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

Valerie Bostwick

Kansas State University

IZA Institute of Labor Economics

Christopher Severen

Federal Reserve Bank of Philadelphia

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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 increases short-run schoolgoing and long-run educational attainment while reducing teen labor force participation. We develop a multiple discrete choice model that rationalizes these unintended consequences and reveals that the 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

1   Introduction

Teenagers make decisions regarding human capital accumulation that can permanently alter their lifetime economic trajectories. Of particular import is whether or not to complete high school. High school education increases adult earnings and lifetime wealth (Angrist and Krueger 1991; Oreopoulos 2007) and reduces rates of teen pregnancy and the incidence of adolescent crime (Black, Devereux, and Salvanes 2008; Anderson 2014; Bell, Costa, and Machin 2022). Policymakers, educators, and parents invest substantial time, money, and effort to shape such human capital decisions. However, understanding the effects of policies on teen schoolgoing is complicated if policies also shift the attractiveness of alternative activities, such as work and leisure.

In this paper, we show that a policy targeted at improving teen car safety had unintended and long-lasting effects on human capital accumulation. Specifically, we investigate graduated driver 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 the number of passengers who may ride with a teen driver. These mobility restrictions may directly impact teens' ability to commute to school, to work, and to leisure activities. But there may also be indirect channels linking these activities, e.g., reduced access to employment opportunities could discourage teens from dropping out of high school.

We combine quasi-experimental variation in the timing of GDL law adoption with cross-state variation in compulsory schooling laws to identify the effects of teen driving restrictions on high school retention. Our research design compares the effect of GDL laws on the dropout behavior of 16-year-olds in states where compulsory schooling laws allow dropout at age 16 to the effect of GDL laws in states where 16-year-olds are prohibited from dropping out.[1] Using microdata from the Current Population Survey's Annual Social and Economic Supplement and a difference-in-differences-in-differences model, we estimate that GDL laws decrease the probability of high school dropout at age 16 by 1.1–1.3 percentage points (pp, a 29–35% reduction at the mean). We further show that, by age 17, these teens are 1.6pp more likely be in school, suggesting that GDL laws encourage teens not only to postpone dropping out but to eventually complete high school. To confirm this finding, we use American Community Survey data to estimate long-run per-

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[1]This is the age that is most impacted by GDL laws. From 1990 to 2017, 40 states switched from allowing 16-year-olds to obtain full driver's licenses to restricting this privilege to older teens.

sistence of GDL effects on human capital accumulation. We find that adults aged 22–34 who experienced a GDL restriction as a teen are 0.6pp more likely to eventually obtain a traditional high school diploma.

We show that the negative effect of GDL laws on high school dropout behavior is robust to a number of alternative estimation choices. We estimate the triple-difference model using three different estimators: a two-way fixed effects OLS estimator, the imputation estimator of Borusyak, Jaravel, and Spiess (2024) (which is robust to bias from negative weights and dynamic treatment effects), and a fixed effects probit maximum likelihood estimator. We also estimate a dynamic triple-difference event study model, as well as separate dynamic difference-in-differences event studies for the group of states where compulsory schooling laws allow dropout and the group of states that do not. All three of these event studies show parallel pre-treatment trends, providing support for the identifying assumption of parallel counterfactual trends. Finally, we show that our results can be replicated using an alternative dataset and alternative identification strategy. Using school-district-level data from the National Center for Educational Statistics' Common Core of Data, we estimate an analogous decline in dropout rates following the adoption of GDL laws: a 15% reduction in 11th grade dropout (the grade in which teens are most likely to be of an age to face binding GDL restrictions). This broad evidence base supports a causal interpretation of the primary results that GDL restrictions create a meaningful and long-lasting reduction in high school dropout behavior.

We next turn to the complicated task of disentangling potential mechanisms for this main effect. Because GDL laws reduce teen mobility and therefore may decrease access to school, ceteris paribus, it may be surprising that GDL laws actually improve educational attainment. However, these driving restrictions also limit access to teen employment and leisure activities. The indirect effects on high school completion stemming from changes in access to alternative activities could dominate any direct effect, depending on the strength of those indirect channels and substitution patterns between activities. The positive estimates on high school retention and completion suggest that these indirect margins are important to the underlying teen decision-making process. We use variation in the intensity of mobility restrictions imposed by each state's GDL laws to provide further insight into these direct and indirect channels. The strictest variant of GDL law, which completely disallows unsupervised 16-year-old driving, does *not* lead to a reduction in high school dropout. This strongly suggests the presence of a countervailing direct impact of GDL laws limiting access to school when teen driving is fully prohibited.

The policy implication is that limiting teen driving can improve educational outcomes by reducing access to outside options (such as leisure or employment), but these positive effects diminish as mobility restrictions increase.

To investigate whether the indirect effects of GDL laws may be attributable to reduced access to work opportunities, we analyze changes in teen employment outcomes. The resulting estimates show that GDL laws reduce 16-year-old labor force participation by 2.3pp (a 9% reduction at the mean) only in state-years where those teens are unrestricted by compulsory schooling laws. This result strongly suggests an indirect channel linking teens' decisions regarding school and work. However, these estimates do not allow us to distinguish how much of the reductions in labor supply or dropout rates reflect the indirect channel stemming from reduced access to leisure activities.

To distinguish these channels, we develop a multiple discrete choice model. In addition to confirming the effects estimated by the triple-difference designs, the model rationalizes these findings by decomposing total effects into the direct effects of GDL laws and the indirect effects due to activities being substitutes or complements. In a reduced-form analysis, such a decomposition would be confounded by correlation in teens' preferences for school and work. Exclusion restrictions based on the same policies used in the triple-difference designs enable separating the substitutability or complementarity of school and work from such correlated preferences. The structural estimates reveal that employment is not a strong substitute for high school attendance—in fact, they are weak complements. Counterfactual simulations indicate that improved high school retention from GDL laws is thus *not* due to reductions in labor market access, but instead reflects decreased access to other activities.[2]

This paper offers several contributions. The first is to a small but growing literature that seeks to understand the consequences of teen mobility and restrictions thereto. Teenage driving is risky, and mortality rates increase after the onset of driving age (Huh and Reif 2021). Several studies find that GDL laws and related policies substantially reduce the injury and fatality risk teens face by limiting driving (Dee, Grabowski, and Morrisey 2005; Shults, Olsen, and Williams 2015; Moore and Morris 2024), though GDL laws do not improve driving safety in the long run (Karaca-Mandic and Ridgeway 2010; Gilpin 2019). Related research examines the effects of these policies on non-driving outcomes. Deza and Litwok (2016) and Deza (2019) provide evidence that GDL laws reduce

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

teen criminal activity and pregnancy. Argys, Mroz, and Pitts (2019) show that GDL laws explain about half of the drop in teen labor force participation in the U.S. since 1995, which is consistent with our mechanism analysis findings.[3] Our findings complement this literature. We: (1) show that GDL laws impact an important teen outcome with long-lived consequences—high school completion; (2) reveal a key interaction with compulsory schooling policies; and (3) provide a structural framework to interpret effects.

