with minimal access to alternative transportation, this direct effect may lead to a significant increase in high school dropout rates. However, the mobility restrictions imposed by GDL laws may also impact the teen's dropout decision *indirectly* through an effect on access to labor and leisure activities. In fact, we know from previous studies that GDL laws decrease teen participation in risky behaviors and teen labor force participation (Deza and Litwok 2016; Deza 2019; Argys, Mroz, and Pitts 2019).[20]

[17]Identification also relies on an assumption of homogenous treatment effects. We consider threats to the validity of this assumption in detail in Section 4.1 and Appendix C.

[18]For the purposes of this variable, we consider as restrictions: limits of the time of day that one can drive, limits on the number of passengers, or limits on destinations. We do not consider a requirement of parental approval a restriction.

[19]All specifications are estimated using CPS ASEC person-level weights.

[20]Huh and Reif (2021) do not study GDL laws specifically, but investigate the effect of teenage driving more generally on mortality and risky behaviors. They estimate that total mortality rises by 15% at the

The sign of these indirect effects will depend on whether schooling and employment (or schooling and leisure) serve as complements or substitutes in the teen's utility function. If work (or leisure) is seen as a substitute for schooling, then reducing access to employment will lead to a decrease in high school dropout behavior. However, if the teen views the two activities as complements, then the indirect effect will have the reverse sign and could lead to an increase in high school dropouts. Thus, the total or net effect of GDL laws on high school dropout rates will be positive in the absence of indirect effects, but may be either positive or negative if indirect effects are significant.

Our discussion thus far has assumed that teens have the option to drop out of high school in response to changes in their mobility restrictions. This assumption will fail in states that impose compulsory schooling laws making it illegal for younger teens to opt out of high school attendance. These CS laws create a natural placebo test in state-years where the school-leaving age is greater than the minimum age needed for an unrestricted driver's license. To the extent that compulsory schooling laws are well enforced, these policies effectively shut down all effects of the GDL laws on dropout behavior.

The coefficient $\beta_1$ in Equation 1 will therefore identify the "placebo" effect of imposing mobility restrictions on dropout behavior in states where 16-year-olds cannot legally drop out. However, if CS laws are not well-enforced (or if they incorporate exemptions for teens who are working or have parental consent), then we may still observe an impact of the GDL laws on high school dropout (ie., $\beta_1 \neq 0$). In this case, an effect should be observable only if there are enough students who experience a direct or indirect effect of the mobility restriction that is large enough to incentivize law-breaking. For example, in rural areas where school attendance zones are expansive and school buses can require long commutes (Howley, Howley, and Shamblen 2001), we might expect the direct effect of GDL laws to be large enough to cause an increase in high school dropout rates, even in states where the compulsory schooling laws are binding.

The coefficient $\beta_2$ in Equation 1 captures the impact of more lenient compulsory schooling laws (minimum school-leaving age is below 17) on high school dropout behavior in the absence of GDL laws. We expect this coefficient to be large and positive. Finally, the coefficient $\beta_3$ will capture the differential effect of increasing driving restrictions on the probability of dropping out between teens who are legally able to do so relative to teens who cannot drop out at age 16. Of particular interest is the sum of the two coeffi-

minimum legal driving age cutoff, driven by an increase in motor vehicle fatalities and poisoning deaths, which are caused primarily by drug overdoses.

cients, $\beta_1 + \beta_3$, which identifies the total effect of GDL laws on those teens who are legally permitted to drop out of school.

# 4 Education Results

We estimate the model in Equation 1 using a probit maximum likelihood estimator.[21] Standard errors are estimated that allow for clustering at the state level. Table 2 reports the corresponding marginal effects for each coefficient evaluated at the mean of all covariates.

In column (1), we estimate the model in Equation 1 excluding all control variables ($X_i$ and $Z_{st}$). Column (2) presents our main specification, which includes all covariates. The estimates in these two columns demonstrate that our results are not sensitive to or driven by the inclusion of covariates. Estimates of $\beta_1$ (our placebo test) are very small and are statistically insignificant, indicating that there is no discernible effect of GDL laws on 16-year-old dropout behavior in states where the minimum schooling-leaving age is binding (17 or older). As expected, the estimates of $\beta_2$ are large and statistically significant, indicating that compulsory schooling laws are generally effective (i.e. the probability of a 16-year-old leaving high school is significantly larger in states where dropout is legally permitted at that age). Moreover, these estimates are quantitatively similar to those in previous studies that analyze the impacts of compulsory schooling laws (Anderson 2014; Oreopoulos 2009). As we use more recent years of data than those papers, this provides some evidence that compulsory schooling laws continue to be impactful for educational attainment.[22]

Estimates of $\beta_3$ indicate that the differential effect of GDL laws on dropout behavior for 16-year-olds in states where dropout is legally permitted (vs. those states where dropout is not legal) is negative and statistically significant. The total (or net) effect of GDL laws on teen dropout behavior is estimated by the sum of coefficients, $\beta_1 + \beta_3$. This sum reveals that increasing the minimum driving age in states where 16-year-olds can legally drop out reduces the probability that these teens are no longer in school by approximately 1.1pp, a 28% reduction from the mean.[23] This negative estimate of the net effect indicates that,

[21] Given that only 3.8% of 16-year-olds are not in school (Table 1), a probit specification avoids the probable pitfall of predicting probabilities outside the unit interval. However, results estimated using a linear probability model are qualitatively and quantitatively similar and are shown in Appendix Table B.1.

[22] This is in mild contrast to Bell, Costa, and Machin (2016), who find inconsistent patterns between various measures of compulsory schooling and educational attainment.

[23] We assign a GDL law to a year if that law was in effect by December of that year. The CPS ASEC survey is conducted in March of each year. Because we do not observe each teen's month of birth, this means

Table 2: The Effect of Minimum Unrestricted Driving Age on 16-yo Dropout

| | Not In School = 1 | | | | | |
|---|---|---|---|---|---|---|
| | Triple-Diff | | | | Diff-in-Diff | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Min. Unres. Driving Age >16 ($\beta_1$) | 0.0022 | 0.0014 | 0.0030 | 0.0013 | -0.0031 | -0.0032 |
| | (0.0042) | (0.0039) | (0.0049) | (0.0040) | (0.0038) | (0.0035) |
| School-Leaving Age $\leq$ 16 ($\beta_2$) | 0.0197*** | 0.0182*** | | | | |
| | (0.0048) | (0.0047) | | | | |
| Min. Unres. Driving Age >16 $\times$ School-Leaving Age $\leq$ 16 ($\beta_3$) | -0.0129*** | -0.0119** | -0.0191*** | -0.0115** | | |
| | (0.0048) | (0.0048) | (0.0071) | (0.0054) | | |
| **Effect of GDL if School-Leaving Age $\leq$ 16** ($\beta_1 + \beta_3$) | -0.0107** | -0.0105** | -0.0161** | -0.0102** | | |
| | (0.0050) | (0.0049) | (0.0076) | (0.0051) | | |
| School-Leaving Age | As Observed | | Never Switchers Only | Fixed in Yr. of GDL Change | - | - |
| Controls | - | Y | Y | Y | - | Y |
| Obs | 75,196 | 75,196 | 46,567 | 75,196 | 75,196 | 75,196 |

Marginal effects evaluated at sample means from probit regression using CPS ASEC data from 1990–2017. All specifications include state and year fixed effects. Controls in columns (2)–(4) and (6) are: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; and state log real effective minimum wage. Columns (4) and (6) also include indicators for the state minimum legal dropout age. Column (3) limits the sample to states that never changed school-leaving age, while Column (4) fixes school-leaving age to its level when the state increased minimum unrestricted driving age to >16. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

if there is any direct effect of GDL laws on high school attendance (through increased difficulty in commuting to/from school), it is more than completely offset by the indirect effects of GDL laws through reduced access to labor and leisure activities.

## 4.1 Robustness and Alternative Estimators

We provide an array of robustness checks and alternative estimators to ensure that our main findings are robust to various assumptions. In this section, we discuss robustness to various assumptions on policy interactions between GDL and CS laws. We also provide evidence supporting the assumption of parallel pre-trends using an event study specification. We also address the new literature on staggered adoption difference-in-differences models and show that our results are robust to alternative estimators that do not rely on

that some teens may have turned 16 before the GDL law went into place. We account for this potential misclassification by dropping observations from the initial treatment year for each state and find that our results are largely unchanged. These results are available upon request.

an assumption of homogenous treatment effects. In Section 4.2, we further show that our results can be replicated using an alternative dataset. Finally, in Section 6 we estimate a structural model that relies on alternative assumptions and also verifies our main findings.

One potential confounding factor in the empirical model stems from the fact that compulsory schooling laws were also evolving during our sample. Between 1990 and 2017, about half of states increased their minimum school-leaving age (see Figure 1b). To avoid conflating effects from changes in this policy with the effects of the GDL laws, we employ two robustness checks. First, we estimate the model in Equation 1 on the sub-sample of states that did not change their minimum school-leaving age during the time period under study. Results for this restricted sample are shown in column (3) of Table 2 and are consistent with our main specification and somewhat larger.[24]

Second, we replace $CS_{st}$ in Equation 1 with a time-invariant measure that is fixed at each state's minimum school-leaving age in the year that the GDL law first increases the minimum unrestricted driving age to over 16. For states where the minimum unrestricted driving age is either always less than or equal to 16 or always greater than 16, we use the minimum school-leaving age from the first year of the sample, 1990.[25] The results of this specification are shown in column (4) of Table 2. Estimates are nearly identical to the estimates from our main specification in column (2) and support our previous findings.

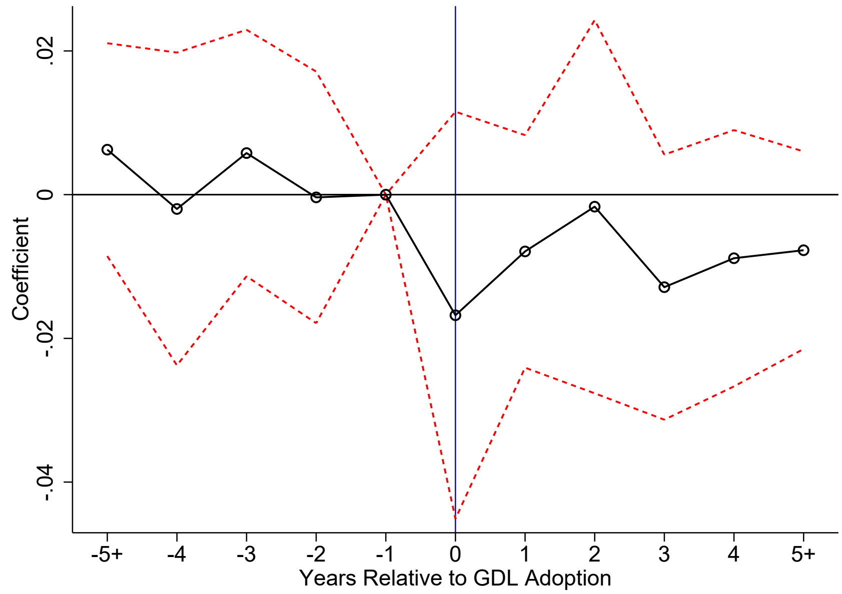
Columns (5)–(6) of Table 2 display the results of a simpler difference-in-differences model that excludes the interaction term, $\beta_3 GDL_{st} * CS_{st}$. These results are relatively small and insignificant. This is to be expected as these small, negative estimates reflect a weighted average of the null GDL effects in states that have a binding school-leaving age and the larger negative effects in states with less restrictive compulsory schooling laws. These results clearly demonstrate the advantage of estimating the more robust triple-difference model.

We next provide an indirect test of the plausibility of our identifying assumptions using an event study specification. We estimate:

$$NotInSchool_{ist} = \beta_1 GDL_{st} + \beta_2 CS_{st} + \sum_{k=-5}^{-2} \theta_k GDL_{stk} * CS_{st} + \sum_{k=0}^{5} \theta_k GDL_{stk} * CS_{st} + X_i'\nu + Z_{st}'\mu + D_s + D_t + \epsilon_{ist}, \tag{2}$$

[24]Note that in these alternate specifications, the coefficient $\beta_2$ is absorbed by the state fixed effects.
[25]In this specification we also control separately for the actual time-varying school-leaving age.

Figure 2: The Effect of Minimum Unrestricted Driving Age on 16-yo Dropout




Marginal effects evaluated at sample means from probit regression using CPS ASEC data from 1990–2017. This figure plots $\theta_k$, and 95% confidence intervals in dashed lines, from estimating Equation 2. Controls include: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; state log real effective minimum wage, state fixed effects, and year fixed effects. Standard errors are clustered at the state level.

where each $GDL_{stk}$ is an indicator for $k$ years from the adoption of a GDL law (e.g., $GDL_{st0} = 1$ if state $s$ increased the minimum unrestricted driving age to $> 16$ in year $t$). The omitted category is $k = -1$. We restrict the effect of GDL laws on all cohorts who turned 16 more than five years before or after the law went into effect to be unchanging so that $\theta_{-5}$ and $\theta_5$ represent the average effect five or more years prior to or after the GDL adoption, respectively. All other variables are the same as in Equation 1.

The estimates and confidence intervals for each $\theta_k$ coefficient are plotted in Figure 2. An important take-away is that the pre-treatment region reveals that no evidence of anticipatory effects of the GDL laws or of pre-treatment differences between states that eventually implemented GDL laws vs. states that did not. Furthermore, there is a clear decline in the probability of 16-year-old dropout that coincides exactly with the year of GDL law adoption. These post-treatment point estimates are not statistically significant, indicating that we do not have sufficient power to separately estimate treatment effects in each year of treatment timing. However, the sign and magnitudes are broadly consistent with the main estimates in Table 2.

A growing literature has revealed that two-way fixed effects estimation of staggered adoption difference-in-differences research designs does not generally identify the average treatment effect on the treated (ATT) when treatment effects are heterogeneous or dynamic (e.g., Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021; Sun and Abraham 2021). Bias can arise from a number of sources, but is typically due to the implicit selection by the two-way fixed effects estimator of inappropriate counterfactuals (e.g., previously treated units) or of incorrect aggregation weights (e.g., negative weights). While our research design does not fit the standard staggered adoption difference-in-differences mold, the potential for these types of bias is still present.

A number of solutions have been proposed to overcome this issue, however none have thus far been adapted to fit our setting of using repeated cross-sectional data with a placebo-style triple-difference design where policy interactions turns on and off.[26] Moreover, this literature primarily focuses on linear models: as our outcome of interest is binary with a mean value close to zero, a linear probability model is potentially biased and inconsistent. Because of these aspects of our research design, there is no alternative estimation strategy that we can adopt wholesale from the current literature to address the challenges caused by heterogeneous and dynamic treatment effects. Nevertheless, we provide several exercises to test the robustness of our results to possible deviations from static, homogeneous treatment effects. We also alter our design to use the imputation estimator of Borusyak, Jaravel, and Spiess (2021). We detail these strategies and results in Appendix C and provide a summary of our findings below.