Our paper also offers insights into the determinants of educational attainment and high school dropout. A much studied policy intended to impact these outcomes is to legally compel schooling (Angrist and Krueger 1991; Acemoglu and Angrist 2000; Lleras-Muney 2002; Oreopoulos 2009). In summarizing the effects of compulsory schooling on educational attainment, Oreopoulos (2007) concludes, "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 study interfaces with this sentiment by suggesting that the interaction of GDL and compulsory schooling laws shifts access to activities that may distract teens from completing high school. This augments the literature linking non-education policies to high school dropout behavior (Cohodes et al. 2016; Lovenheim, Reback, and Wedenoja 2016; Miller and Wherry 2018; Groves 2020) and connecting leisure activities, and especially risky behaviors, to dropout (Bray et al. 2000; Koch and McGeary 2005; Crispin 2017).[4]

We also provide new insights into teen employment decisions. While our reduced-form estimates show that GDL laws reduce teen labor force participation (as in Argys, Mroz, and Pitts 2019), we provide clarity as to how education and labor responses to GDL laws are interrelated. Structural estimates reveal that GDL policies directly limit access to employment but also show that school and work are complements.[5] Thus, it is not reduced work access that causes the increase in high school retention. Our findings

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[3]A related strand of research studies "No Pass, No Drive" laws that restrict driving if academic performance is poor, finding that they delay the decision to drop out of high school (Kennedy 2020), improve graduation rates for some groups (Barua and Vidal-Fernandez 2014), and decrease crime (Barua, Hoefer-Marti, and Vidal-Fernandez 2024).

[4]Anderson (2014) and Bell, Costa, and Machin (2016) show that the inverse channel, from educational policies to risky behaviors, is also present.

[5]The evidence on the impact of working while in high school largely shows that part-time employment while in school is not detrimental to academic success, implying at least weak complementarity (Montmarquette, Viennot-Briot, and Dagenais 2007; Dustmann and van Soest 2008; Ruhm 1997). Our structural results support this narrative: While teen preferences for schoolgoing and work are negatively correlated, school and work are not substitutes on average.

also complement evidence that mobility restrictions impact the labor supply of non-teen groups (Black, Kolesnikova, and Taylor 2014; Amuedo-Dorantes, Arenas-Arroyo, and Sevilla 2020) and updates the literature linking teen education and employment (e.g., Eckstein and Wolpin 1999).

Finally, we contribute a structural framework for policy analysis that reflects the triple-difference design. The model distinguishes mechanisms, separating direct from indirect (substitution) effects. We adapt the model to contexts with no "outside option" unaffected by the policy of interest by using additional restrictions to set identify a normalizing parameter. The model retains a primary focus on identifying policy parameters while adding structure to gain insight and interpretation; relatively few papers combine quasi-experimental research design with discrete choice models for policy evaluation (an exception is Li 2018).[6]

We describe the background and context for our study and detail data sources in Section 2. In Section 3, we describe the research design, the advantages and disadvantages of each of the three estimators we apply to this design, and address potential threats to identification. Section 4 presents the main results on education outcomes as well as an array of robustness checks employing alternate model specifications, identification strategies, and data sources. We also show that effects persist into adulthood and explore heterogeneity across subgroups of the population. In Section 5, we investigate potential mechanisms by exploiting variation in the intensity of GDL restrictions across states and by looking at GDL effects on teen employment outcomes. Section 6 unites education and employment decisions within a structural model to formally decompose the effects of GDL laws into the direct effects of mobility restriction and the indirect effects of activity substitution.

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. Graduated driver licensing laws are one such policy that began to be widely adopted starting in the mid 1990s. GDL laws often limit full-privilege licenses to older (>16) teens and create an intermediate licensing level that restricts nighttime driving and the number of passengers who may ride with a teen driver. GDL laws have been shown to have

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[6]An extensive literature applies dynamic structural modeling to human capital accumulation. Given our repeated cross-sectional data, our approach instead grows out of product choice models from industrial organization (e.g., Berry, Levinsohn, and Pakes 1995; Goolsbee and Petrin 2004; Gentzkow 2007).

reduced teen traffic fatalities by over 50% in both the U.S. and Australia (Dee, Grabowski, and Morrisey 2005; Shults, Olsen, and Williams 2015; Moore and Morris 2024). Previous studies have further shown that GDL laws decrease fatalities primarily by decreasing teen driving rather than by improving the quality of teen driving, implying restricted mobility (Karaca-Mandic and Ridgeway 2010; Gilpin 2019).

We develop a database of 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.[7] Figure 1a shows counts of the number of states with various types of GDL laws over time.[8] Prior to 1995, fewer than ten states limited full-privilege licenses to those older than 16 or had nighttime driving restrictions on teens. By 2010, forty-seven states had increased restrictions on teenage driving.

To verify that GDL laws had a binding effect on teen automobile use, we link the GDL law dataset to information from the U.S. 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 reduce the rate of fatal car accidents for 16-year-olds by 27%. This result confirms previous findings from the literature and suggests that GDL laws significantly restrict teen driving. We discuss this verification exercise in detail in Appendix B.

Our research design combines variation in GDL laws with variation in state-level compulsory schooling (CS) laws. Specifically, we use the mandated school-leaving age (the minimum age at which a teen is legally allowed to drop out of school) to create a "placebo" group of teens who are exposed to GDL laws but are restricted from dropping out of high school. We extend 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. 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,

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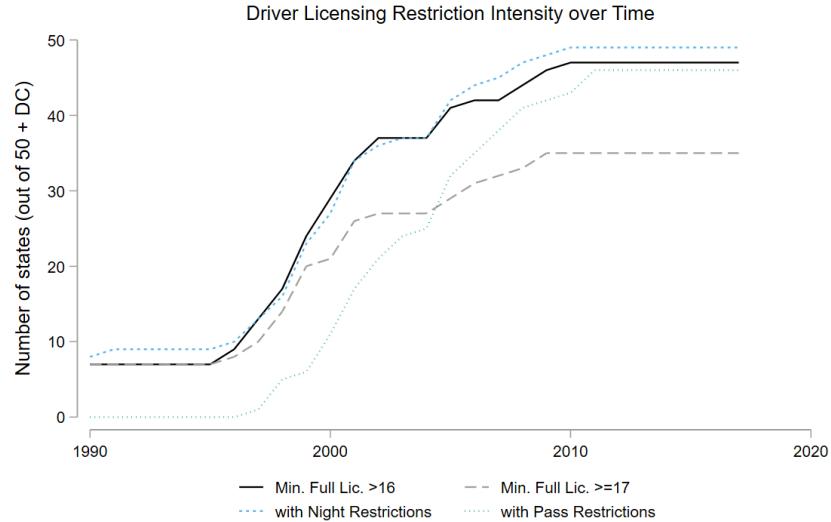
[7]IIHS data begins coverage in 1995. We use FHWA data for the years before 1995 and to rectify uncertainty. The GDL data are based on that used in Severen and Van Benthem (2022).
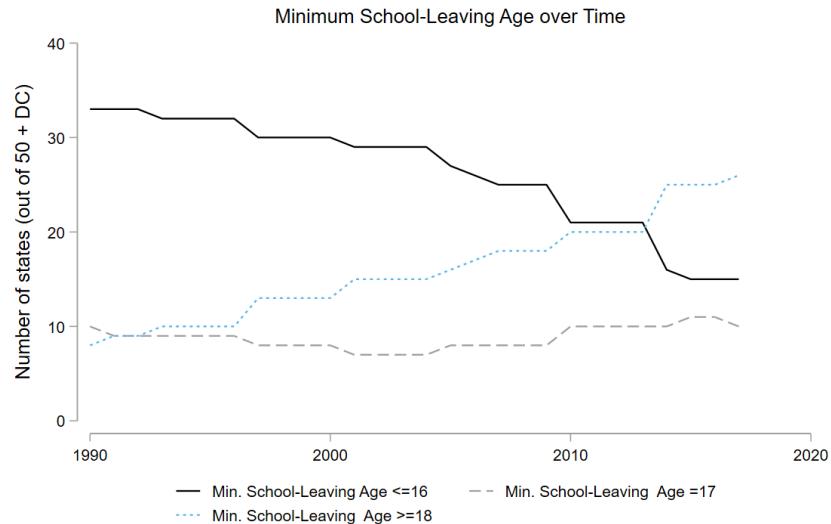
[8]Appendix Figure A.1 shows the same variation as in Figure 1 weighted by state populations (linearly interpolated between census years). Appendix Table A.1 details the years in which GDL laws are adopted.

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

(a) Graduated Driver Licensing Adoption

(b) Minimum Legal 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) (Flood et al. 2023).[9] The CPS ASEC data are from an annual survey

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[9]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 B, we verify that this approach is reasonable.

of U.S. households conducted in March of each year and provide person-level information on a variety of 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.[10] We use these responses 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.[11] CPS ASEC participants are also surveyed on labor force participation and employment status in the preceding week.

To construct the primary 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 unlikely to have completed high school; (2) this is the age most impacted by GDL laws. From 1990 to 2017, 40 states switched from allowing 16-year-olds to obtain full-privilege licenses to restricting this privilege to older teens.[12]

We draw additional data from a variety of other sources. To control for local economic conditions, we include 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.[13] From FRED, we take the maximum of the state and federal minimum wage in each year and inflation-adjust to reflect the binding real minimum wage in each state-year (U.S. Department of Labor). We also use the American Community Survey (ACS) (Ruggles et al. 2024) from 2008 to 2019 for U.S.-born respondents aged 22 to 34 to study longer-run outcomes. Finally, we use Table 1 of Kennedy (2020) to create controls reflecting enrollment-based "No Pass, No Drive" (NPND) laws in each state-year. These laws prohibit teens who drop out of high school from obtaining a driver's license.

Table 1 reports summary statistics for the final linked estimation sample of 75,196 individuals at age 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.

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[10]Students on holiday or seasonal vacation when surveyed are instructed to reply "yes" to this question.

[11]This measure may incorporate some measurement error from those teens who have already completed a high school degree and are not enrolled in college.

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

[13]We use a three-month average of unemployment rates centered on January. For example, the 3-month rate for 1995 is the average of the monthly unemployment rates from December 1994 to February 1995.

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:				
"No Pass, No Drive" Law	0.19	0.39	0	1
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.

3   Empirical Strategy

We seek to identify the effect of teen mobility restrictions (as realized by GDL laws) on high school dropout behavior. The staggered roll-out of GDL laws across states during our observation window provides one source of quasi-random variation for identification. In addition, legal differences across states in compulsory schooling requirements provide a second source of variation that we use to strengthen identification.

In 1990 (the beginning of our sample window), the minimum legal dropout age mandated by CS laws was 16 or less in thirty-three states. When GDL laws were subsequently adopted in those states and 16-year-olds experienced the resulting mobility restrictions, those teens could potentially respond by changing their high school dropout decisions. On the other hand, in the seventeen states where compulsory schooling laws mandated high school attendance through age 17 or 18, 16-year-olds who experienced a mobility re-

striction from GDL law adoption were likely unable to respond by changing their dropout behavior. This variation in compulsory schooling laws generates a sub-group of 16-year-olds that should either be unaffected, or less affected, by GDL law adoption.

Thus, we propose a difference-in-differences-in-differences (triple-difference) model that leverages both the quasi-random staggered adoption of GDL laws, as well as variation across states in the minimum school-leaving age. In a canonical triple-difference model, the first two differences compare outcomes before and after treatment between treated units and untreated units. The third difference is between a subset of the population that is unaffected by treatment and a subset that is susceptible to the treatment. The grouping for the third difference is typically defined using some pre-determined characteristic of the individual, such as gender or age. In our context, the grouping is defined at the state-year level. Specifically, we define the "Treatment" group to be all individuals within a state-year cell where CS laws are not binding and 16-year-olds are legally permitted to drop out of school. We define the "Placebo" group to be all individuals within a state-year cell where CS laws are binding and 16-year-olds are legally compelled to remain in school.

A subtle distinction between our design and canonical triple-differences is that the characteristic defining our third difference can change over time. It is therefore necessary to verify that changes in CS laws, specifically minimum legal dropout ages in a state, are uncorrelated with the timing of GDL law adoption. Doing so supports treating the state's CS law as an exogenous characteristic (we address all identification requirements in detail in Section 3.2).

We estimate the effect of GDL mobility restrictions on dropout behavior with the following specification for the sample of 16-year-olds:

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

where GDL_{st} is an indicator equal to 1 if the minimum unrestricted driving age in state s in year t is greater than 16 (i.e., 16-year-olds experience mobility restrictions).[14] We capture compulsory schooling laws with CS_{st}, an indicator that equals 1 if the minimum school-leaving age is ≤ 16 (i.e., 16-year-olds are legally permitted to drop out of

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[14]We consider as driving restrictions: limits on 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. We explore alternate measures of GDL intensity in Section 5.1.

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. And Z_{st} includes time-varying state-level controls for minimum wage, unemployment rate, and presence of a "No Pass, No Drive" (NPND) law.[15] The model also includes 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). Standard errors permit clustering at the state level.[16]

In Equation 1, β_3 is the primary effect of interest and represents the impact of GDL law adoption on high school dropout behavior. In the triple-difference framework, β_3 is the difference between the difference-in-differences estimate for the Treatment group and the difference-in-differences estimate for the Placebo group. The sign of β_3 is ex ante ambiguous due to the several channels through which GDL laws might impact teen educational attainment. First, the introduction of a GDL law restricting teen driving may have a *direct* effect on dropout decisions if the restriction hinders teens' ability to commute to school. In particular, for teens with little access to alternative transportation, the direct effect may *increase* high school dropout rates.[17] However, the mobility restrictions imposed by GDL laws could also impact a teen's dropout decision *indirectly* by limiting access to labor and leisure activities. The signs on these indirect effects depend on whether schooling and employment (or schooling and leisure) are complements or substitutes for teens. If work is a substitute for schooling, then reducing access to employment decreases high school dropout. However, if a teen views the two activities as complements, then the indirect effect has the reverse sign and could increase high school dropout. Thus, the total effect of GDL laws on high school dropout rates (β_3) is positive if direct effects dominate, but may be either positive or negative if indirect effects are significant.

The coefficients β_1 and β_2 are also of interest. In particular, β_1 represents the difference-in-differences for the Placebo group and captures the effect of imposing GDL-based mobility restrictions on high school dropout behavior in states where 16-year-olds cannot

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[15]We construct an indicator for whether state s has enacted an enrollment-based NPND in or prior to year t that does *not* include exemptions for: hardship, employment, GED, or parental permission.

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

[17]The availability of busing services may also impact school access. While our data do not include travel mode, more than 77% of students aged 16–18 used a car to get to school in 2001 (*National Household Travel Survey Travel to School: The Distance Factor* 2008). The share of students in grades 9–12 who traveled by school bus increased from 19% to 26% between 1995 and 2009 (McDonald et al. 2011), before falling again to 21% by 2017 (Lidbe et al. 2020).

legally drop out. We expect the value of β_1 to be close to zero. If it is not, this could indicate that the timing of GDL law adoption is correlated with some unobserved factor that determines teen dropout rates (e.g., local low-skill labor market shocks). However, this is a key benefit of the triple-difference design, as this type of omitted variable bias will be absorbed by β_1 and differenced out of β_3, making the main coefficient robust to unobserved shocks that are common to both the Treatment and Placebo groups.