The first approach estimates a model similar to our preferred specification but considers subsets of the time variation used in the full analysis and allows for some dynamism in treatment effects. The results (shown in Table C.1 and Table C.2) provide evidence that our main results are not being driven by long-run dynamics in the treatment effects of GDL laws and that effects remain relatively constant over time.

The second approach recasts our research design as difference-in-differences (instead of triple-difference) and assumes a linear specification so that we can apply the imputation estimator detailed in Borusyak, Jaravel, and Spiess (2021).[27] This estimator recovers a well-defined ATT even under arbitrary treatment-effect heterogeneity and dynamism.

[26]Chaisemartin and D'Haultfoeuille (2022) make some progress toward interacted designs, however, by studying difference-in-differences designs with multiple treatments.

[27]See Appendix C, section C.1 for additional details.

Table 3: Imputation Based Effect of Minimum Unrestricted Driving Age on 16-yo Dropout

| | Not In School = 1 | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| **Effect of GDL if School-Leaving Age $\leq$ 16** | -0.0109** (0.0047) | -0.0111** (0.0045) | -0.0113** (0.0046) |
| Exclude Always Treated | Y | Y | Y |
| Exclude Never Treated | - | - | Y |
| Controls | - | Y | Y |
| Obs | 50,729 | 50,729 | 46,853 |

Static treatment effect estimated using the imputation estimator of Borusyak, Jaravel, and Spiess (2021). Data are from the CPS ASEC covering 1990–2017. All specifications include state and year fixed effects and indicator for minimum legal dropout age. Controls in columns (2–3) are: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; and state log real effective minimum wage. * p<0.10, ** p<0.05, *** p<0.01

The results of this estimator are shown in Table 3 and are strikingly similar to our main results, even despite the smaller sample size. The model in column (1) omits all controls except $CS_{st}$. Column (2) includes all control variables ($X_i$ and $Z_{st}$). Column (3) omits never-treated units (all three columns omit always-treated units) to test whether our results hinge on comparisons to states that are subject to different trends than those that eventually adopt GDL laws. Taken together, these results strongly suggest that our main results are robust to dynamics, to arbitrary treatment effect heterogeneity, and to reasonable restrictions on the control group.

## 4.2 Alternative Dropout Data

To further support our main findings from Table 2, we also analyze the impact of GDL laws on teen dropout decisions using school-district level data from the NCES' Common Core of Data. The Common Core is a comprehensive national database of all public elementary and secondary schools and provides high school dropout rates aggregated at the school district-by-grade level. A primary advantage of this dataset is that, because it includes data by school district, we can include school district fixed effects to control for time-invariant differences between places within states; we discuss this data and analysis in detail in Appendix D.

We find that the implementation of GDL laws leads to a 0.42pp reduction in high school dropout rates in the NCES data (a 12% reduction at the mean).[28] Furthermore, the effects of increasing the minimum driving age to over 16 are largest in the 10th and 11th grades (the grades in which students are most likely to be 16 years old and thus directly affected by GDL laws) - a 14% reduction from the mean for both grades. These results confirm our main findings in Table 2 and provide compelling evidence that imposing restrictions on teen mobility maintains high school enrollment and leads to a significant reduction in high school dropouts.

As noted in Section 3, we might expect the direct effect of GDL laws on high school attendance, which functions through restricting teens' ability to commute to school, to differ across various subgroups. To some extent, the NCES analysis controls for average differences across school districts. However, it seems likely that teens from rural areas or from low-income backgrounds might experience larger direct effects. We next return to the CPS data to investigate this type of potential heterogeneity.

### 4.3 Heterogeneity Analysis

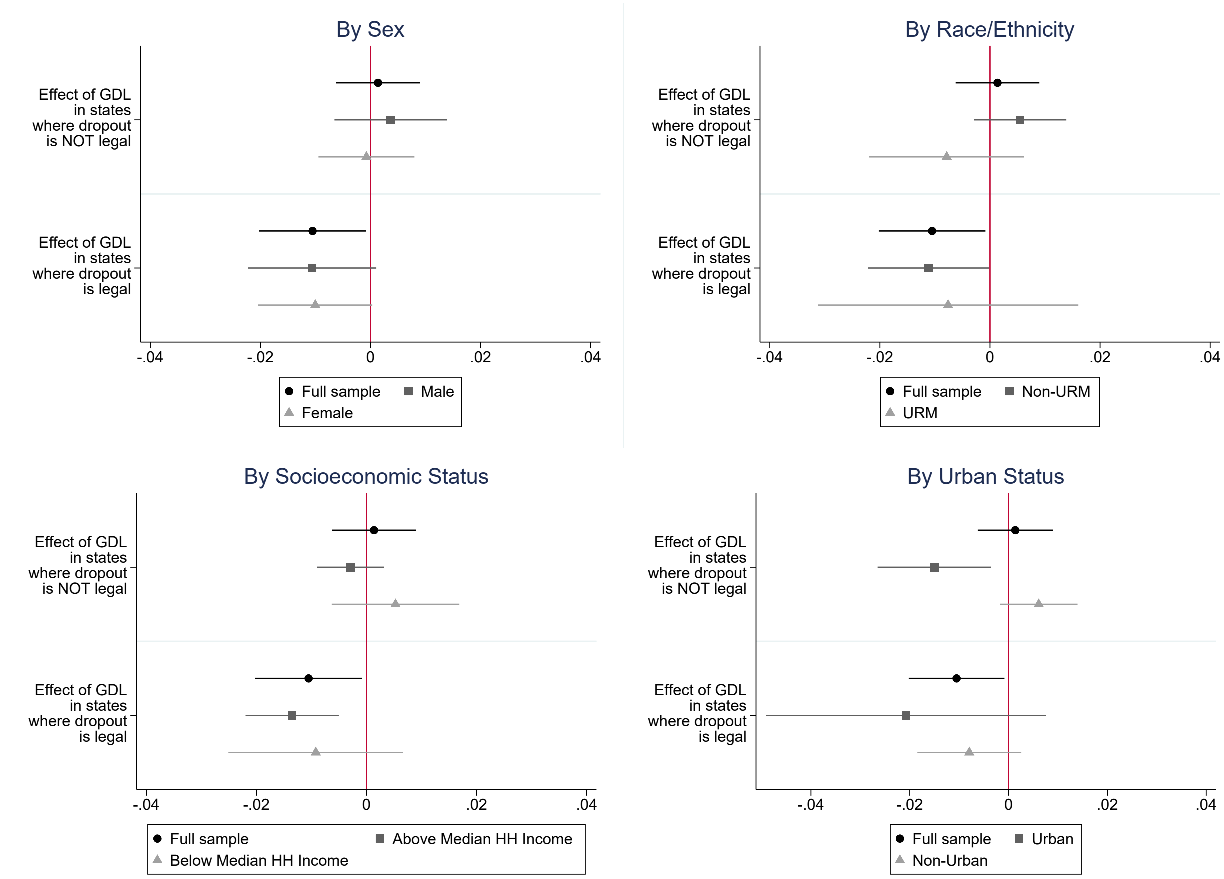
We estimate our preferred specification, given by Equation 1, separately for several subpopulations of interest. The marginal effects estimates are shown in Table 4, which also reports mean outcome values for each subgroup, and Figure 3. The top-left panel of Figure 3 shows the effects of GDL laws on 16-year-old dropout separately for males and females. The top three estimates show the effects of GDL laws in states where dropout is not legal ($\beta_1$) for the full sample, for male teens only, and for female teens only. The bottom three estimates show the effects of GDL laws in states where dropout is legal for 16-year-olds ($\beta_1 + \beta_3$) for those same three populations. It is clear from these estimates that there are no meaningful differences in the effects of GDL laws by sex and a Wald test reveals that the estimates are also not statistically different.[29]

We next examine heterogeneity by race and household income. Heterogeneity in effects among these groups could reflect differential vehicle availability to teens, or could also reflect differential reliance on a vehicle, if available. For example, a lower-income

[28] The NCES data is reported at the district-by-grade level and the estimates from this analysis thus combine the GDL effects on students of various ages, some of whom might be directly impacted by the law change and others who are not. It is therefore unsurprising that these estimates are smaller in magnitude than those reported in Table 2.

[29] Due to the difficulties of testing for equality of marginal effects estimates across samples in the probit specification, we instead test for equality across samples using linear probability model estimates.

Figure 3: The Effect of Minimum Unrestricted Driving Age on 16-yo Dropout for Sub-Populations




Marginal effects evaluated at sample means from probit regression using CPS ASEC data from 1990–2017. Bars show 95% confidence intervals. All specifications include state and year fixed effects. Controls include: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; and state log real effective minimum wage. Standard errors are clustered at the state level.

household may be less able to purchase or afford a vehicle for teen use. If vehicle take-up for teens in lower-income households is ex ante low, there would be less margin for GDL policies to shift behavior. At the same time, teens in lower-income households may have less access to alternatives to driving, such as parental transportation. This would suggest increased exposure to changes wrought by GDL laws and potentially larger effects.

In the top-right panel of Figure 3 (and columns (4)–(5) of Table 4) are effects of GDL laws estimated separately for underrepresented minorities (teens who identify as Black, Hispanic, or Native American) and all other race/ethnicity groups (non-URM). These estimates reveal that the negative impact of GDL laws on high school dropout is largely driven by non-URM 16-year-olds, who typically have a lower average dropout rate. The estimates for URM teens are quite noisy and much smaller than in the overall population. These results may reflect greater access to vehicles related to wealth or household income, or a greater affinity for car culture among non-URM families.

In the bottom-left panel of Figure 3 (and columns (6)–(7) of Table 4), we split the sample into two halves based on household income (as reported in the CPS). The median household income is $53,236 (in 1999 dollars). Sixteen-year-olds in lower-income households are more than twice as likely to be observed as not in school than those in higher-income households. However, the estimated effects of GDL laws are noticeably smaller and less precise for the lower-income sub-sample (despite having the same sample size). This provides some support to the hypothesis that teens from lower-income backgrounds are more likely to experience direct effects of the GDL laws making travel to school more difficult and therefore increasing the probability of dropout. Those (positive) direct effects would then counterbalance the (negative) indirect effects and lead to a combined effect that is closer to zero. An alternative explanation is greater vehicle availability for teens in higher-income households, for whom GDL laws decrease the probability of high school dropout by 56% at the mean. Note, however, that the difference in the estimates across the lower-income and higher-income groups is not statistically significant.

Finally, the bottom-right panel of Figure 3 (and columns (8)–(9) of Table 4) show the effects of GDL laws estimated separately for teens living in urban and non-urban areas. For teens in urban locations, the effects of GDL laws on high school dropout are negative and significant even when compulsory schooling laws make dropout illegal for the 16-year-olds in our sample. This suggests that access to the automobile may provide even greater access to educational distractions in urban areas; GDL laws so greatly reduce access to these activities that CS laws do not modulate their effect.

Table 4: The Effect of Minimum Unrestricted Driving Age on 16-yo Dropout for Sub-Populations

| | Not In School = 1 | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Full Sample | Men | Women | Non-URM | URM | HH Income $\geq$ Median | HH Income $<$ Median | Non-Urban | Urban |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Min. Unres. Driving Age >16 ($\beta_1$) | 0.0014 | 0.0037 | -0.0007 | 0.0055 | -0.0079 | -0.0029 | 0.0053 | 0.0061 | -0.0150** |
| | (0.0039) | (0.0052) | (0.0044) | (0.0043) | (0.0072) | (0.0031) | (0.0059) | (0.0040) | (0.0059) |
| School-Leaving Age $\leq$ 16 ($\beta_2$) | 0.0182*** | 0.0225*** | 0.0142*** | 0.0184*** | 0.0234** | 0.0137*** | 0.0228** | 0.0163*** | 0.0281** |
| | (0.0047) | (0.0065) | (0.0053) | (0.0054) | (0.0105) | (0.0043) | (0.0099) | (0.0048) | (0.0110) |
| Min. Unres. Driving Age >16 | -0.0119** | -0.0142** | -0.0093* | -0.0166*** | 0.0002 | -0.0106*** | -0.0145* | -0.0140*** | -0.0058 |
| $\times$ School-Leaving Age $\leq$ 16 ($\beta_3$) | (0.0048) | (0.0064) | (0.0050) | (0.0055) | (0.0107) | (0.0040) | (0.0088) | (0.0049) | (0.0136) |
| **Effect of GDL if** | -0.0105** | -0.0106* | -0.0100* | -0.0111** | -0.0076 | -0.0135*** | -0.0092 | -0.0079 | -0.0208 |
| **School-Leaving Age $\leq$ 16** ($\beta_1 + \beta_3$) | (0.0049) | (0.0059) | (0.0053) | (0.0056) | (0.0121) | (0.0043) | (0.0081) | (0.0054) | (0.0145) |
| Mean Outcome | 0.038 | 0.040 | 0.035 | 0.032 | 0.050 | 0.024 | 0.051 | 0.035 | 0.046 |
| Obs | 75,196 | 38,587 | 36,609 | 52,641 | 22,441 | 37,598 | 37,598 | 59,227 | 15,897 |

Marginal effects evaluated at sample means from probit regression using CPS ASEC data from 1990–2017. All specifications include state and year fixed effects. Controls include: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; and state log real effective minimum wage. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

## 4.4 Variation in GDL Intensity

We next investigate potential mechanisms to explain why increasing the minimum driving age reduces the probability of high school dropout in states where teens can legally drop out. The negative estimate of the net effect of GDL laws indicates that any direct effect of GDL laws on commuting to high school is more than completely offset by indirect effects stemming from reduced access to labor and leisure activities. We can further tease this apart using variation in the intensity of GDL laws.

As discussed in Section 2, GDL laws create an intermediate licensing level that restricts nighttime driving and/or restricts the number of passengers who may ride with a teen driver. Our binary measure of GDL laws ($GDL_{st} = 1$ if the minimum unrestricted driving age is $> 16$) encompasses two levels of mobility restrictions: (A) state-years where 16-year-olds have access only to an intermediate license; and (B) state-years where 16-year-olds do not have access to any level of license (except perhaps a learner's permit). When teens have access to the intermediate license, it is unlikely that we would observe a direct effect of the GDL law on the dropout decision. Because the intermediate license primarily restricts nighttime driving and carpooling it seems less likely that this type of GDL restriction would hinder the teen's ability to commute to school. On the other hand, when a teen has no access to driving, we expect to see both an indirect channel from reduced access to labor and leisure activities as well as the direct channel stemming from limiting transportation to and from school.