The coefficient β_2 reflects the impact of a non-binding compulsory schooling law on dropout behavior in the absence of GDL laws. While we do not target causal identification of this parameter, based on previous studies of the impacts of CS laws (Anderson 2014; Oreopoulos 2009), we expect this coefficient to be positive and significant, indicating that lower minimum school-leaving age restrictions lead to higher dropout rates.[18]

3.1 Estimators

We estimate the triple-difference model in Equation 1 using three different estimators, each with their own advantages and disadvantages. Results (reported in Section 4) are generally consistent across all three estimators, demonstrating that our main findings are not driven by the unique assumptions required for any one of these estimators.

First, we use a two-way fixed-effect ordinary least squares (TWFE OLS) estimator. This estimator is straightforward to calculate and has attractive properties for inference. However, it is now well-established that, in order for the TWFE OLS estimator to identify an unbiased average treatment effect in a staggered adoption design, an additional assumption must be satisfied: the treatment effect should be constant between groups and over time (de Chaisemartin and D'Haultfœuille 2023). In our context, this equates to an assumption of constant and homogeneous treatment effects of GDL laws within both the Treatment and Placebo groups. A second potential pitfall of the OLS estimator is that it may predict dropout probabilities that lie outside of the unit interval (Table 1 reports that the in-sample dropout rate is fairly close to zero at 3.8%), leading to biased estimates (Horrace and Oaxaca 2006).

To address the former limitation, we apply the imputation estimator developed in

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[18]For β_2 to represent a well-identified causal effect of compulsory schooling laws in our setting would require a more stringent set of assumptions based on the "interacted treatments" literature (e.g., Johnson and Jackson 2019). Specifically, identification requires assuming parallel counterfactual trends across states in the absence of GDL laws adoption *as well as* parallel trends across states in the absence of CS law changes. While we find no evidence of differential pre-trends along either of these dimensions, we do not present those event studies in order to focus on the effects of GDL law adoption.

Borusyak, Jaravel, and Spiess (2024) (hereafter BJS). Unlike the TWFE OLS estimator, the BJS estimator recovers a well-defined average treatment effect on the treated even under arbitrary treatment-effect heterogeneity and dynamism. It is also the most efficient linear unbiased estimator given pre-specified weighted sums of treatment effects under parallel trends and homoskedasticity, and retains attractive inference properties under other error processes.[19]

One limitation of the BJS estimator (along with all other estimators in the current set of heterogeneity-robust difference-in-differences methods), is that it does not utilize information from always-treated units (states). Furthermore, these estimators cannot accommodate a treatment variable that "turns on" and then "turns off"—i.e., treatment must be an absorbing state. In the context of our triple-difference model, treatment "turns on" when a state has *both* an active GDL law and a non-binding compulsory school law (meaning that 16-year-olds are legally permitted to drop out of school). This was common in the beginning portion of our sample, as many states had lenient CS laws and gradually adopted GDL laws between 1995 and 2005. Starting after 2005, more states began to increase the minimum school-leaving age to >16, causing treatment to "turn off" again.[20] We address both of these limitations by trimming the sample for the BJS estimation. Specifically, we drop all data from the seven always-treated states and, within each treated state, we also drop any observations that occur after treatment "turns off" due to adoption of a more stringent CS law. This means that our sample (and power to detect small effects) will be somewhat diminished when using the BJS estimator.

Finally, to address the potential issue of predicted probabilities outside of the unit interval, we also estimate Equation 1 using probit maximum likelihood. This estimator better fits the binary nature of the outcome variable of interest, however it imposes the additional assumption of normally distributed errors. Ultimately, the resulting estimates (shown in Section 4) from all three estimators are highly similar.

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[19]BJS is preferable to other heterogeneity-robust estimators in our setting (such as Callaway and Sant'Anna 2021). Alternate estimators rely on comparisons based on small subsets of pre-treatment periods, which often have only a few observations in our data. Moreover, BJS is more computationally robust than estimators that individually estimate and aggregate all possible 2x2 difference-in-differences designs. The repeated cross-sectional data in our sample include many 2x2 designs that are based on a small number of observations and are therefore noisy. The imputation approach uses more information to estimate st-specific treatment effects and so is more efficient.

[20]To illustrate, the solid black line in Appendix Figure A.2 plots the number of states for which the combined treatment status is equal to one over time.

3.2 Identification

A causal interpretation of β_3 in Equation 1 requires two key identifying assumptions: (A1) the timing of CS law changes is exogenous with respect to the timing of GDL law adoption; and (A2) counterfactual outcomes follow parallel trends in ratios.

The first assumption, (A1), permits treating CS laws as an exogenous dimension providing the third difference for the triple-difference design. Specifically, (A1) requires that changes to the minimum legal school-leaving age in a state over time are not correlated with the timing of the adoption of GDL laws in that state. To probe whether these two policies are adopted independently, we first note that states rarely changed both policies simultaneously.[21] Most states adopted GDL laws between 1995 and 2005, during which time only four states changed restrictions on the relevant minimum school-leaving age. States later began to gradually implement more stringent CS laws throughout our sample window, with an increase in pace from 2010–2015. As a test of assumption (A1), we regress GDL_{st} on CS_{st}, along with state-level fixed effects, year fixed effects, and the full set of covariates listed in Equation 1. The estimated correlation from this model is quantitatively small and statistically insignificant. The point estimate of -0.0062 (p-value = 0.885) indicates very little correlation in the take-up of these two policies. Finally, to quell any lingering concern over the conditional exogeneity of changes in compulsory schooling laws, we also show in Section 4.1 that our main findings are robust to alternate specifications in which we remove the time dimension of variation from the CS_{st} variable.

The second identifying assumption, (A2), is the standard triple-difference requirement: parallel counterfactual trends in the outcome variable across the Treatment and Placebo groups in the absence of GDL law adoption. Olden and Møen (2022) refer to this requirement as the "parallel trend assumption, in ratios." This assumption is substantively *less restrictive* than the parallel trends assumption required for standard difference-in-differences. To build intuition, consider two separate difference-in-differences designs, one for the sample in the Treatment group (where teens can freely drop out) and one for the sample in the Placebo group (i.e., where dropout is not legal). The triple difference estimator recovers a causal estimate of the treatment effect even if the two separate difference-in-differences estimates are biased *as long as the bias is the same in both groups* (Olden and Møen 2022). The third difference subtracts out that bias and yields an unbiased tripe-difference estimator.

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[21]We detail the timing of both policy changes in each state in Appendix Table A.1 and depict the variation in both policies in Figure 1 and their interaction in Appendix Figure A.2.

To probe the validity of (A2), we estimate several event study models. These allow visual and statistical assessment of the potential presence of differential pre-treatment trends. We first estimate separate dynamic difference-in-differences models for the Placebo and Treatment groups. Although this is informative about differential pre-trends in each difference-in-differences sample, recall that we do not necessarily need to see evidence of parallel trends in either of these event studies. Instead, to support the parallel trends in ratios assumption (A2), any pre-trends should be statistically indistinguishable *across* the two models. If outcomes are trending similarly across the Placebo and Treatment groups in the pre-treatment periods, we assume that they would continue to trend similarly in the counterfactual absence of treatment.