We estimate the following model to allow for these different levels of mobility restriction within GDL laws:

$$\begin{aligned} NotInSchool_{ist} = {} & \beta_1^A IntLicense_{st} + \beta_1^B NoLicense_{st} + \beta_2 CS_{st} \\ & + \beta_3^A IntLicense_{st} * CS_{st} + \beta_3^B NoLicense_{st} * CS_{st} \\ & + X_i'\nu + Z_{st}'\mu + D_s + D_t + \epsilon_{ist}. \end{aligned} \tag{3}$$

This specification is identical to Equation 1, except that we have replaced the single binary measure of GDL restrictions with two indicator variables corresponding to the two different levels of mobility restrictions. $IntLicense_{st}$ is an indicator variable that equals one if 16-year-olds in state $s$ in year $t$ can procure an intermediate driver's license *only* (and cannot obtain a full-privilege license until they are older). $NoLicense_{st}$ is an indicator variable that equals one if 16-year-olds cannot obtain either type of driver's license (intermediate or unrestricted). The omitted category comprises state-years where 16-year-olds

Table 5: Effects of Different Levels of Mobility Restrictions on 16-yo Dropout

| | Not In School = 1 | |
|---|---|---|
| | (1) | (2) |
| GDL at 16: | | |
| Intermediate License Only ($\beta_1^A$) | 0.0037 | 0.0028 |
| | (0.0043) | (0.0040) |
| No License ($\beta_1^B$) | 0.0022 | 0.0023 |
| | (0.0057) | (0.0052) |
| School-Leaving Age $\leq 16$ ($\beta_2$) | 0.0187*** | 0.0175*** |
| | (0.0049) | (0.0048) |
| GDL at 16 $\times$ School-Leaving Age $\leq 16$: | | |
| Intermediate License Only ($\beta_3^A$) | -0.0133*** | -0.0123*** |
| | (0.0046) | (0.0046) |
| No License ($\beta_3^B$) | -0.0019 | -0.0032 |
| | (0.0059) | (0.0059) |
| **Effect of Intermediate License Only if** | -0.0096* | -0.0095* |
| **School-Leaving Age $\leq$ 16** ($\beta_1^A + \beta_3^A$) | (0.0049) | (0.0049) |
| **Effect of No License if** | 0.0003 | -0.0008 |
| **School-Leaving Age $\leq$ 16** ($\beta_1^B + \beta_3^B$) | (0.0084) | (0.0080) |
| **Additional Effect of No License if** | 0.0099** | 0.0087* |
| **School-Leaving Age $\leq$ 16** ($\beta_1^B + \beta_3^B$)- ($\beta_1^A + \beta_3^A$) | (0.0047) | (0.0045) |
| Controls | - | Y |
| Obs | 75,196 | 75,196 |

Results from two-way fixed-effects regression using CPS ASEC data from 1990–2017. All specifications include state and year fixed effects. Controls in column (2) are: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; and state log real effective minimum wage. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

have access to unrestricted, full-privilege licenses. The marginal effects estimates from this expanded model are shown in Table 5.

As with the main results in Table 2, estimates of the placebo test in our expanded model ($\beta_1^A$ and $\beta_1^B$) are small and statistically insignificant under both levels of GDL restrictions. The estimate of $\beta_3^A$ indicates that the differential effect of having access to an intermediate license only for 16-year-olds in states where dropout is legally permitted is negative and statistically significant. The total effect of the restriction to an intermediate license on teen dropout behavior is estimated by the sum of coefficients, $\beta_1^A + \beta_3^A$. This sum reveals that limiting teen driving access to only the intermediate license level reduces the probability of high school dropout by 0.95pp in states where compulsory schooling laws are non-binding. Because the intermediate license is unlikely to hinder access to school transportation, we can interpret this negative effect as representing only the indirect channels. In other words, the reduction in access to labor and/or leisure activities caused by limiting 16-year-old driving privileges leads to a 25% reduction in the probability of high school dropout among this age group.

The estimate of $\beta_3^B$ indicates that the differential effect of having no access to driving for 16-year-olds in states where dropout is legally permitted (vs. states where the CS laws are binding) is negative but statistically insignificant. The total effect of the restriction to no license on teen dropout behavior is estimated by the sum of coefficients, $\beta_1^B + \beta_3^B$. This sum is almost precisely zero.[30] This estimate suggests that the negative effect of the GDL law on high school dropout stemming from reduced access to alternate activities is offset by a positive direct effect stemming from reduced ability to commute to school when teen access to driving is completely removed.

Also of interest here is the difference between the two total effect estimates, ($\beta_1^B + \beta_3^B$) - ($\beta_1^A + \beta_3^A$). This difference identifies the *additional* effect of going from a GDL law that restricts teens to an intermediate license only to a GDL law that fully restricts teen driving (at age 16). This estimate, a 0.87pp *increase* in the probability of high school dropout, again suggests that there is a significant direct effect of the GDL laws on teens' ability to commute to school that can lead to an increase in high school dropout if teen access to driving is completely removed. Note, however, that interpreting this point estimate solely as the direct effect requires the strong assumption that the indirect effect of fully restricting teen driving is no larger than the indirect effect of the intermediate license alone. Therefore, we take the estimates in Table 5 as merely an indication that both direct and indirect channels exist for this policy and rely on structural estimation to provide a more formal effect decomposition in Section 6.

[30]Note that only 12 states ever fully restricted access to driving for 16-year-olds during the time period under study. Thus, estimation of $\beta_1^B$ and $\beta_3^B$ relies on a relatively small number of observations.

# 5 Employment Results

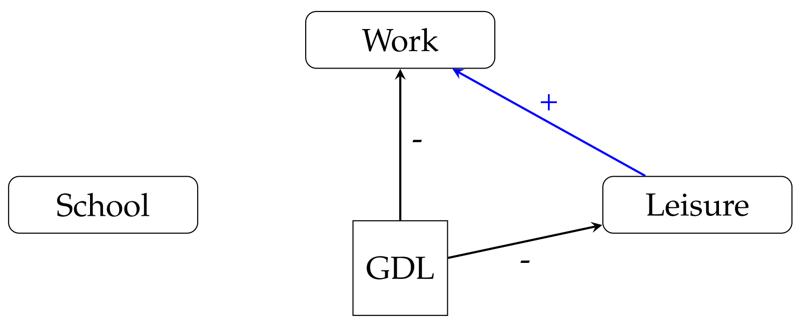
We next turn to an investigation of the effects of GDL laws on teen employment decisions. This analysis provides some evidence on whether our findings on high school dropouts are attributable, at least in part, to reduced access to labor force participation under GDL laws. We replace the dependent variable in Equation 1 with an indicator for whether the individual 16-year-old teen is currently in the labor force:

$$LFP_{ist} = \beta_1 GDL_{st} + \beta_2 CS_{st} + \beta_3 GDL_{st} * CS_{st} \\ + X_i' \nu + Z_{st}' \mu + D_s + D_t + \epsilon_{ist}. \tag{4}$$

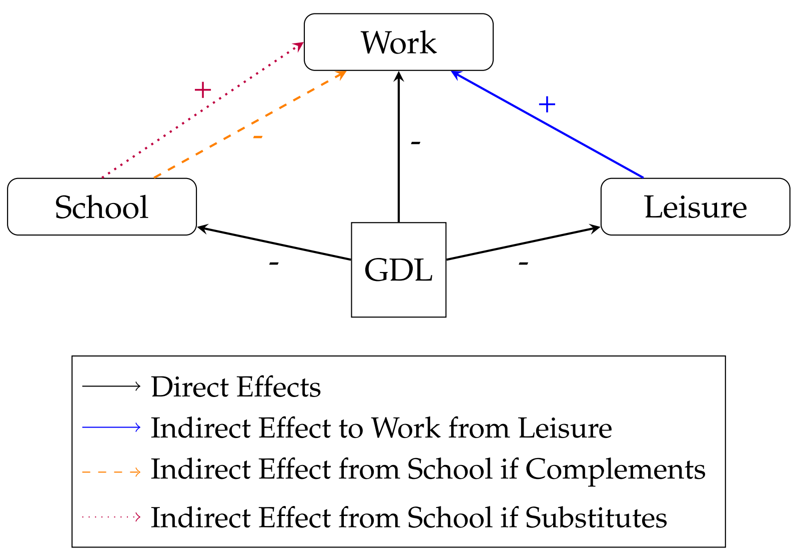
All other variable definitions are unchanged.

Figure 4: Direct and Indirect Effects of GDL Laws on Labor Force Participation

(a) When Teens Cannot Drop Out, $CS_{st} = 0$




(b) When Teens Can Legally Drop Out, $CS_{st} = 1$



In Figure 4, we illustrate the potential direct and indirect channels through which GDL laws might impact teen labor force participation. In panel (a), we consider the case where the teen resides in a state with compulsory schooling laws that do not permit dropping out at age 16. In this case, the restriction on teen driving imposed by the GDL laws will have a negative direct effect on employment. However, the GDL laws may also impact teen employment indirectly by limiting access to leisure activities. This indirect effect will have a positive effect on employment.[31] Because of the binding compulsory schooling laws, there is no effect of the GDL laws on the teen's schooling decision (and therefore, no indirect effect on teen employment coming through that channel). The coefficient $\beta_1$ in Equation 4 captures the sum of the direct effect and the indirect effect from leisure when CS laws prohibit 16-year-old drop out.

In panel (b) of Figure 4, we illustrate the case where CS laws are non-binding and 16-year-olds are legally permitted to drop out of school. This adds an additional channel through which GDL laws can impact teen labor force participation. Namely, the reduction in access to school may have an indirect effect on teen employment decisions. If teens view work and school as substitutes, then the indirect effect caused by reduced access to school contributes *positively* to labor force participation. If instead, they view work and school as complementary activities, then the indirect effect from the school channel contributes *negatively* to labor force participation. In Equation 4, $\beta_3$ captures this additional indirect channel between work and school.

Columns (1)–(2) of Table 6 show estimated average marginal effects from the triple-difference model in Equation 4. The effect of increasing the minimum driving age on labor force participation in states where 16-year-olds cannot legally drop out is negative, but also relatively small and statistically insignificant. As discussed above, this estimate reveals the sum of the (negative) direct effect of GDL laws on teen labor force participation and the (positive) indirect effect stemming from reduced access to leisure activities. We can therefore interpret this null finding as either an indication that neither of these two effects are very large, or that they are approximately equal in magnitude (and opposite in sign).

Conversely, the effect of GDL laws in states where teens are legally able to drop out is much larger and statistically significant: the probability of labor force participation drops by 1.7pp. At the mean, this is a 7% reduction in 16-year-old labor force partici-

[31]Note that we are implicitly assuming in Figure 4 that work and leisure are substitutes. Although this does seem like a reasonable assumption, we do not actually impose such a restriction in our estimation strategy.

Table 6: Effects of Minimum Unrestricted Driving Age on Teen Labor Force Participation

| | In Labor Force = 1 | | | | | |
|---|---|---|---|---|---|---|
| | Triple-Diff | | | | Diff-in-Diff | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Min. Unres. Driving Age >16 ($\beta_1$) | -0.0033 | -0.0024 | -0.0113 | 0.0010 | -0.0087 | -0.0075 |
| | (0.0102) | (0.0116) | (0.0145) | (0.0105) | (0.0079) | (0.0085) |
| School-Leaving Age $\leq$ 16 ($\beta_2$) | 0.0244 | 0.0183 | | | | |
| | (0.0160) | (0.0168) | | | | |
| Min. Unres. Driving Age >16 $\times$ School-Leaving Age $\leq$ 16 ($\beta_3$) | -0.0138 | -0.0149 | -0.0195 | -0.0211 | | |
| | (0.0130) | (0.0141) | (0.0199) | (0.0129) | | |
| **Marginal Effect of GDL if School-Leaving Age $\leq$ 16** ($\beta_1 + \beta_3$) | -0.0171* | -0.0173* | -0.0308** | -0.0202* | | |
| | (0.0100) | (0.0101) | (0.0140) | (0.0108) | | |
| School-Leaving Age | As Observed | | Never Switchers Only | Fixed in Yr. of GDL Change | - | - |
| Controls | - | Y | Y | Y | - | Y |
| Obs | 75,196 | 75,196 | 46,567 | 75,196 | 75,196 | 75,196 |

Marginal effects evaluated at sample means from probit regression using CPS ASEC data from 1990–2017. All specifications include state and year fixed effects. Controls in columns (2) and (4) are: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; and state log real effective minimum wage. Column (4) also includes indicators for the state minimum legal dropout age. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

pation (about one quarter of 16-year-olds work in this sample; see Table 1).[32] Moreover, results estimated using a linear probability model are similar to these probit results but slightly larger in magnitude and substantively more significant (see Appendix Table B.2). This negative estimate indicates that allowing for the additional channel of high school dropout creates a negative indirect effect on teen labor force participation. In other words, when GDL laws reduce access to school, the negative direct effect on school-going also leads to a negative indirect effect on the propensity to work. This is consistent with a model in which teens view work and school as complementary activities, rather than as substitutes.

We show two robustness checks in columns (3)-(4) of Table 6. Column (3) shows estimates when the sample is restricted to states that did not change their minimum school-leaving age during the time period under study, while Column (4) replaces $CS_{st}$ with a time-invariant measure that is fixed at each state's minimum school-leaving age in the

[32]Results are similar if we replace the dependent variable with an indicator for employment rather than labor force participation.

year that the GDL law first increases the minimum unrestricted driving age to greater than 16 or (if the state never changes) in 1990. Results from both exercises are stronger than the baseline estimates.

Columns (5)–(6) of Table 6 show the marginal effects estimates resulting from the difference-in-differences model that excludes the interaction term, $\beta_3 GDL_{st} * CS_{st}$. These results indicate that increasing the unrestricted driving age to greater than 16 weakly reduces labor force participation for 16-year-olds by approximately 0.8pp, though this effect is imprecisely measured. This estimate is, again, an average of the effects for teens that can legally dropout and those that cannot. Moreover, these results are qualitatively similar to those in Argys, Mroz, and Pitts (2019), though somewhat smaller in magnitude.[33]

These results indicate that, when teens are required to stay in school, the impact of GDL restrictions is, at most, a weak reduction in teen labor force participation. However, when teens are at liberty to drop out of school, they significantly reduce labor force participation in response to the GDL laws. This strongly suggests that there is an indirect channel linking teens' decisions regarding schooling and work when they are faced with mobility restrictions. However, GDL laws may also restrict access to other activities besides work and school, and the estimates in Table 2 could reflect substitution away from those activities as well. We next turn to a formal discrete choice model to better understand these findings.

# 6 Distinguishing Channels with Model-Based Analysis

We have provided ample evidence that the positive effects of GDL laws on high school retention likely reflect indirect channels caused by decisions regarding work or leisure activities that dominate any direct effects on access to school. Which of these indirect channels is most important depends on whether school and work are complements or substitutes in teens' utility functions. If school and work are highly substitutable, then the negative effect of GDL laws on high school dropout rates likely reflects a reduction in teen labor force participation. If not, then the change in dropout behavior can be attributed to changes in access to other activities. Distinguishing between these two types of indirect channels has important implications for future policy recommendations.

[33]The analysis in Argys, Mroz, and Pitts (2019) differs from ours in several respects including a different data sample and observation window. Argys, Mroz, and Pitts also probabilistically assign treatment (GDL laws) to monthly employment data and do not consider compulsory schooling laws.

We develop a model that disentangles these channels. Agents choose between work, school, both work and school, or neither activity, and school and work can be complements or substitutes (similar to Gentzkow 2007).[34] Agents have idiosyncratic preferences for school and for work and these preferences may be correlated. Exclusion restrictions separately identify this correlation from the complementarity or substitutability of school and work. GDL laws can differentially impact each choice–including the outside option (choosing neither work nor school) —and separate direct from indirect effects. Allowing GDL laws to impact the outside option requires a non-standard normalization, which we set identify using auxiliary sign restrictions. Thus, the model has similarities to Montmarquette, Viennot-Briot, and Dagenais (2007) or a static version of Eckstein and Wolpin (1999), but where we focus on estimating and decomposing treatment effects and cleanly identifying school-work spillovers.

Denote work and school as $A$ and $B$, respectively. Each agent $i$ chooses to partake in one activity, both, or neither; their choice set is $(y_i^A, y_i^B) \in \{0,1\}^2 = \mathcal{C}$. Agents receive indirect utility $\tilde{V}_i(y_i^A, y_i^B)$ from each bundle. The normalized indirect utility that agent $i$ obtains from each choice is:

$$V_i(0,0) = \tilde{\gamma}^0 GDL_{st}^0 \tag{5}$$
$$V_i(1,0) = \alpha^A + (\gamma^A + \tilde{\gamma}^0) GDL_{st}^A + x_{ist}'\lambda^A + z_{st}'\pi^A + f^A(s,\xi) + \delta_t^A + e_i^A \tag{6}$$
$$V_i(0,1) = \alpha^B + (\gamma^B + \tilde{\gamma}^0) GDL_{st}^B + x_{ist}'\lambda^B + z_{st}'\pi^B + f^B(s,\xi) + \delta_t^B + e_i^B \tag{7}$$
$$V_i(1,1) = V_i(1,0) + V_i(0,1) + \Gamma + (\gamma^\Gamma - \tilde{\gamma}^0) GDL_{st}^\Gamma, \tag{8}$$

where $\gamma^{k_+}$ are the parameters of interest intended to capture the utility effect of the graduated driver license policy, for $k_+ \in \{0, A, B, \Gamma\}$. We index $GDL$ by $k_+$ to anticipate the decomposition; each individual experiences only one value $GDL_{st}^{k_+} = GDL_{st}$, but this does not hold in the decomposition.

The idiosyncratic terms reflect the latent indirect utility associated with each activity, and may be correlated. Thus, $e_i^A$ can be interpreted as motivation to work or expected labor market returns, and $e_i^B$ can be interpreted as motivation for school or expected returns to schooling. Teens with low values of both terms have a high value of leisure. Comple-

[34] Because the activities do not have observed pecuniary costs, they are substitutes (complements) in the sense that restricting access to one activity increases (reduces) demand for the other activity. An alternative model is bivariate probit with both outcomes endogenous. However, Lewbel (2007) shows that such a model is generally incoherent and/or incomplete. Recent work explores new combinatorial discrete choice methods (Arkolakis and Eckert 2017).

mentarity or substitutability between school and work is captured by $\Gamma+\gamma^{\Gamma}GDL_{st}$, which is $>0$ if the activities are relative complements and $<0$ if relative substitutes. This term captures the (utility-valued) effect of school-going on work (and vice versa).

State-year characteristics, $z_{st}$, separately identify $\Gamma+\gamma^{\Gamma}GDL_{st}$ from the correlation between $e^A$ and $e^B$, where

$$z'_{st}=\left[UR_{st},\ln(MW_{st}),CS_{st},GDL^B_{st}\times CS_{st}\right].$$

Here, $UR_{st}$ is the state-level unemployment rate, $\ln(MW_{st})$ is log real minimum wage, and $CS_{st}$ and $GDL_{st}$ are measures of the compulsory schooling laws and graduated driver licensing laws, as in Equation 1. The $f^k(s,\xi)$ terms include correlated random effects (discussed below) and $\delta^k_t$ represent year dummies, $k\in\{A,B\}$. The model also includes individual characteristics $x_{ist}$: gender, race/ethnicity indicators, mother's education, presence of father in household, and receipt of SNAP benefits. Agents choose the bundle that maximizes utility: $\max_{(y^A_i,y^B_i)}V_i(y^A_i,y^B_i)$. The probability that agent $i$ chooses bundle $c\in\mathcal{C}$ is the probability that $i$'s utility from $c$ is greater than that from all other choices $c'$: $P^c_i=\Pr\left(V_i(c)\geq V_i(c'),\forall c'\in\mathcal{C}\right)$.

This model features a non-standard normalization.[35] Because discrete choice models only identify *relative differences* in utility, the utility of one choice is typically normalized to zero (e.g., $V_i(0,0)=0$). Such a normalization does not affect model fit or identification, but makes the implicit assumption the the normalized option is not altered by treatment. However, the literature relating changes in teen risky behaviors to GDL law adoption strongly suggests that the value of the neither-work-nor-school option was shifted by the implementation of GDL policies (e.g., Deza and Litwok 2016; Deza 2019; Huh and Reif 2021). Accordingly, we normalize by interacting an auxiliary parameter, $\tilde{\gamma}^0$, with the policy, $V_i(0,0)=\tilde{\gamma}^0GDL^0_{st}$, to allow the GDL policy to directly impact the utility of the outside option. We then set the utility for each other option to: $V_i(y^A_i,y^B_i)=\tilde{V}_i(y^A_i,y^B_i)-\tilde{V}_i(0,0)+\tilde{\gamma}^0GDL^0_{st}$. The observed data is compatible with any value of the auxiliary parameter; $\tilde{\gamma}^0$ merely redistributes the incidence of the direct effects of the GDL laws to the outside option.

The model decomposes total effects into their direct and indirect channels. **Total effects** of GDL laws on each activity capture the overall impact of increasing the minimum unrestricted driving age from 16 or less. Total effects are the model-based analogs to the

[35]Our model nests the standard normalization, which occurs when $\tilde{\gamma}^0=0$.

reduced-form estimates shown in Section 4 and Section 5. **Direct effects** (denoted $\theta^k_{\text{Dir}}$) reflect how each GDL component affects its *own activity*, e.g., the effect of $GDL^A$ on working and of $GDL^B$ on school enrollment. **Indirect effects** capture the consequences of the direct changes in utility of GDL laws on one activity to the *other activities*, i.e., of $GDL^0$, $GDL^B$, and $GDL^\Gamma$ on working, or $GDL^0$, $GDL^A$ and $GDL^\Gamma$ on schooling decisions.[36] While total effects are invariant to $\tilde{\gamma}^0$, direct and indirect effects are not.

### 6.1 Identification and Estimation

We make the following assumptions to identify and estimate the model parameters:

**Assumption 1** (Idiosyncratic Preferences are Bivariate Normal)**.** *Idiosyncratic preferences are independent and are distributed bivariate normal:* $\mathbf{e}_i = [e_i^A \;\; e_i^B]' \sim N(0, \Omega)$*, where*

$$\Omega = \begin{pmatrix} 1 & \rho\sigma \\ \cdot & \sigma^2 \end{pmatrix},$$

*such that the scale of the idiosyncratic preference is normalized to activity $A$ (work).*

**Assumption 2** (Exclusion Restrictions)**.** *Components of $z$ may shift the utility of at most one of $A$ or $B$. Specifically,*

$$\pi^{A'} = \left[\pi^A_{UR}, \pi^A_{MW}, 0, 0\right]$$
$$\pi^{B'} = \left[0, 0, \pi^B_{CS}, \pi^B_{GDL\times CS}\right].$$

**Assumption 3** (Correlated Random Effects)**.** *The state-specific unobserved effects $f^k(s,\xi)$ for $k \in \{A, B\}$ are correlated with $GDL_{st}$, $x_{ist}$, and $z_{st}$ in the following manner:*

$$f^A(s,\xi) = \xi_1^k \overline{GDL}_s + \overline{x}_s'\xi_2^k + \overline{z}_s'\xi_3^k,$$

*where $\bar{\cdot}_s$ indicates an average across observations in state $s$.*

Assumption 1 imposes the structure of a multinomial probit model (e.g., Goolsbee and Petrin 2004) with the addition of multiple discreteness (Gentzkow 2007). The exclusion restrictions (Assumption 2) separately identify $\Gamma$ from $\rho$ and strengthen identification of

[36] We provide precise definitions in Appendix E. There are several reasonable ways to define these effects; our definition preserves additivity such that total effects are the sum of direct and indirect effects.

the model more generally.[37] Assumption 3 imposes a correlated random effects structure on the model, allowing for parametric correlation between unobserved state-specific factors and observable covariates (Mundlak 1978). This helps ensure that estimates of $\gamma^{k+}$ are not confounded by other state-specific factors that may be correlated with the implementation of GDL policies.[38]

These three assumptions are sufficient to identify all model parameters except for $\tilde{\gamma}_0$. Because $GDL_{st}^{k+} = GDL_{st}$ for estimation, choice probabilities are invariant to $\tilde{\gamma}_0$. Thus, it is impossible to identify $\tilde{\gamma}_0$ from observed choice data alone. Conversely, all other parameters are unaffected by the value of $\tilde{\gamma}_0$. However, identification of $\tilde{\gamma}_0$ will be important for the decomposition of the total effects into direct and indirect channels. To set identify this parameter, we make an assumption on the sign and relative size of the direct effects in the model:

**Assumption 4** (Normalization)**.** *Let $\tilde{\gamma}^0$ be such that the indirect utility impact of GDL laws on neither, work, and school are weakly negative ($\tilde{\gamma}^0 \leq 0$, $\gamma^A + \tilde{\gamma}^0 \leq 0$, and $\gamma^B + \pi^B_{CS\times GDL} + \tilde{\gamma}^0 \leq 0$) and that the direct effect on schooling is no larger in magnitude than the direct effect on work $\left(|\theta^B_{Dir}| \leq |\theta^A_{Dir}|\right)$. That is, $\tilde{\gamma}^0 \in \mathcal{G}$, where*

$$\mathcal{G} = \{g : \left(|\theta^A_{Dir}(g)| < |\theta^B_{Dir}(g)|\right) \wedge \left(g \leq \min\{0, -\gamma^A, -(\gamma^B + \pi^B_{CS\times GDL}\}\right)\}.$$

These assumptions are independently reasonable. First, direct effects are likely weakly negative because GDL laws do not increase access to any activity; each activity has become weakly harder to access. Second, direct employment effects are likely larger in magnitude than direct schooling effects because there are a number of transportation alternatives to access school (e.g., school buses) that may not be available for work access.

To estimate the model, we rely on **Lemma 1** (see proof in Appendix E), which asserts that, under Assumption 1, the model given by Equations (5)–(8) can be estimated with a Geweke, Hajivassiliou, and Keane (GHK) simulator.[39] We estimate the model using maximum simulated likelihood to identify all parameters except $\tilde{\gamma}_0$. These parameters

[37] See Appendix E for details.

[38] Correlated random effects models are distinct both from standard random effects models and from random coefficients models. Correlated random effects are intuitively similar to fixed effects, but are more convenient in nonlinear settings. In fact, in linear models, fixed effects and correlated random effects are numerically equivalent (Mundlak 1978).

[39] This result is somewhat unexpected, as the error covariance structure of the four-choice multinomial probit model implied by Equations (5)–(8) is not positive definite. For a description of the GHK simulator, see Train (2009).

Table 7: Key Model Parameters

| | | | Work | | School | | | | |
|---|---|---|---|---|---|---|---|---|---|
| $\rho$ | $\sigma$ | $\Gamma$ | $\alpha^A$ | $\gamma^A$ | $\alpha^B$ | $\gamma^B$ | $\pi^B_{CS}$ | $\pi^B_{CS\times GDL}$ | $\gamma^\Gamma$ |
| -0.4690 | 0.0233 | 0.0119 | -0.6222 | -0.0268 | 0.0131 | 0.0004 | -0.0055 | 0.0034 | -0.0022 |
| (0.0012) | (0.0004) | (6.03e-5) | (0.0122) | (0.0005) | (2.52e-5) | (5.34e-5) | (0.0001) | (3.71e-5) | (2.89e-5) |

Point estimates of key model parameters estimated via maximum simulated likelihood using a GHK simulator and limited-memory BFGS optimization algorithm with 250 draws per observation of idiosyncratic preferences. Standard errors (in parentheses) are calculated from the Hessian and are not robust to correlation or clustering. Observations are weighted using sample weights.

are sufficient to estimate total effects and to determine $\mathcal{G}$ (given Assumption 4), which we then use to set identify the direct and indirect effects.

## 6.2 Model Results

Table 7 shows estimates of ten key model parameters.[40] Non-policy parameters of particular interest are the correlation of idiosyncratic preferences for school and work, $\rho$, and the complementarity between activities, $\Gamma$. The negative estimate of $\rho$ (-0.47) indicates negative correlation in the 'types' of teens that choose school or work. Those who receive a high (utility) value from school are more likely to receive low value from work. Conversely, those receiving the highest utility from work are less likely to find school valuable. Despite this, the estimate of $\Gamma > 0$ indicates that school and work are weak complements: decreasing access to school mildly decreases the value of work (and vice versa). This is the first key piece of evidence that the decline in employment and increase in school-going in response to GDLs does not primarily reflect substitution between these two activities. It also highlights the importance of identifying the negative correlation in preferences for school-going and work. Failing to account for $\rho < 0$ would make working while in high school appear more deleterious for school-going than it actually is.

These results both confirm and contrast previous findings. Eckstein and Wolpin (1999) and Montmarquette, Viennot-Briot, and Dagenais (2007) both find evidence of negative correlation in preferences for school and work ($\rho < 0$), although Montmarquette, Viennot-Briot, and Dagenais (2007) find evidence that an additional preference for good grades undoes some of this relationship. The results in Eckstein and Wolpin (1999) for 16 year-old

[40] Table E.1 assesses model fit by comparing how often a simulated choice matches the observed choice (averaged over 100 draws of $\mathbf{e}$). The model returns choice shares that deviate by less than 0.02pp from the observed sample. Overall, the model correctly classifies those in the sample 62.2% of the time. Given the large number of idiosyncratic factors that we do not observe, we believe this to be reasonable.

teens indicate that there is a negative psychic cost of participating in both school and part-time work simultaneously, although this substitutability decreases with age. However, Montmarquette, Viennot-Briot, and Dagenais (2007) find evidence that, at least for some high school seniors, school and work are complementary. This is supported by Ruhm (1997), who shows that part-time work has no negative effect on educational outcomes.[41] Relative to this literature, we are able to separately identify both $\rho$ and $\Gamma$, lending credibility to the narrative that teens' preferences for school-going and work are negatively related but that school and work are not substitutes, at least on average.