We estimate the following dynamic difference-in-differences model separately for each $g \equiv CS_{st} \in \{0, 1\}$:

$$NotInSchool_{gist} = \sum_{k=-5+}^{-2} \theta_{g,k} GDL_{s,t+k} + \sum_{k=0}^{5+} \theta_{g,k} GDL_{s,t+k} + X_i' \nu_g + Z_{st}' \mu_g$$
$$+ D_{gs} + D_{gt} + \epsilon_{gist}, \tag{2}$$
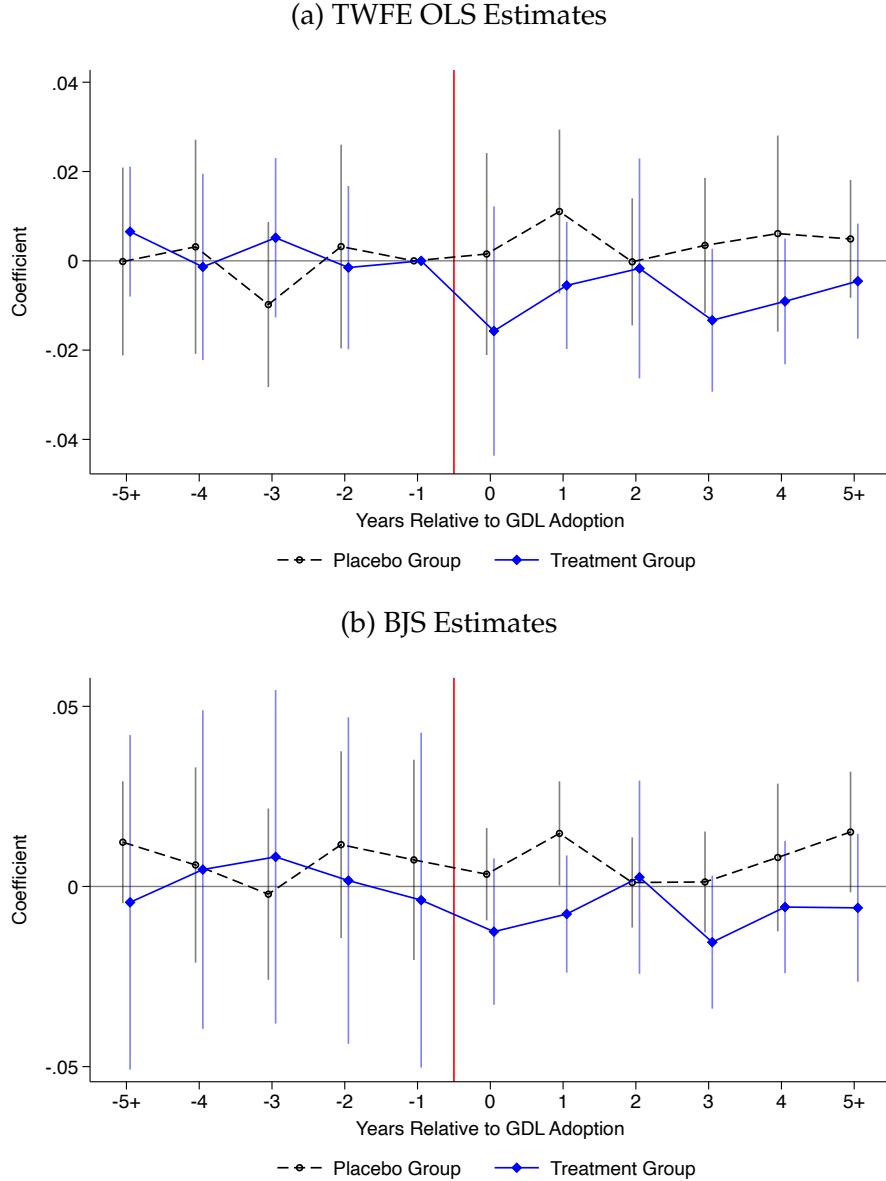
where each $GDL_{s,t+k}$ is an indicator for k years from the adoption of a GDL law and g indexes the two possible CS law regimes. All other variables are defined as in Equation 1. Coefficients $\theta_{0,k}$ represent the pre- and post-treatment effects of GDL law adoption in the Placebo group where CS laws are binding and 16-year-olds are legally required to stay in school ($CS_{st} = 0$).[22] Estimates and confidence intervals for $\theta_{0,k}$ are shown in the black, dashed lines in Figure 2. Estimates of $\theta_{1,k}$ are shown in solid blue and represent the dynamic effects of GDL law adoption in the Treatment group where CS laws are non-binding and dropout is legally permitted for 16-year-olds ($CS_{st} = 1$). Panel A of Figure 2 displays TWFE OLS estimates of Equation 2, while Panel B shows the corresponding BJS estimates.[23]

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[22]TWFE OLS estimates restrict the effect of GDL laws on cohorts who turned 16 more than five years before or five years after the law went into effect to be constant so that $\theta_{g,-5}$ and $\theta_{g,5}$ represent the average effect five or more years prior to or after the GDL law adoption, respectively.

[23]The TWFE OLS estimator omits the time period $k = -1$. In contrast, the BJS estimator does not use the last untreated time period ($k = -1$) as the reference group, instead using all omitted pre-treatment time periods (as well as the never-treated units). We therefore want to include as many pre-treatment time periods as possible, while simultaneously ensuring that the reference group is sizable enough. For the placebo states, we estimate five pre-treatment periods. For the treatment states, we have a sizable never-treated group and a longer panel and so are able to estimate eighteen pre-treatment periods. Only five are displayed here for harmony with the other graphs. We do not estimate event study models using the probit estimator due to the incidental parameters problem.

Figure 2: The Effect of Minimum Unrestricted Driving Age on 16-Year-Old Dropout, Dynamic Difference-in-Differences

(a) TWFE OLS Estimates

(b) BJS Estimates

Coefficient estimates of $\theta_{g,k}$ and 95% confidence intervals using CPS ASEC data from 1990–2017. Controls include: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; NPND laws; state log real effective minimum wage, state fixed effects, and year fixed effects. Standard errors are clustered at the state level.

Pre-treatment estimates in both panels of Figure 2 reveal no significant pre-trends in either the Placebo or Treatment group. More importantly, these estimates show that the

pre-trends in the two separate difference-in-differences models are statistically indistinguishable from one another. This lends strong support to the identifying assumption of parallel counterfactual trends in ratios (A2). Furthermore, the fact that there is no evidence of differential pre-trends in either of the separate difference-in-differences models indicates that we could also reasonably make the more stringent assumption of separate parallel counterfactual trends in both groups, per the "interacted treatments" literature (e.g., Johnson and Jackson 2019).

In addition to providing an indirect test of identifying assumptions, Figure 2 also provides a first piece of evidence that GDL laws cause a reduction in high school dropouts. While pre-treatment point estimates are similar and very close to zero in both the Placebo and Treatment groups, outcomes for these two groups start to deviate once GDL laws are adopted. The estimated effect of GDL law adoption on the probability of dropout remains very close to zero or positive in the post-treatment years for the Placebo group. In contrast, there is a clear decline in the probability of 16-year-old dropout that coincides with GDL law adoption in the Treatment group. Finally, the takeaways from these event study estimates are the same across estimators (i.e., the BJS estimates in Panel B confirm the findings of the TWFE OLS estimates in Panel A).