The policy parameters ($\gamma$ and $\pi$) are qualitatively consistent with results in Section 4 and Section 5. The utility effect of GDL laws on teen labor force participation is larger than the corresponding effect on high school enrollment, both in absolute levels and in terms of standard deviations of idiosyncratic preference ($|\gamma^A| > |\frac{\gamma^B}{\sigma}|$). Legalizing high school dropout (by instituting a lower minimum school-leaving age) substantially decreases the relative indirect utility of attending school. However, the interaction of legalizing school-leaving and restricting mobility (through GDL laws) partially reverses that reduction in relative utility. Finally, GDL laws mildly reduce the complementarity between school-going and work, as indicated by the negative value on $\gamma^\Gamma$.

We show model-based equivalents of the design-based treatment effects estimated in Sections 4 and 5 as **total effects** in the top row of Table 8. The model predicts that adopting a GDL law when school-leaving is legal increases the probability of being enrolled in school by 1.31pp and decreases the probability of labor force participation by 0.83pp.[42] These results are roughly in line with those in prior sections, though the magnitudes differ a bit. This is to be expected, as the model incorporates additional information by modeling the entire decision space, while also imposing additional structure via the correlated preferences and exclusion restrictions. The model suggests that GDL policies reduce the likelihood of the "neither work nor school" option by about -1.06pp, or about 44% from the mean.[43] We interpret this "neither" option as reflecting teen preferences for leisure activities, which encompass both risky behaviors as well as less-risky forms of truancy. It is then unsurprising that the estimated reduction in this category is somewhat larger than previously estimated effects found in the literature on the impacts of GDL laws and teen

[41] Relatedly, Light (1999) finds that the effect of high school employment on subsequent earnings for men is small and relatively short-lived.

[42] Counterfactuals impose the triple-difference design and estimate effects assuming teens have the option to drop out.

[43] In our estimation sample, 2.4% of 16-year-olds are neither working nor in school and 23.1% are both in school and working.

Table 8: Decomposition of GDL Law Effects by Activity

| | Effect of GDL Laws on: | | | | | |
|---|---|---|---|---|---|---|
| | Neither | | Work | | School | |
| | Effect | % of Total | Effect | % of Total | Effect | % of Total |
| **Total effect** | -1.06pp | | -0.83pp | | 1.31pp | |
| **A. Upper-bound renormalization** $\tilde{\gamma}^0 = \min\{0, -\gamma^A, -(\gamma^B + \pi^B_{CS \times GDL})\}$. | | | | | | |
| Direct | -1.13pp | 106.7% | -0.88pp | 106.6% | 0pp | 0.0% |
| Indirect | 0.07pp | | 0.05pp | | 1.31pp | |
| *via Neither* | - | | 0.01pp | -1.2% | 1.13pp | 86.1% |
| *via Other activities* | 0.07pp | -6.7% | 0.04pp | -5.4% | 0.18pp | 13.9% |
| **B. Lower-bound renormalization** $\tilde{\gamma}^0 : \theta^A_{\text{Dir}} = \theta^B_{\text{Dir}}$. | | | | | | |
| Direct | -1.56pp | 146.8% | -0.93pp | 112.8% | -0.93pp | -71.3% |
| Indirect | 0.50pp | | 0.11pp | | 2.24pp | |
| *via Neither* | - | | 0.01pp | -1.7% | 1.78pp | 136.0% |
| *via Other activities* | 0.50pp | -46.8% | 0.09pp | -11.1% | 0.46pp | 35.3% |

These are the simulated total, direct, and indirect effects of policy counterfactuals using parameters shown in Table 7 averaged over 100 draws $\mathrm{e}_i$ per person. To match the triple-difference design, for all counterfactuals $CS_{st} = 1$ (and so $GDL^B_{st} \times CS_{st} = GDL^B_{st}$). Observations are weighted using sample weights.

driving on risky behaviors (Deza and Litwok 2016; Deza 2019; Huh and Reif 2021).

Table 8 also shows the decomposition of each total effect into direct and indirect channels for $\tilde{\gamma}_0$ at the upper and lower boundaries of $\mathcal{G}$. We further decompose the indirect effects for work and school into their root causes in italics: changes in the indirect utility of neither-work-nor-school or changes in the indirect utility of the other activity and the complementarity between the two activities.

Panel A of Table 8 shows the decomposition assuming $\tilde{\gamma}^0 = \sup \mathcal{G} = -0.0038$. Under this assumption, the renormalized impact of GDL laws on the indirect utility of school-going is 0, and thus so is its direct effect. All of the total effect on school-going is therefore indirect, and is primarily coming from a reduction in the utility of neither-work-nor-school. Only 14% of the total schooling effect is due to changing work access or complementarity effects. In contrast, the total effect of GDL laws on teen employment is entirely attributable to a direct effect, with only a small countervailing indirect effect mainly reflecting the complementarity between school and work. Similarly, most of the total effect of GDL laws on neither-work-nor-school is through the direct channel.

Panel B of Table 8 instead assumes $\tilde{\gamma}^0 = \inf \mathcal{G} = -0.0056$. In this scenario, the direct effects of GDL laws on labor force participation and school enrollment are, by assumption, equivalent. At this lower bound, the impact of GDL laws on the utility of school-going generates a direct effect of -0.93pp, but this is counteracted by a large indirect effect, again predominately due to the reduction in the utility of neither-work-nor-school. A much smaller portion of the total effect is due to the indirect channel stemming from reduced access to work and the declining complementarity between work and school. The decomposition of the total effect of GDL laws on teen employment is largely similar to that in Panel A, with only a slightly larger indirect effect due to school-going. The direct effect on neither-work-nor-school is quite large in this scenario.

In summary, the reduced access to employment created by GDL laws increases teen school-going by only 0.18pp–0.46pp, which accounts for at most 35% of the total impact of GDL laws on high school enrollment/dropout. The total effect of the GDL policy on school-going is instead attributable largely to the reduced utility from the neither-work-nor-school option.

This conclusion is crucial for interpreting the effects presented earlier in this paper and for future policy design. The reduction in high school dropout due to GDL laws is not caused by teens substituting away from employment, and is unlikely to mask large negative direct effects of GDL laws on access to high school. On the other hand, the estimated reduction in teen employment due to GDL laws seems to have been driven primarily by a reduction in teens' ability to commute to jobs. School and work are somewhat complementary, so policy design need not be overly concerned about substitution between the two. Future policies that specifically target access to non-work, non-school activities would therefore likely be able to preserve the negative dropout effect without inducing a corresponding negative effect on teen employment.

# 7 Conclusion

We interact graduated driver licensing and compulsory schooling laws to study the effects of mobility restrictions on schooling and employment outcomes for 16-year-olds in the United States. GDL laws were adopted by many states in the late 1990s, before the gradual ratcheting up of minimum legal dropout ages in the 2000s. This created a window of time during which teen automobility was restricted but when teens could choose to drop out of school. We use this window to determine whether mobility restrictions increase or decrease school-leaving in a setting in which students still have the option to leave school.

A robust set of results indicate that GDL laws—which restrict teen mobility—actually decrease high school dropout by about 1.1pp (a 28% reduction from the mean), but only in settings in which school-leaving is a legal option. This potentially surprising result contrasts with evidence from large, middle-income cities that transit expansions increasing school access improve educational outcomes (Asahi and Pinto 2022; Dustan and Ngo 2018). Our result instead suggests that access to other activities may have decreased even more than access to school in the US setting, leading to substitution towards schooling. To this end, we estimate the effect of GDL laws on teen labor force participation and find that these laws led to a 1.7pp (7% at the mean) reduction in 16-year-old labor force participation.

We turn to a structural model of multiple activity choice to help interpret these results. The model has its own set of identification and interpretation challenges, and our discussion of these may be useful for others combining policy analysis with structural modeling. The model separates the direct effects of the policy from indirect channels (through substitution or complementarity effects). Under reasonable assumptions, we find that the indirect impacts of GDL laws on schooling are not due to decreased access to work, but likely reflect decreased access to activities that are neither work nor school. This accords with the literature on GDL laws and risky behaviors.

Teen mobility restrictions offer a classic economic example of trade-offs in policy design. While the motivation for GDL laws was to increase teen safety, they had a number of other effects on teen behavior. We find an additional benefit on school-going, contributing to educational attainment. However, GDL laws also decreased teen work, which may itself have additional positive or negative consequences in the long run. Our decomposition of the total effects of GDL laws into direct and indirect channels offers important insight for future policy design. Namely, that policies limiting teen mobility might preserve the benefit to educational attainment, while avoiding the negative effect on teen employment by targeting access to non-work, non-school activities.

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# Appendix

## A GDL Laws and Teen Driving

To verify that GDL laws had a binding effect on teen automobile use, we estimate the effect of GDL roll-out on a proxy for driving.[44] We use the rate of fatal car accidents involving a teen driver as a proxy for the prevalence of teen driving by linking the GDL laws to data from the US Department of Transportation's Fatality Analysis Reporting System (FARS). FARS is a nationwide census of all fatal injuries suffered in motor vehicle crashes and provides data on the location and timing of the accident as well as the involved drivers' birth-years.

We collapse each year of FARS data into state-by-age-of-driver bins and calculate the number of car accidents involving a fatality for each bin. To convert these accident counts into rates, we use data from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) dataset, which includes estimates of year-by-age populations for every county. This allows us to create state-, year-, and age-specific measures of the fatal car accident rate. An advantage of this outcome is that FARS contains the universe of fatal car accidents in the United States over our entire sample period and includes all persons involved in accidents that result in a fatality, not just fatalities themselves.

We estimate the effect of increasing the minimum full-privilege driving license age on age-specific accident rates using a two-way fixed effects model:

$$AccRate_{16,st} = \beta GDL_{st} + D_s + D_t + \epsilon_{st}, \tag{A.1}$$

where $AccRate_{16,st}$ is the count of fatal car accidents in which at least one driver was aged 16 divided by the population aged 16 in state $s$ in year $t$ (in 1,000s). The primary variable of interest is $GDL_{st}$, which measures the minimum age at which teens can obtain a full driver's license with no restrictions. The model includes both state and year fixed effects and is weighted by the population aged 16 in state $s$ in year $t$. Standard errors are clustered at the state level.

Column (1) of Table A.1 shows that a one year increase in the minimum age at which teens can receive an unrestricted driver's license reduces the rate of fatal car accidents for

[44] Few data directly report teen automobile use, and none that we are aware of contain large samples of teens across states and over time.

drivers aged 16 by 0.032 accidents per thousand 16-year-olds in the (state's) population. At the mean (0.259 fatal accidents per thousand population aged 16), this is equivalent to a 12% reduction. In column (2), we replace the continuous measure of unrestricted driving age with an indicator variable that equals one if the minimum unrestricted driving age is strictly greater than 16 (corresponding to the solid, black line in Figure 1a). This yields an even larger negative estimate of 0.07 accidents per thousand 16-year-old population, indicating that teens are a statistically significant 27% less likely to be involved in a fatal car accident when they cannot access an unrestricted driver's license.

Table A.1: Effect of Minimum Driving Age on Fatal Car Accidents with Age 16 Drivers

| | Accidents per 1,000 | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Minimum Unrestricted Driving Age | -0.032*** | | |
| | (0.011) | | |
| Min. Unres. Driving Age > 16 (year t+2) | | | -0.013 |
| | | | (0.018) |
| Min. Unres. Driving Age > 16 (year t+1) | | | 0.009 |
| | | | (0.014) |
| Min. Unres. Driving Age > 16 | | -0.070*** | -0.022 |
| | | (0.016) | (0.015) |
| Min. Unres. Driving Age > 16 (year t-1) | | | -0.038*** |
| | | | (0.012) |
| Min. Unres. Driving Age > 16 (year t-2) | | | -0.018 |
| | | | (0.015) |
| Mean Outcome | | 0.259 | |
| Obs | 1,400 | 1,400 | 1,200 |

Specifications include state and year fixed-effects. Data are from FARS, are collapsed to state-year cells, and cover 1990–2017. All specifications are weighted by the total state population and standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

The results in Table A.1 indicate that the introduction of GDL laws significantly restricted teen driving de facto. In column (3) we also include two leads and two lags of the minimum driving age indicator variable as a test for whether we are merely picking up trends in teen driving behavior. We find no evidence of pre-trends, however, there does seem to be a slightly delay in the timing of the effect on fatal accident rates. This result provides a measure of confidence that we are conservatively assigning changes in GDL laws to the effective year or the year prior.

These findings accord with previous work showing that the implementation of GDL laws decreased teen driving fatalities (Dee, Grabowski, and Morrisey 2005). While our results likely reflect declines in teen driving, they may also capture changes in other margins of driving behavior, such as safety. However, Gilpin (2019) and Karaca-Mandic and Ridgeway (2010) show that decreases in driving fatalities stem primarily from reductions in teen driving rather than improvements in the quality of teen driving.[45] When taken in conjunction with our results, it appears that GDL laws did, in fact, restrict teen mobility.

[45]Relatedly, Severen and Benthem (2019) find that GDL laws do not appear to lead to long-run reductions in driving. Bostwick (2018) uses changes in school start times to show that teen driving safety is very responsive to outside factors, such as cognitive load and sleepiness as well as traffic congestion.

# B Additional Results

Table B.1: The Effect of Minimum Unrestricted Driving Age on Teen Dropout Decision (Linear)

| | Not In School = 1 | | | | | |
|---|---|---|---|---|---|---|
| | Triple-Diff | | | | Diff-in-Diff | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Min. Unres. Driving Age >16 ($\beta_1$) | 0.0019 | 0.0009 | 0.0022 | 0.0012 | -0.0030 | -0.0032 |
| | (0.0041) | (0.0039) | (0.0049) | (0.0040) | (0.0039) | (0.0036) |
| School-Leaving Age $\leq$ 16 ($\beta_2$) | 0.0207*** | 0.0196*** | | | | |
| | (0.0049) | (0.0048) | | | | |
| Min. Unres. Driving Age >16 | -0.0119** | -0.0108** | -0.0168** | -0.0110** | | |
| $\times$ School-Leaving Age $\leq$ 16 ($\beta_3$) | (0.0045) | (0.0046) | (0.0065) | (0.0052) | | |
| **Effect of GDL if** | -0.0101** | -0.0099** | -0.0145** | -0.0098* | | |
| **School-Leaving Age $\leq$ 16** ($\beta_1 + \beta_3$) | (0.0045) | (0.0047) | (0.0069) | (0.0049) | | |
| School-Leaving Age | As Observed | | Never Switchers Only | Fixed in Yr. of GDL Change | - | - |
| Controls | - | Y | Y | Y | - | Y |
| Obs | 75,196 | 75,196 | 46,567 | 75,196 | 75,196 | 75,196 |

Results from two-way fixed-effects regression using CPS ASEC data from 1990–2017. All specifications include state and year fixed effects. Controls in columns (2)–(4) and (6) are: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; and state log real effective minimum wage. Columns (4) and (6) also include indicators for the state minimum legal dropout age. Column (3) limits the sample to states that never changed school-leaving age, while Column (4) fixes school-leaving age to its level when the state increased minimum unrestricted driving age to >16. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

Table B.2: The Effect of Minimum Unrestricted Driving Age on Teen Labor Force Participation (Linear)

| | In Labor Force = 1 | | | | | |
|---|---|---|---|---|---|---|
| | Triple-Diff | | | | Diff-in-Diff | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Min. Unres. Driving Age >16 ($\beta_1$) | -0.0048 | -0.0045 | -0.0175 | 0.0017 | -0.0119 | -0.0110 |
| | (0.0119) | (0.0130) | (0.0140) | (0.0126) | (0.0099) | (0.0101) |
| School-Leaving Age $\leq$ 16 ($\beta_2$) | 0.0329** | 0.0287* | | | | |
| | (0.0146) | (0.0150) | | | | |
| Min. Unres. Driving Age >16 × School-Leaving Age $\leq$ 16 ($\beta_3$) | -0.0174 | -0.0173 | -0.0180 | -0.0315** | | |
| | (0.0130) | (0.0137) | (0.0171) | (0.0134) | | |
| **Marginal Effect of GDL if School-Leaving Age $\leq$ 16** ($\beta_1 + \beta_3$) | -0.0222** | -0.0218** | -0.0356** | -0.0298** | | |
| | (0.0109) | (0.0107) | (0.0149) | (0.0112) | | |
| School-Leaving Age | As Observed | | Never Switchers Only | Fixed in Yr. of GDL Change | - | - |
| Controls | - | Y | Y | Y | - | Y |
| Obs | 75,196 | 75,196 | 46,567 | 75,196 | 46,567 | 75,196 |

Results from two-way fixed-effects regression using CPS ASEC data from 1990–2017. All specifications include state and year fixed effects. Controls in columns (2)–(4) and (6) are: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; and state log real effective minimum wage. Columns (4) and (6) also include indicators for the state minimum legal dropout age. Column (3) limits the sample to states that never changed school-leaving age, while Column (4) fixes school-leaving age to its level when the state increased minimum unrestricted driving age to >16. Standard errors are clustered at the state-level. * p<0.10, ** p<0.05, *** p<0.01

# C Robustness Analyses

In this section, we address the literature that has identified biases in two-way fixed effects estimation of staggered adoption difference-in-differences research designs (e.g., Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021; Sun and Abraham 2021). One source of such bias highlighted by Goodman-Bacon (2021) is that the two-way fixed effects estimator for a difference-in-differences identification strategy implicitly uses previously-treated cohorts to estimate counterfactual outcomes for later-treated cohorts. This can be problematic if treatment effects are changing over time. Given that our data covers 28 years and that there are changes in GDL laws over several years, we are able to consider subsets of the full study window in order to probe whether our estimated treatment effects are dynamic or static.

Specifically, we introduce two sample restrictions into the estimation of the probit model in Equation 1. First, we remove states that are "always-treated" in our study window (i.e., adopted a GDL law restricting full privilege licenses to teens older than 16 prior to 1997).[46] This precludes long-run dynamic effects from early-adopter states from contaminating estimated effects. Second, we cut off the sample at earlier and earlier years, targeting the 1997–2002 window when most states adopted GDL laws.

Table C.1 shows the results of these exercises. Column (1) replicates our preferred specification (column (2) of Table 2) to aid comparison. Columns (2)–(5) drop any states that are always-treated during our study window (about 20% of observations). While column (2) uses data over the full study window, columns (3)–(5) respectively omit the five, ten, and fifteen most recent years of data. Results for all model estimates are relatively constant across specifications, though they become less precise as more data is omitted. The placebo effect ($\beta_1$) remains close to zero, whereas the interaction effect ($\beta_3$) and total effect of GDL where dropouts are legal ($\beta_1 + \beta_3$) both vary within relatively narrow bands. There is a bit more variation in the CS effect ($\beta_2$), but these estimates all suggest that, if anything, our primary estimates are conservative relative to other sample windows. The results in Table C.1 suggest that our main findings are not being driven by long-run dynamics in the effects of GDL laws.

In a second test of the dynamism of GDL law treatment effects, we estimate a model that includes indicators for bins of years in post-treatment time: 0–4, 5–9, 10–14, and 15+ years after GDL adoption. As before, we also drop always-treated units to avoid

[46]Our data observation window begins in 1990, but no states adopted a new GDL law between 1990 and 1997.

Table C.1: The Effect of Min. Unrestricted Driving Age on Dropout for a Limited Panel

| | Not In School = 1 | | | | |
|---|---|---|---|---|---|
| | | Drop always-treated states & Limit sample to years: | | | |
| | Full Sample | 1990-2017 | 1990-2012 | 1990-2007 | 1990-2002 |
| | (1) | (2) | (3) | (4) | (5) |
| Min. Unres. Driving Age >16 ($\beta_1$) | 0.0014 | -0.0008 | 0.0008 | 0.0022 | 0.0018 |
| | (0.0039) | (0.0039) | (0.0029) | (0.0032) | (0.0059) |
| School-Leaving Age $\leq$ 16 ($\beta_2$) | 0.0182*** | 0.0202*** | 0.0229*** | 0.0274*** | 0.0257*** |
| | (0.0047) | (0.0051) | (0.0057) | (0.0063) | (0.0099) |
| Min. Unres. Driving Age >16 $\times$ School-Leaving Age $\leq$ 16 ($\beta_3$) | -0.0119** | -0.0135** | -0.0131*** | -0.0144*** | -0.0135* |
| | (0.0048) | (0.0052) | (0.0047) | (0.0051) | (0.0075) |
| **Effect of GDL if School-Leaving Age $\leq$ 16** ($\beta_1 + \beta_3$) | -0.0105** | -0.0143** | -0.0123** | -0.0121** | -0.0117* |
| | (0.0049) | (0.0061) | (0.0055) | (0.0055) | (0.0062) |
| Exclude Always Treated | - | Y | Y | Y | Y |
| Obs | 75,196 | 60,864 | 49,038 | 35,755 | 21,603 |

Marginal effects evaluated at sample means from probit regression using CPS ASEC data. All specifications include state and year fixed effects. Controls include: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; and state log real effective minimum wage. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

contamination from long-run effects. Table C.2 reports the results of this "grouped" triple-difference design. Estimates of $\beta_1$ are stable and close to zero, providing further placebo evidence that our research design and implementation identifies the effect of interest and is not overly subject to dynamic contamination. Moreover, the total effects of GDL laws in states without binding CS laws ($\beta_1 + \beta_3$) are fairly constant over time as well, further suggesting that our estimates are not biased by treatment effect dynamism.

## C.1 Imputation Design

In order to implement the solutions provided in the recent literature on robust difference-in-difference estimation, we recast our research design into a more compatible framework for those proposed estimators. This section details how we recast our design to fit the imputation estimator of Borusyak, Jaravel, and Spiess (2021).

We make three major changes from our preferred specification. First, we disallow an independent effect of GDL laws on school-going when teens are not permitted to drop

Table C.2: The Effect of Minimum Unrestricted Driving Age on 16-yo Dropout Over Time

| | Not In School = 1 | |
|---|---|---|
| | Main Specification (1) | Effect Over Time (2) |
| Min. Unres. Driving Age >16 ($\beta_1$) | -0.0008 | |
| | (0.0039) | |
| 0-4 Yrs Post | | 0.0007 |
| | | (0.0041) |
| 5-9 Yrs Post | | -0.0012 |
| | | (0.0048) |
| 10-14 Yrs Post | | -0.0019 |
| | | (0.0074) |
| 15+ Yrs Post | | 0.0041 |
| | | (0.0096) |
| School-Leaving Age $\leq$ 16 ($\beta_2$) | 0.0202*** | 0.0240*** |
| | (0.0051) | (0.0063) |
| Min. Unres. Driving Age >16 | -0.0135** | |
| $\times$ School-Leaving Age $\leq$ 16 ($\beta_3$) | (0.0052) | |
| 0-4 Yrs Post | | -0.0148** |
| | | (0.0059) |
| 5-9 Yrs Post | | -0.0065 |
| | | (0.0042) |
| 10-14 Yrs Post | | -0.0119* |
| | | (0.0055) |
| 15+ Yrs Post | | -0.0169** |
| | | (0.0072) |
| **Effect of GDL if** | -0.0143** | |
| **School-Leaving Age $\leq$ 16** ($\beta_1 + \beta_3$) | (0.0061) | |
| 0-4 Yrs Post | | -0.0142** |
| | | (0.0056) |
| 5-9 Yrs Post | | -0.0077 |
| | | (0.0056) |
| 10-14 Yrs Post | | -0.0139** |
| | | (0.0055) |
| 15+ Yrs Post | | -0.0128 |
| | | (0.0083) |
| Obs | 60,864 | 60,864 |

Marginal effects evaluated at sample means from probit regression using CPS ASEC data from 1990–2017. All specifications include: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; state log real effective minimum wage; state and year fixed effects. Observations within states for which the minimum unrestricted driving age is always greater than 16 during our sample are omitted. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

out (when school-leaving age is >16). That is, we recast our triple-difference design as a more standard difference-in-differences design where the treatment is the interaction of restricted driving laws and non-binding compulsory schooling laws. Given the small, insignificant, and relatively precise estimates of $\beta_1$ throughout our analyses, we view this as a reasonable restriction on the estimation model.

Second, we assume a linear probability model. This is potentially consequential because our binary outcome variable has a mean that is very close to zero (only 3.8% of 16-year-olds drop out in our sample), a setting in which a linear probability model will usually generate biased and inconsistent estimates. However, comparing the linear probability model estimates in Table B.1 with the probit results in Table 2 suggests that this is reasonable.

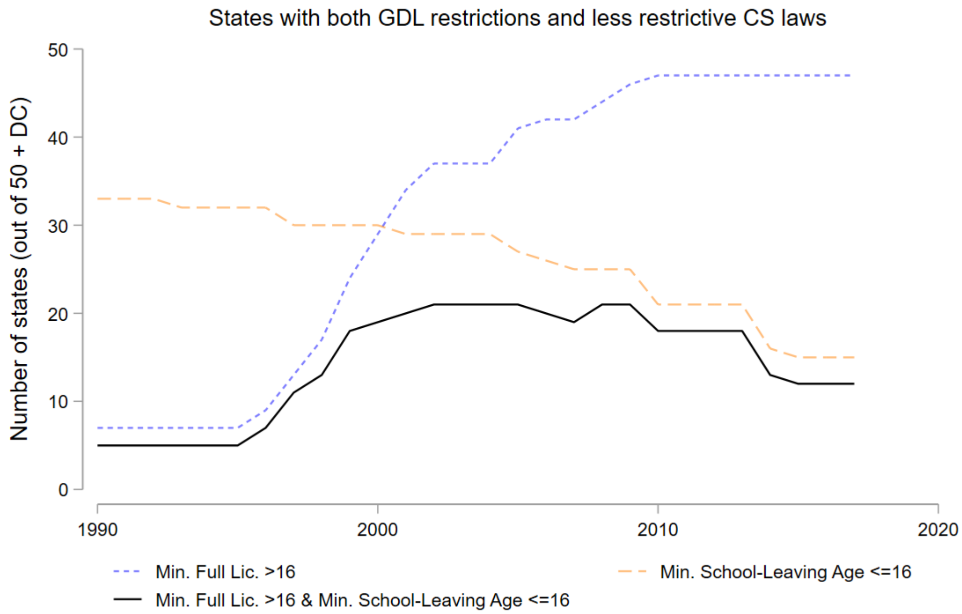
Finally, the newly developed estimators that account for treatment effect dynamics in a difference-in-differences model do not permit treatment to "turn on" and then "turn off" again. Therefore, we must omit some data from our sample to account for the fact that our interacted treatment ($GDL_{st} * CS_{st}$) both turns on and turns off over time. Specifically, in states for which the interacted treatment ever equals one (turns on), we drop all years of data after treatment then turns off. Figure 1a reveals that states are gradually adopting GDL laws, and Figure 1b shows that they are also gradually restricting the ability of 16-year-olds to drop out. This implies that the interaction of restricted GDL laws and unrestricted dropout legality typically comes into effect (turns on) for a period of time before being blocked (turns off) by more restrictive compulsory schooling laws. To illustrate, the solid black line in Figure C.1 plots the number of states for which the interacted treatment is equal to one over time. Many states adopt GDL laws without restricting dropping out between 1995 and 2001, but the number of states with this interacted treatment begins to decline slowly through 2010 and more abruptly in 2013 and 2014.

We consider a model similar to Equation 1 that excludes the non-interacted $GDL_{st}$ term:

$$\begin{aligned} NotInSchool_{ist} = \beta_2 CS_{st} + \sum_k \beta_{sk} 1[t - E_s = k] \\ + X_i'\nu + Z_{st}'\mu + D_s + D_t + \epsilon_{ist}, \end{aligned} \tag{C.1}$$

wherein $E_s$ is the first year that $GDL_{st} * CS_{st} = 1$ in state $s$ and the $\beta_{sk}$ are potentially heterogeneous and dynamic treatment effects that, when aggregated, correspond to $\beta_3$ in

Figure C.1: Prevalence of the "Interacted" Treatment over Time




Equation 1.[47] If, as previously estimated, the true value of $\beta_1$ is zero, then estimates from Equation 1 and Equation C.1 should be very similar.

We apply the imputation estimator of Borusyak, Jaravel, and Spiess (2021), which is the most efficient linear unbiased estimator of any pre-specified weighted sum of treatment effects under the assumptions of parallel trends and homoskedasticity, and has attractive efficiency properties under heteroskedasticity.[48] This estimator recovers a well-defined ATT even under arbitrary treatment-effect heterogeneity and dynamism.

Table 3 in Section 4.1 shows the results using the imputation estimator. The model in column (1) omits all controls except $CS_{st}$. Column (2) includes all control variables ($X_i$ and $Z_{st}$). Column (3) omits never-treated units (all three columns omit always-treated units) to test whether our results hinge on comparisons to states that are subject to different trends than those that eventually adopt GDL laws. These estimates are nearly identical to the main results in Table 2. The standard errors, which are conservative under

[47] They also correspond to $\beta_1 + \beta_3$ when aggregated because $\beta_1$ here is assumed to be zero.