We further explore these results by estimating a dynamic triple-difference model:

$$
\begin{aligned}
NotInSchool_{ist} = & \sum_{k=-5+}^{-2} \gamma_k GDL_{s,t+k} + \sum_{k=0}^{5+} \gamma_k GDL_{s,t+k} + \beta CS_{st} \\
& + \sum_{k=-5+}^{-2} \delta_k GDL_{s,t+k} * CS_{st} + \sum_{k=0}^{5+} \delta_k GDL_{s,t+k} * CS_{st} \\
& + X_i' \nu + Z_{st}' \mu + D_s + D_t + \epsilon_{ist}.
\end{aligned}
\tag{3}
$$
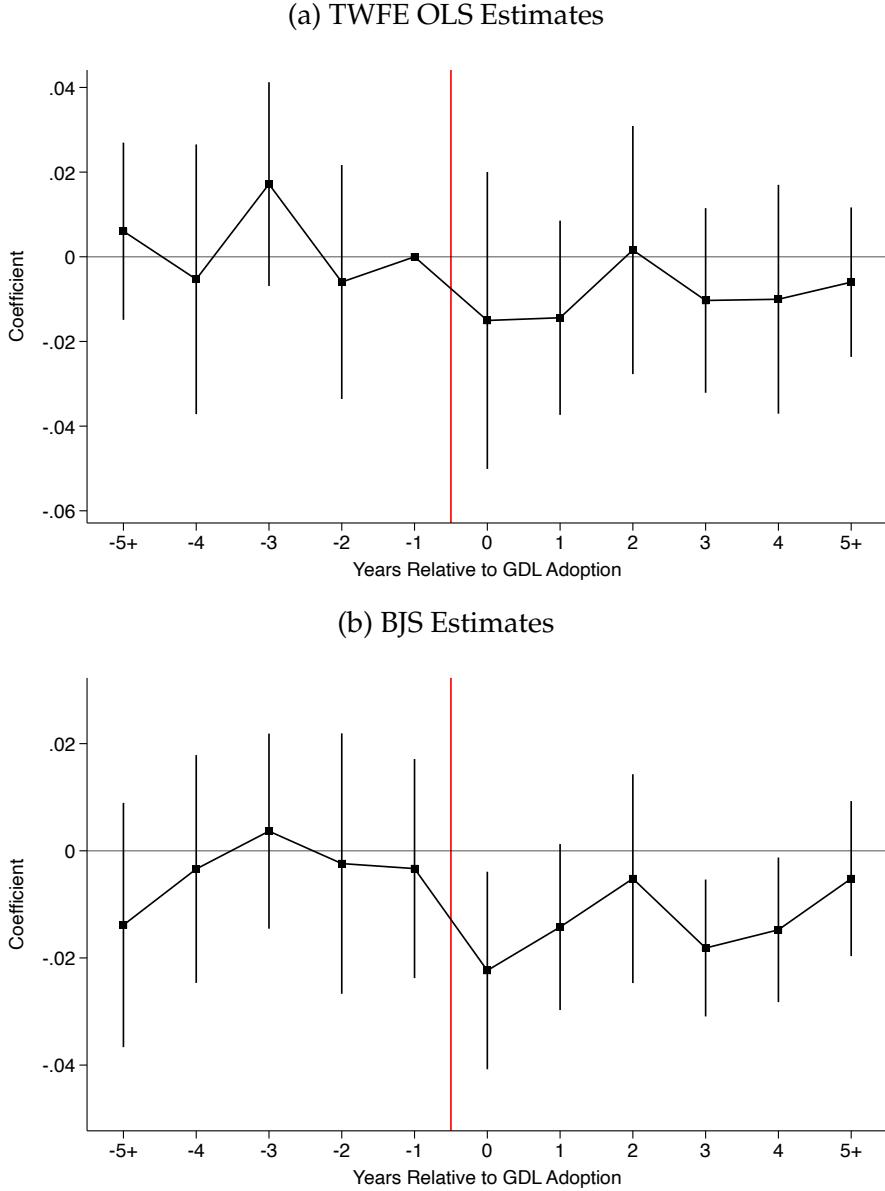
Each γ_k coefficient in Equation 3 is conceptually equivalent to the corresponding $\theta_{0,k}$ coefficient in Equation 2 and represents the pre- or post-treatment effect of GDL laws in the Placebo group. Similarly, the δ_k coefficients are conceptually equal to the difference in coefficients $\theta_{1,k} - \theta_{0,k}$ from Equation 2.[24] These represent the pre- and post-treatment effects of GDL laws in the Treatment group net of any bias captured by the difference-in-difference effect of GDL laws in the Placebo group.

Estimates of the δ_k coefficients and corresponding 95% confidence intervals are plotted

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[24]This mapping is not numerically exact because the estimates of the fixed effects and the coefficients on the covariates may vary between Equations 2 and 3.

Figure 3: The Effect of Minimum Unrestricted Driving Age on 16-Year-Old Dropout, Dynamic Triple-Difference

(a) TWFE OLS Estimates

(b) BJS Estimates

Coefficient estimates of δ_k and 95% confidence intervals using CPS ASEC data from 1990–2017. Controls include: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; NPND laws; state log real effective minimum wage, state fixed effects, and year fixed effects. Standard errors are clustered at the state level.

in Figure 3. Panel A displays the TWFE OLS estimates and Panel B shows the correspond-

ing BJS estimates.[25] The pre-treatment estimates in Figure 3 provide a more direct test of assumption (A2), as they reveal the difference in pre-trends between the Treatment and Placebo groups. Pre-treatment point estimates are all small and statistically indistinguishable from zero in both panels, providing further evidence in support of identification. Moreover, Figure 3 reports post-treatment point estimates that are consistently negative across most time periods (although they are not always statistically significant). These point estimates suggest a decline in the probability of 16-year-old dropout in response to GDL law adoption of up to 2.2pp.

The triple difference identification strategy addresses many potential sources of bias in comparing outcomes across states. The first two differences remove differences in levels across states and broad secular trends over time. The third difference removes any bias generated by correlation in changes in educational outcomes and the timing of GDL law adoption, under conditions (A1) and (A2). The remaining threat to identification lies in the possibility of some other latent trend impacting the Treatment group more than the Placebo group. The third difference only captures bias generated by non-random timing of GDL law adoption to the extent that unobservable factors are strong enough to influence dropout decisions in the face of binding CS laws. For example, if GDL law adoption were correlated with shocks to each state's low-skill labor market and those shocks reduced high school dropout in the Treatment group, but were not "strong enough" to change the dropout decisions of teens in the Placebo group, then assumption (A2) is violated. However, event study evidence suggests that such differential trends are not present prior to treatment. This means that the remaining threat to identification is that such differential impacts only emerged between the Treatment and Placebo groups immediately after GDL laws were enacted.

Finally, if our third difference is badly defined—if teens are able to respond to mobility restrictions by dropping out regardless of CS statutes—then our model collapses down to a difference-in-difference model, allowing for heterogeneity in effects by CS law.[26] In this case, we would rely on the traditional parallel trends assumption holding in both the Treatment and Placebo groups, which is consistent with the event studies shown in Figure 2. Ultimately, this concern is negated by the near-zero point estimates of β_1 across

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[25]The BJS estimator uses ten pre-treatment periods. Only five are displayed here for harmony with the other graphs. See footnote 23 for more detail.