[48] In our setting, this imputation estimator is more computationally robust than estimators that individually estimate and aggregate all possible 2x2 difference-in-differences designs (such as Callaway and Sant'Anna (2021)). Our data includes many individual 2x2 designs that are based on a small number of observations, and individual estimates from these designs are extremely noisy. The imputation approach uses more information to estimate $st$-specific treatments (under a maintained assumption of parallel trends), and so is more efficient.

treatment effect heterogeneity but exact if treatment effects are homogenous, are actually slightly smaller.[49] These results imply that our main results are robust to arbitrary treatment effect heterogeneity and dynamics.

# D District-Level Dropout Analyses

To support the findings on teen education outcomes shown in Section 4, we collect school-district level data on high school dropouts from the National Center for Education Statistics' (NCES) Common Core of Data (CCD). This data covers school-years from 1994 to 2009 and includes the combined dropout rate for grades 9-12 as well as several time-varying measures of district-level student demographics and other characteristics. For a smaller set of years (1994-2001) dropout rates are also reported separately for each grade 9 through 12. Due to reporting inconsistencies, the data comprises an unbalanced panel of 12,325 school-districts over 16 school-years.

Because this data is aggregated up to the district-by-grade level, we are not able to implement our preferred triple-difference identification strategy. Each grade will include individuals who are of varying ages, some of whom might be restricted by the state's compulsory schooling laws while others within the same grade are not. Thus, we analyze the effect of teen driving restrictions on high school dropout rates using a difference-in-differences strategy, which we estimate with two-way fixed effects:

$$DropoutRate_{dst} = \beta GDL_{st} + X'_{dt}\nu + Z'_{st}\mu + D_d + D_t + \epsilon_{dst}, \tag{D.1}$$

where $DropoutRate_{dst} \in [0, 1]$ is the high school dropout rate for school district $d$ in state $s$ in year $t$. Table D.1 shows that the overall average high school dropout rate in our sample is 3.5%, ranging from an average of 2.6% for 9th graders to 4.3% for those in the 12th grade.

The primary variable of interest is $GDL_{st}$, which measures the minimum age at which teens can obtain a full (unrestricted) driver's license. The vector $X_{dt}$ includes time-varying school-district level controls: percent of students eligible for free lunch; percent of students white; number of full-time equivalent teachers; log of total expenditures per student; and urbanization indicators. The variable $Z_{st}$ includes the state's minimum school-leaving age, log minimum wage, and 3-month average unemployment rate. The model

[49] See Borusyak, Jaravel, and Spiess (2021) for discussion of inference.

Table D.1: Summary Statistics on School Districts

| | Mean | Std. Dev | Min | Max |
|---|---|---|---|---|
| High School Dropout Rates: | | | | |
| Grades 9-12 | 0.034 | 0.05 | 0 | 0.99 |
| Grade 9* | 0.026 | 0.05 | 0 | 1 |
| Grade 10* | 0.035 | 0.05 | 0 | 1 |
| Grade 11* | 0.041 | 0.05 | 0 | 1 |
| Grade 12* | 0.043 | 0.06 | 0 | 1 |
| % of Students Free-Lunch Eligible | 30.4 | 19.4 | 0 | 99.7 |
| % of Students White | 77.7 | 26.2 | 0 | 100 |
| # of Full-time Equivalent Teachers | 257 | 843 | 0 | 65,804 |
| Expenditure per Pupil (in $1,000s) | 10.1 | 5.71 | 0 | 283 |
| Urbanization Category: | | | | |
| Large City | 0.02 | 0.15 | 0 | 1 |
| Mid-size or Small City | 0.05 | 0.22 | 0 | 1 |
| Suburb of Large City | 0.16 | 0.37 | 0 | 1 |
| Suburb of Mid-size or Small City | 0.08 | 0.27 | 0 | 1 |
| Large Town | 0.02 | 0.15 | 0 | 1 |
| Small Town | 0.17 | 0.37 | 0 | 1 |
| Rural - outside CBSA/MSA | 0.39 | 0.49 | 0 | 1 |
| Rural - inside CBSA/MSA | 0.11 | 0.31 | 0 | 1 |
| Minimum Unrestricted Driving Age | 16.7 | 0.71 | 15 | 18 |
| Minimum School-Leaving Age | 16.8 | 0.91 | 16 | 18 |

Source: NCES Common Core Data linked to GDL and CS data; see text for more details. This data comprises an unbalanced panel of 12,149 school districts over the 16 years spanning 1994-2009 with a total 114,414 district-year observations. *Dropout rates for each grade are available for only a subset of years (1994-2001) and are based on a smaller sample of 45,407 district-year observations.

also includes both district and year fixed effects. District fixed effects control for time-invariant characteristics of a school, such as location and district membership. Because schools typically stay relatively fixed in the income distribution of attendee families in the short and medium term, these also control to some degree for socioeconomic differences in student populations. We estimate Equation D.1 as a linear model and estimate standard errors clustered at the state level.

Column (1) of Table D.2 shows that a one year increase in the minimum unrestricted driving age leads to a 0.42pp reduction in high school dropout rates. This is equivalent to an 12% reduction in the dropout rate when evaluated at the mean. In Column (2), we replace the continuous measure of unrestricted driving age with an indicator variable equal to one if the minimum unrestricted driving age is greater than 16. Increasing the unre-

Table D.2: The Effect of Minimum Unrestricted Driving Age on High School Dropout Rates

| | Dropout Rate Grades 9-12 | Dropout Rate Grades 9-12 | Dropout Rate Grades 9-12 | Dropout Rate Grade 9 | Dropout Rate Grade 10 | Dropout Rate Grade 11 | Dropout Rate Grade 12 |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Min. Unres. Driving Age | -0.0042*** | | | | | | |
| | (0.0011) | | | | | | |
| Min. Unres. Driving Age >16 | | -0.0032* | -0.0046** | -0.0036 | -0.0050** | -0.0058** | -0.0047* |
| | | (0.0017) | (0.0021) | (0.0024) | (0.0021) | (0.0025) | (0.0026) |
| Years in Sample | 1994-2009 | 1994-2009 | 1994-2001 | 1994-2001 | 1994-2001 | 1994-2001 | 1994-2001 |
| Mean Dropout Rate | 0.034 | 0.034 | 0.036 | 0.026 | 0.035 | 0.041 | 0.042 |
| Obs | 114,043 | 114,043 | 44,735 | 44,166 | 44,246 | 44,366 | 44,623 |

All specifications include: % of public school students in the district eligible for free lunch; % of public school students who are white; # of full-time equivalent teachers; log of total expenditures per student; indicators for the district's urbanization level; the state minimum legal dropout age; state unemployment rate; state minimum wage; and district and year fixed-effects. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

stricted driving age, and thus restricting teen mobility, is then associated with a 0.32pp reduction in the high school dropout rate (a 9% reduction from the mean).

In columns (4)-(7), we estimate the effect of teen driving restrictions on dropout rates for each grade of high school separately. Because of reporting limitations, this restricts our sample to years before 2002, limiting identifying variation to those states that were relatively early adopters of GDL laws. Column (3) replicates the specification of Column (2), but includes only years up to 2001 in the sample. The effect of raising the minimum driving age to greater than 16 on overall high school dropouts is somewhat larger in magnitude in this sub-sample, reducing dropouts by 0.46pp. Columns (4)-(7) show that the effects of increasing the minimum driving age to over 16 are largest for 10th- and 11th-grade dropout rates (a 14% reduction from the mean in both grades). It is during these grades that many teenagers obtained full privilege licenses prior to GDL laws (as teens generally turn 16 during the 10th or 11th grade). These results indicate that imposing restrictions on teen mobility leads to a sizable reduction in high school dropout rates of 9-14%.

# E Details of the Model-Based Analysis

In this Appendix, we detail additional comments about the model and its estimation that are too lengthy to be included in the main text of Section 6.

## E.1 Identification

Model identification takes advantage of Assumption 1 (idiosyncratic preferences for work and school are distributed bivariate normal). However, three facets of identification warrant additional discussion. First, both $\Gamma$ and $\rho$ reflect how likely both activities are to be chosen and are not separately identified without further assumptions. However, activity-specific utility shifters provide variation that disentangles these two parameters.[50] A shift in the utility of one activity only increases (decreases) the likelihood of choosing the other activity if both activities are complements (substitutes). Thus comparing outcomes across different values of the shifter identifies $\Gamma$, while $\rho$ then reflects how correlated idiosyncratic tastes are for the two activities.

Second, while the parameters of multinomial probit models are theoretically identified from choice data, Keane (1992) shows that this identification is weak even in datasets with reasonable numbers of observations. Stronger identification can be obtained with activity-specific characteristics, and many applications of multinomial probit exploit such exclusion restrictions (e.g., Goolsbee and Petrin 2004). Assumption 2 assigns restrictions on the parameters that serve as coefficients for $z_{st}$. These become activity-specific utility shifters, in that two components of $z_{st}$ can only shift the utility of work and the other two components of $z_{st}$ can only shift the utility of school. As with instrumental variables, the exclusion restriction alone may not aid estimation. It is also useful for at least one of the activity-specific utility shifters to have a non-zero effect on the activities they shift. This relevance condition is likely satisfied, given the $\pi$ coefficients in Table 7.

The third identification challenge is to ensure that $\gamma^{k+}$ reflect the effects of GDL policies and not other factors that may be correlated with GDL policies. In linear settings, state and year fixed effects would control for many of these potentially confounding factors. In non-linear settings, the inclusion of fixed effects can create estimation challenges (if there are large number of effects) and induce bias in parameter estimates (the "incidental parameters problem"). Correlated random effects models share many of the benefits of fixed effects models but are more amenable to non-linear settings. In fact, estimators using fixed effects and correlated random effects are numerically equivalent in linear models (Mundlak 1978). We therefore assume correlated random effects (Assumption 3) and include in $f^k(s, \xi)$ a vector with the average value of each $x$ and $z$ for each state. The model also includes a vector of time dummies (omitting the first sample year to avoid

[50] Gentzkow (2007) mentions this possibility although instead uses repeated observations per individual to separate $\Gamma$ and $\rho$.

collinearity).[51]

## E.2 GHK Simulator

Our model is similar—but not identical—to a four-choice multinomial probit model. The fundamental difference is that the idiosyncratic component of the $AB$ choice (choosing both work and school) is simply a sum of $e^A$ and $e^B$. While this is a seemingly minor change, it has one important consequence. As presented in Assumption 1, $\Omega$ is positive definite matrix, therefore allowing for a Cholesky factorization of $\Omega$ (a Cholesky factor is a lower triangular matrix $L$ such that $LL' = \Omega$). However, if we were to represent the (normalized) covariance matrix of idiosyncratic preferences in the usual way for a multinomial probit, we would have:

$$\Omega^{\text{Extended}} = \begin{pmatrix} 1 & \rho\sigma & 1+\rho\sigma \\ \cdot & \sigma^2 & \sigma^2+\rho\sigma \\ \cdot & \cdot & \sigma^2+1 \end{pmatrix}.$$

Unfortunately, $\Omega^{\text{Extended}}$ is not generally positive definite and so Cholesky factorization of $\Omega^{\text{Extended}}$ may not be possible.

The positive definiteness of the covariance matrix of idiosyncratic preferences has important implications for estimation. Lemma 1 shows that even though the implicit covariance matrix is $\Omega^{\text{Extended}}$, we can instead rely just on $\Omega$ and thus the model can be estimated using a GHK (Geweke, Hajivassiliou, and Keane) simulator.[52] This simulator is advantageous because it is both fast and reasonably easy to implement, and results in much smoother likelihood functions than accept-reject simulators. These properties are computationally useful and also help ensure convergence.

**Lemma 1.** *Under Assumption 1, the model (Equations 5–8) can be estimated with a GHK simulator.*

*Proof.* To show that the model can be estimated with a GHK simulator is to show that the model's choice probabilities can be expressed in the following form

$$\Pr(\eta_k < \kappa_k) \times \Pr(\eta_{k'} < \kappa_{k'}(\eta_k) \mid \eta_k = x) \text{ for } k' \neq k,$$

[51]Our correlated random effects model has 104 parameters to estimate instead of the 182 required in a fixed effects specification, saving computational time, improving the likelihood of convergence, and reducing concerns about incidental parameters.

[52]For a detailed description of the GHK simulator, see Train (2009).

where $\eta_k$ and $\eta_{k'}$ are random variables distributed i.i.d. standard normal and $\kappa$ are constants that potentially depend upon realizations of $\eta$. The key feature is that each choice probability can be written as multiplicatively separable probabilities in which the first probability evaluates the unconditional probability of a single, i.i.d. random variable. The proof will thus proceed in two steps, first showing that the model can be expressed in terms of i.i.d. standard normal random variables, and second, showing that the choice probabilities then take the above form.

*Step 1:* The Cholesky factorization of $\Omega$ is a matrix $L$ such that $LL' = \Omega$. This gives

$$L = \begin{pmatrix} 1 & 0 \\ c & d \end{pmatrix},$$

where $c = \rho\sigma$ and $d = \sqrt{\sigma^2(1-\rho^2)}$. Thus, $(e^A, e^B) \overset{d}{=} (\eta_1, c\eta_1 + d\eta_2)$, where $\eta_1$ and $\eta_2$ represent i.i.d. standard normal variables. Rewrite the model in light of this equivalence in distribution (suppressing notation denoting individual $i$):

$$\begin{aligned} V(0,0) &= 0 \\ V(1,0) &= V_1 + \eta_1 \\ V(0,1) &= V_2 + c\eta_1 + d\eta_2 \\ V(1,1) &= V_1 + V_2 + \Gamma_{12} + (1+c)\eta_1 + d\eta_2. \end{aligned} \qquad \text{(i.i.d. normal model)}$$

Straightforward substitution of data and coefficients for $V_1$, $V_2$, and $\Gamma$ show equivalence to the primary model (Equations 5–8). Specifically, if $V_1 = \alpha^A + \gamma^A GDL^A_{st} + x'_{ist}\lambda^A + z'_{st}\pi^A + f^A(s,\xi) + \delta^A_t$, $V_2 = \alpha^B + \gamma^B GDL^B_{st} + x'_{ist}\lambda^B + z'_{st}\pi^B + f^B(s,\xi) + \delta^B_t$, and $\Gamma_{12} = \Gamma + \gamma^\Gamma GDL^\Gamma_{st}$, then the models are equivalent.