[26]This may be a particular concern in rural areas where school attendance zones are expansive and school buses often require long commutes (Howley, Howley, and Shamblen 2001). GDL-induced mobility restrictions there could be large enough to increase high school dropout rates, even in states with binding compulsory schooling laws.

all specifications shown in Table 2.

Given the initial evidence of a reduction in high school dropout in response to GDL law mobility restrictions shown in Figures 2 and 3, we turn to estimating the triple-difference specification given in Equation 1. By aggregating the pre- and post-treatment years, this model provides more power to detect average treatment effects.

4   Results

Table 2 presents estimates of the coefficients of interest in Equation 1. Columns (1) and (2) show the TWFE OLS estimates. Columns (3) and (4) show the BJS imputation estimates.[27] Finally, columns (5) and (6) show the average marginal effects corresponding to the probit estimates. Specifications in odd-numbered columns exclude control variables (X_i and Z_{st}), while those in even-numbered columns include all controls. Comparing estimates across columns, results are not sensitive to or driven by the inclusion of covariates. Importantly, estimates of all three coefficients of interest are also strikingly similar across all proposed estimators. This provides reassurance that underlying assumptions particular to any one of the estimators is unlikely to be driving these results.

The triple-difference estimates of β_3 indicate that the effect of GDL laws on dropout behavior is negative and statistically significant. These estimates reveal that increasing the minimum driving age above 16 reduces the probability that 16-year-olds are no longer in school by approximately 1.1pp to 1.3pp, a 29% to 35% reduction from the mean. This negative effect indicates that teens respond to reduced access to driving by staying in school longer. This unintended consequence of GDL laws may have long-run implications for the trajectories of the affected teen population. The returns to an additional year of high school education have been estimated to increase lifetime wealth by 15% (Oreopoulos 2007). Thus, even a small reduction in dropout rates can have large impacts on long-run well-being.

The negative sign of this net effect also indicates that any potential direct effect of GDL laws on high school attendance (from making it harder to commute to school) is more than completely offset by the indirect effects of GDL laws operating through reduced access to other activities, such as labor or leisure (or both). Thus, the net effect of direct and indirect channels is to increase the length of time that teens stay in school. We investigate potential mechanisms for this net effect and attempt to disentangle direct and

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[27]Recall that this estimator excludes always-treated units and therefore has a smaller sample.

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

	Not In School = 1					
	(1)	(2)	(3)	(4)	(5)	(6)
Min. Unres. Driving Age >16 (β_1)	0.0019	0.0009	0.0048	0.0036	0.0022	0.0014
	(0.0041)	(0.0038)	(0.0047)	(0.0042)	(0.0043)	(0.0040)
School-Leaving Age $\leq$ 16 (β_2)	0.0207***	0.0195***	0.0190***	0.0210***	0.0200***	0.0191***
	(0.0049)	(0.0047)	(0.0067)	(0.0075)	(0.0049)	(0.0049)
Min. Unres. Driving Age >16	-0.0119**	-0.0110**	-0.0126***	-0.0125***	-0.0132***	-0.0129**
$\times$ School-Leaving Age $\leq$ 16 (β_3)	(0.0045)	(0.0047)	(0.0045)	(0.0046)	(0.0050)	(0.0052)
Estimator	TWFE	TWFE	BJS	BJS	Probit	Probit
Controls	-	Y	-	Y	-	Y
Exclude Always Treated	-	-	Y	Y	-	-
Obs	75,196	75,196	59,207	59,207	75,196	75,196

TWFE OLS estimates (columns 1–2), imputation estimates of Borusyak, Jaravel, and Spiess (2024) (columns 3–4), and average marginal effects from probit regression (columns 5–6) 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; NPND laws; 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

indirect effects in Section 5 and Section 6.

Estimates of β_1, which represent the effect of GDL laws on dropout behavior in the Placebo group, are small and statistically insignificant. This reinforces the evidence in Figure 2, further confirming that there is no effect of GDL laws on 16-year-old dropout behavior when dropout is legally prohibited. This also supports the assertion that the timing of GDL law adoption was quasi-random. If adoption had been correlated with unobserved factors influencing teen dropout decisions, we might have expected to observe a significantly non-zero estimate of β_1 (capturing the correlation between high school attendance and those other factors in the Placebo group).

As expected, estimates of β_2 are positive 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 higher 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).[28] As we use more recent data than those papers, this provides some new evidence

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[28]Oreopoulos (2009) finds that a school-leaving age below 16 increases the fraction of 20- to 24-year-olds with less than a high school degree by 1.3pp. Our estimates in Table 2 are slightly larger (1.9 - 2.1pp), however we measure dropout in a younger population, at age 16. In Section 4.2, we investigate longer-run effects of GDL and CS laws and find a smaller effect (1.1pp) of school-leaving age on the probability of

that compulsory schooling laws continue to be impactful for educational attainment.[29]

4.1 Alternative Specifications, Data, and Identification Strategies

We provide several robustness checks to support identification. In Table 3, we estimate alternate specifications that provide additional evidence that the changes in compulsory schooling laws over time are not contaminating the main estimates. First, 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 state first adopts a GDL law that 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.[30] Results, shown in columns (1)–(3) of Table 3, are nearly identical to the main findings in Table 2.[31] Second, we estimate Equation 1 on the sub-sample of states that never changed their minimum school-leaving age during the time period under study. Results, shown in columns (4)–(6) of Table 3, are a bit larger in magnitude than in the main specifications and remain statistically significant, despite the reduced sample size.[32]

Given that the estimates of β_1 across both Tables 2 and 3 reveal very little evidence of bias in the difference-in-differences estimate for the Placebo group, we estimate a stand-alone difference-in-differences model for the sub-sample in the Treatment group. This is analogous to aggregating all pre- and post-treatment years of the event study model in Equation 2 for observations where $CS_{st} = 1$. A drawback to this approach is that it significantly limits the size of the estimation sample. Appendix Table A.2 reports results from this model estimated via the TWFE OLS, BJS, and probit estimators. OLS and probit estimates confirm that GDL laws reduce the probability of 16-year-old dropout by approximately 1.1pp. These estimates are significant at the 10% level. The BJS estimate is slightly smaller at -0.9pp and is not statistically significant (p-value of 0.169). However

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having less than a HS diploma at age 22-34. Our estimates are also similar to those in Anderson (2014), who finds that a school-leaving age of 18 or older reduced high school dropout rates by 2pp.

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

[30]This specification also controls separately for the actual time-varying school-leaving age.

[31]In these alternate specifications, the coefficient β_2 is absorbed by the state fixed effects.

[32]An additional concern regarding CS laws may be that employment exemptions (which allow teens to drop out of school prior to reaching the age threshold if they are employed) cause measurement error in the CS_{st} variable. We collect data on such exemptions (from Bell, Costa, and Machin 2022) and show that results are robust to dropping states with employment exemptions in their CS laws. These results are available upon request.