*Step 2:* We now show that the choice probabilities from this i.i.d. normal model can be derived in order to take advantage of the i.i.d. nature of the $\eta_1$ and $\eta_2$ variables. We show this sequentially for each choice in the choice set. First, the probability of choosing neither activity is:

$$\begin{aligned}
\Pr(\emptyset) &= \Pr\left(V_1 + \eta_1 < 0 \cap V_2 + c\eta_1 + d\eta_2 < 0 \cap V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 < 0\right) \\
&= \Pr(\eta_1 < -V_1) \cdot \Pr\left(V_2 + c\eta_1 + d\eta_2 < 0 \cap V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 < 0 \mid \eta_1 < -V_1\right) \\
&= \Pr(\eta_1 < -V_1) \cdot \Pr\left(\eta_2 < \frac{\min\left\{0, -(V_1 + \Gamma + \eta_1)\right\} - V_2 - c\eta_1}{d} \mid \eta_1 < -V_1\right) \\
&= \Phi(-V_1) \int_{-\infty}^{-V_1} \Phi\left(\frac{\min\left\{0, -(V_1 + \Gamma + \eta_1)\right\} - V_2 - c\eta_1}{d}\right) \phi(\eta_1) d\eta_1,
\end{aligned}$$

where $\phi$ and $\Phi$ represent the standard normal p.d.f. and c.d.f., respectively, and $\Gamma = \Gamma_{12}$ for ease of exposition. Next, the probability of choosing work only is:

$$\begin{aligned}
\Pr(A) &= \Pr\left(0 < V_1 + \eta_1 \cap V_2 + c\eta_1 + d\eta_2 < V_1 + \eta_1 \cap V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 < V_1 + \eta_1\right) \\
&= \Pr(\eta_1 > -V_1) \\
&\quad \cdot \Pr\left(V_2 + c\eta_1 + d\eta_2 < V_1 + \eta_1 \cap V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 < V_1 + \eta_1 \mid \eta_1 > -V_1\right) \\
&= \Pr(\eta_1 > -V_1) \cdot \Pr\left(\eta_2 < \frac{\min\left\{V_1 + \eta_1, -\Gamma\right\} - V_2 - c\eta_1}{d} \mid \eta_1 > -V_1\right) \\
&= (1 - \Phi(-V_1)) \int_{-V_1}^{\infty} \Phi\left(\frac{\min\left\{V_1 + \eta_1, -\Gamma\right\} - V_2 - c\eta_1}{d}\right) \phi(\eta_1) d\eta_1.
\end{aligned}$$

Next, the probability of choosing the school activity only is:

$$\begin{aligned}
\Pr(B) &= \Pr\left(0 < V_2 + c\eta_1 + d\eta_2 \cap V_1 + \eta_1 < V_2 + c\eta_1 + d\eta_2\right. \\
&\quad \left. \cap V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 < V_2 + c\eta_1 + d\eta_2\right) \\
&= \Pr\left(0 < V_2 + c\eta_1 + d\eta_2 \cap V_1 + \eta_1 < V_2 + c\eta_1 + d\eta_2 \cap V_1 + \Gamma + \eta_1 < 0\right) \\
&= \Pr(\eta_1 < -V_1 - \Gamma) \cdot \Pr\left(0 < V_2 + c\eta_1 + d\eta_2 \cap V_1 + \eta_1 < V_2 + c\eta_1 + d\eta_2 \mid \eta_1 < -V_1 - \Gamma\right) \\
&= \Pr(\eta_1 < -V_1 - \Gamma) \cdot \Pr\left(\eta_2 > \frac{\max\{0, V_1 + \eta_1\} - V_2 - c\eta_1}{d} \mid \eta_1 < -V_1 - \Gamma\right) \\
&= \Phi(-V_1 - \Gamma) \int_{-\infty}^{-V_1 - \Gamma} \left(1 - \Phi\left(\frac{\max\{0, V_1 + \eta_1\} - V_2 - c\eta_1}{d}\right)\right) \phi(\eta_1) d\eta_1.
\end{aligned}$$

And, finally, the choice probability for both activities is:

$$
\begin{aligned}
\Pr(AB) &= \Pr\left(0 < V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 \cap \right.\\
&\qquad V_1 + \eta_1 < V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 \cap \\
&\qquad \left. V_2 + c\eta_1 + d\eta_2 < V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2\right) \\
&= \Pr\left(0 < V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 \cap \right.\\
&\qquad V_1 + \eta_1 < V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 \cap \\
&\qquad \left. 0 < V_1 + \Gamma + \eta_1\right) \\
&= \Pr(\eta_1 > -V_1 - \Gamma) \cdot \\
&\qquad \Pr\left(0 < V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 \cap \right.\\
&\qquad\qquad \left. V_1 + \eta_1 < V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 \mid \eta_1 > -V_1 - \Gamma\right) \\
&= \Pr(\eta_1 > -V_1 - \Gamma) \cdot \Pr\left(\eta_2 > \frac{\max\{-(V_1 + \eta_1), 0\} - V_2 - \Gamma - c\eta_1}{d} \mid \eta_1 > -V_1 - \Gamma\right) \\
&= (1 - \Phi(-V_1 - \Gamma)) \int_{-V_1 - \Gamma}^{\infty} \left(1 - \Phi\left(\frac{\max\{-(V_1 + \eta_1), 0\} - V_2 - \Gamma - c\eta_1}{d}\right)\right) \phi(\eta_1) d\eta_1.
\end{aligned}
$$

Thus, the choice probabilities can be written as multiplicatively separable probabilities in which the first probability evaluates the unconditional probability of a single, i.i.d. random variable. $\square$

Although the algebra is somewhat cumbersome, there is no significant additional computational cost beyond what is used when estimating a typical (normalized) trinomial probit model. The primary differences with a trinomial probit are that (i) there is one additional choice probability and that (ii) the conditional probabilities contain non-linear functions of the conditioning random variable.

The advantage of the GHK simulator over simply estimating directly from draws of $(e^A, e^B)$ is that the GHK simulator preserves continuity in one of the dimensions of the random variable. That is to say, the unconditional probability in the above choice probabilities need not be simulated, and so can be smoothly evaluated via standard numerical means. Simulation needs to be undertaken only for the conditional probabilities. This smoothing greatly enhances the performance of optimization routines at finding maxima.

## E.3 Estimation Details

To simulate $\eta_2$, we use Halton draws. We then estimate the model by maximum simulated likelihood. Specifically, we code the model in Julia using the L-BFGS optimization routine in the Optim package (Mogensen and Riseth 2018). Because $\rho$ and $\sigma$ cannot take on all real values, we transform them as:

$$\tilde{\rho} = \frac{1}{2} \ln \left( \frac{1+\rho}{1-\rho} \right) \quad \text{and} \quad \tilde{\sigma} = \ln(\sigma).$$

We use $\tilde{\rho}$ and $\tilde{\sigma}$, along with all the other coefficients (as is) for unconstrained optimization.

Our optimization procedure consists of several steps:

1. Maximize the simulated likelihood of a variant of the model where $\delta_t^A$ and $\delta_t^B$ are replaced with linear time trends with 100 draws of $\eta_2$ for each observation, using starting values of $\tilde{\rho} = -0.2$ and $\tilde{\sigma} = -0.5$ and all other coefficient at zero (experimentation showed that these starting values improved convergence).

2. Maximize the simulated likelihood of a variant of the model where $\delta_t^A$ and $\delta_t^B$ are replaced with linear time trends with 250 draws of $\eta_2$ for each observation, using the optimum from Step 1 for starting values.

3. Maximize the simulated likelihood of the full model with 100 draws of $\eta_2$ for each observation, using the optimum from Step 2 for starting values and extrapolating the linear time trends to create starting values for individual year dummies.

4. Maximize the simulated likelihood of the full model with 250 draws of $\eta_2$ for each observation, using the optimum of Step 3 for starting values.

## E.4 Model Fit

Table E.1 assesses how well our estimated model explains the data by showing how often a simulated choice matches the observed choice (averaged over 100 draws of $(e_i^A, e_i^B)$ for each individual). The model slightly overestimates the probabilities of choosing neither work nor school (0,0) and school only (0,1), while it slightly underestimates the probabilities for work only (1,0) and the both work and school choice (1,1). Overall, summing the diagonal components of Table E.1, the model correctly classifies those in the sample 62.3% of the time. Given the large number of individual characteristics that we do not observe, we believe this to be a reasonable approximation.

Table E.1: Model Fit

| | | True $\mathcal{P}^{(0,0)}$ | True $\mathcal{P}^{(1,0)}$ | True $\mathcal{P}^{(0,1)}$ | True $\mathcal{P}^{(1,1)}$ |
|---|---|---|---|---|---|
| | *Totals* | 2.454% | 1.329% | 74.271% | 21.946% |
| Model $\mathcal{P}^{(0,0)}$ | 2.470% | 0.084% | 0.040% | 1.911% | 0.435% |
| Model $\mathcal{P}^{(1,0)}$ | 1.309% | 0.037% | 0.020% | 0.935% | 0.317% |
| Model $\mathcal{P}^{(0,1)}$ | 74.282% | 1.877% | 0.972% | 56.188% | 15.245% |
| Model $\mathcal{P}^{(1,1)}$ | 21.939% | 0.455% | 0.296% | 15.238% | 5.949% |

Shows the shares of each observed and simulated outcome of the model using parameters shown in Table 7 averaged over 100 draws of errors from a bivariate normal with a standard generator. The top row shows the observed share of the population choosing each outcome, whereas the right column shows the average simulated shares that choose each outcome. The other cells show the average shares of the population for each observed and simulated outcome combination. Observations are weighted using sample weights.

## E.5 Counterfactuals: Decompositions and Invariance

To decompose total treatment effects into their direct and indirect components, first let $\mathcal{P}^c$ be functions of the data and estimated parameters that explicitly take the four vectors of GDL variables and the auxiliary parameter as arguments:

$$\mathcal{P}^c(GDL^0_{st}, GDL^A_{st}, GDL^B_{st}, GDL^\Gamma_{st}, \tilde{\gamma}^0) = \\ n^{-1} \sum_i \mathbb{E}_e 1[V_i(c) \geq V_i(c') | GDL^0_{st}, GDL^A_{st}, GDL^B_{st}, GDL^\Gamma_{st}, \tilde{\gamma}^0],$$

where $n$ is the total number of observations. The right hand side captures the average probability of an activity choice, given the GDL variables and $\tilde{\gamma}^0$. In a slight abuse of notation, let 0 or 1 be admissible arguments to the GDL arguments of $\mathcal{P}^k$ that reflect setting all values to 0 or 1, e.g., $\mathcal{P}^{(0,1)}(0, 0, 0, 0, \tilde{\gamma}^0)$ The total shares of the population that choose each activity are:

$$\mathcal{Q}^A(\cdot) = \mathcal{P}^{(1,0)}(\cdot) + \mathcal{P}^{(1,1)}(\cdot), \quad \mathcal{Q}^B(\cdot) = \mathcal{P}^{(0,1)}(\cdot) + \mathcal{P}^{(1,1)}(\cdot), \quad \text{and } \mathcal{Q}^\varnothing(\cdot) = \mathcal{P}^{(0,0)}(\cdot)$$

for work, school, and neither work nor school, respectively.

The **total effect** of GDL laws captures the overall effect on each activity of increasing the minimum unrestricted driving age from 16 or less to greater than 16. In the model, this is captured by the differences in choices when $GDL^k_{st} = 1$ compared to when $GDL^k_{st} = 0$,

$\forall k, s, t$:

$$\theta^k_{\text{Tot}}(\tilde{\gamma}^0) = \mathcal{Q}^k(1,1,1,1,\tilde{\gamma}^0) - \mathcal{Q}^k(0,0,0,0,\tilde{\gamma}^0),\ \forall k \in \{\varnothing, A, B\}.$$

The total effect is invariant to the value $\tilde{\gamma}^0$, so $\theta^k_{\text{Tot}} = \theta^k_{\text{Tot}}(\tilde{\gamma}^0), \forall \tilde{\gamma}^0$, though this will not be generally true for the decompositions. We simulate these model-based treatment effects (and their decompositions) to reflect the triple-difference design described in Section 3. That is, for these simulations we set $CS = 1$ and thus $CS \times GDL = GDL$.

We next use the model to decompose each of the three total effects into their direct and indirect channels. The **direct effects** reflect how each GDL component affects its *own activity*, e.g., the effect of $GDL^A$ on working and of $GDL^B$ on school. As such, it is governed by $\tilde{\gamma}^A$ for work, $\tilde{\gamma}^B$ for school, and $\tilde{\gamma}^0$ for neither. Because GDL laws restrict mobility, we expect that they will weakly reduce the value of each activity and that direct effects will therefore be weakly negative. The **indirect effects** capture the consequences of the GDL components on the *other activities*, i.e., of $GDL^0$, $GDL^B$, and $GDL^\Gamma$ on working, or $GDL^0$, $GDL^A$ and $GDL^\Gamma$ on school-going.

We define these effects in a consistent manner that additively decomposes the total effects into the two types of channels.[53] Specifically:

*Neither activity effects*

$\theta^\varnothing_{\text{Dir}} = \mathcal{Q}^\varnothing(1,0,0,0,\tilde{\gamma}^0) - \mathcal{Q}^\varnothing(0,0,0,0,\tilde{\gamma}^0)$ Direct effect on "neither" activity

$\theta^\varnothing_{\text{Ind}} = \mathcal{Q}^\varnothing(1,1,1,1,\tilde{\gamma}^0) - \mathcal{Q}^\varnothing(1,0,0,0,\tilde{\gamma}^0)$ Indirect effect on "neither" activity

*Employment effects*

$\theta^A_{\text{Dir}} = \mathcal{Q}^A(0,1,0,0,\tilde{\gamma}^0) - \mathcal{Q}^A(0,0,0,0,\tilde{\gamma}^0)$ Direct effect on employment

$\theta^A_{\text{Ind}} = \mathcal{Q}^A(1,1,1,1,\tilde{\gamma}^0) - \mathcal{Q}^A(0,1,0,0,\tilde{\gamma}^0)$ Indirect effect on employment

*Schooling effects*

$\theta^B_{\text{Dir}} = \mathcal{Q}^B(0,0,1,0,\tilde{\gamma}^0) - \mathcal{Q}^B(0,0,0,0,\tilde{\gamma}^0)$ Direct effect on school-going

$\theta^B_{\text{Ind}} = \mathcal{Q}^B(1,1,1,1,\tilde{\gamma}^0) - \mathcal{Q}^B(0,0,1,0,\tilde{\gamma}^0)$ Indirect effect on school-going

Table 8 includes in italics additional terms that focus on specific indirect channels to aid interpretation. For example, the indirect effect of GDL laws on schooling decisions consists of a component stemming from reduced access to employment and a component stemming from reduced access to leisure (represented by the neither option).

[53] There are several reasonable ways to define these effects to reflect slightly varied counterfactuals. This definition has the advantage of additivity.