Table 3: The Effect of Minimum Unrestricted Driving Age on 16-Year-Old Dropout, Robustness

| | Compulsory Schooling Law: | | | | | |
| | Fixed in Year of GDL Adoption | | | Never-Switchers Only | | |
	(1)	(2)	(3)	(4)	(5)	(6)
Min. Unres. Driving Age >16 (β_1)	0.0011	0.0041	0.0014	0.0023	0.0071	0.0033
	(0.0039)	(0.0039)	(0.0042)	(0.0049)	(0.0051)	(0.0051)
School-Leaving Age ≤ 16 (β_2)						
Min. Unres. Driving Age >16	-0.0110**	-0.0116**	-0.0123**	-0.0165**	-0.0160***	-0.0200***
$\times$ School-Leaving Age ≤ 16 (β_3)	(0.0052)	(0.0048)	(0.0058)	(0.0065)	(0.0061)	(0.0077)
Estimator	TWFE	BJS	Probit	TWFE	BJS	Probit
Exclude Always Treated	-	Y	-	-	Y	-
Obs	75,196	64,089	75,196	46,567	39,333	46,567

Columns (1)–(3) fix school-leaving age to its level in the year that the state increased minimum unrestricted driving age to >16, while columns (4)–(6) limit the sample to states that never changed school-leaving age. TWFE OLS estimates (columns 1 and 4), imputation estimates of Borusyak, Jaravel, and Spiess (2024) (columns 2 and 5), and average marginal effects from probit regression (columns 3 and 6) using CPS ASEC data from 1990–2017. All specifications include state and year fixed effects as well as covariates: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; NPND laws; state unemployment rate; and state log real effective minimum wage. Columns (1)–(3) also include 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

this is unsurprising, given that this estimator omits observations from always-treated units and thus employs an even smaller sub-sample (one-third the size of the main estimation sample).

To further support our main findings, we next show that our results can be replicated using an alternative dataset and alternative identification strategy. We conduct a complementary analysis of the impact of GDL laws on teen dropout decisions at the school-district level using the NCES' Common Core of Data. The Common Core is a comprehensive national database of public elementary and secondary schools and provides high school dropout rates aggregated at the school district-by-grade level. A key advantage of this dataset is that we can include school-district fixed effects to control for time-invariant differences between places within states; we detail this data and analysis in Appendix C.

We find that the implementation of GDL laws leads to a 0.4pp reduction in overall high school dropout rates in the NCES data (a 13% reduction at the mean). Furthermore, the effects of increasing the minimum driving age to over 16 are largest in the 11th grade (a grade in which students are likely to be 16 years old and thus directly affected by GDL

laws)—a 15% reduction from the mean. The NCES data are reported at the district-by-grade level and so estimates from this analysis 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. Furthermore, within a grade, some students might be restricted by the state's compulsory schooling laws while others within the same grade are not. Because of these dual sources of measurement error, it is unsurprising that these estimates are smaller in magnitude than those reported in Table 2. However, we view these results as supporting our main findings and adding compelling evidence that imposing restrictions on teen mobility maintains high school enrollment.

4.2   Medium- and Long-Run Effects

Thus far, we have focused on the immediate impacts of GDL laws on the behavior of 16-year-olds. To determine whether GDL laws have long-lasting effects on educational attainment, we extend our analysis to study medium-run effects on the dropout behavior of treated teens at age 17. We then estimate long-run effects on eventual high school completion in an adult sample using census data.

To estimate medium-run effects, we create an alternate sample of 17-year-olds from the CPS ASEC and estimate a slightly modified version of the triple-difference model:

$$NotInSchool_{ist} = \beta_1 GDL_{st-1} + \beta_2 CS_{st} + \beta_3 GDL_{st-1} * CS_{st} + X_i'\nu + D_s + D_t + \epsilon_{ist}, \quad (4)$$

where GDL_{st-1} is an indicator variable that equals one if the minimum unrestricted driving age in state s was > 16 in year $t-1$. That is, we link the sample of 17-year-olds to their state's GDL laws from the previous year (when the individual was aged 16). Compulsory schooling laws are captured by CS_{st}, which here equals one if the minimum school-leaving age is ≤ 17 (i.e., 17-year-olds are legally permitted to drop out of school). We retain gender and race/ethnicity controls (as in Equation 1), as well as state and year fixed effects, but drop other controls for consistency with the longer-run results below. Given the robustness of the main findings across all three proposed estimators, we focus for the remainder of the analyses on the TWFE OLS estimates.

Column (1) of Table 4 reports estimates of Equation 4 for the main estimation sample of 16-year-olds but using only the limited gender and race/ethnicity controls (for comparability). Column (2) displays the estimates for the sample of 17-year-olds. These results show that the effects of GDL laws persist for at least one year after a teen has first ex-

Table 4: Effects of Minimum Unrestricted Driving Age on Long-Run Educational Attainment

	CPS Sample		ACS Sample			
	Not In School = 1		Max Grade		Did Not Complete	
	At Age 16	At Age 17	≤ 10	≤ 11	HS or GED	HS
	(1)	(2)	(3)	(4)	(5)	(6)
Min. Unres. Driving Age >16 (β_1)	0.0016	0.0063	-0.0008	-0.0006	0.0006	0.0006
	(0.0041)	(0.0056)	(0.0014)	(0.0017)	(0.0017)	(0.0027)
School-Leaving Age Allows Dropout (β_2)	0.0203***	0.0107**	0.0048***	0.0058***	0.0069***	0.0106**
	(0.0049)	(0.0052)	(0.0016)	(0.0019)	(0.0024)	(0.0043)
Min. Unres. Driving Age >16	-0.0116**	-0.0160**	-0.0028	-0.0040*	-0.0041*	-0.0064**
$\times$ School-Leaving Age Allows Dropout (β_3)	(0.0046)	(0.0068)	(0.0018)	(0.0021)	(0.0022)	(0.0028)
Mean of Outcome	3.8%	6.2%	4.4%	7.1%	8.7%	13.3%
Obs	75,196	73,187	3,264,783	3,264,783	3,264,783	3,264,783

Columns 1 and 2 represent OLS estimates using CPS ASEC data from 1990–2017 and include state and year fixed effects and indicators for gender and race/ethnicity. The ACS Sample uses single-year ACS data from 2008–2019 for 22–34 year olds and excludes those not born in the United States. Columns 3–6 are OLS estimates using the ACS Sample and include state-by-age and sample-year-by-age fixed effects and indicators for gender and race/ethnicity. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

perienced the driving restriction. GDL laws lead to a 1.6pp decline in the probability of dropout for 17-year-olds, which at the mean represents a 26% reduction. These results are similar to the effects on 16-year-olds, suggesting that restricting access to driving at age 16 may encourage teens to not only postpone dropping out, but to eventually complete their high school diploma.

To test this potential for long-run impacts of GDL restrictions, we next turn to the 2008–2019 American Community Survey (ACS) for data on educational attainment among US-born respondents aged 22 to 34.[33] Because these data pool people of different ages, we assess educational attainment, y_{isca}, of person i in state s in birth cohort c of age a using:

$$y_{isca} = \beta_1 GDL_{sc} + \beta_2 CS_{sc} + \beta_3 GDL_{sc} * CS_{sc} + X_i'\nu + D_{sa} + D_{ca} + \epsilon_{isca}. \qquad (5)$$

We define treatment based on state of birth and cohort, where GDL_{sc} indicates if the minimum unrestricted driving age in state s for cohort c at age 16 was > 16 and CS_{sc} indicates if those under the age of 18 can legally leave school. State-by-age fixed effects, D_{sa}, control for age-specific outcomes that systematically vary by state, and cohort-by-

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[33]In 2008, the ACS began to differentiate GED from regular high school diplomas. The age window of 22 to 34 maximizes overlap with the CPS sample, and we omit those 21 and under to limit measurement error from those still completing secondary education.

age fixed effects, D_{ca}, control for general age-specific trends or cohort trajectories.[34] We restrict the sample to those living in their state of birth at the time of survey in order to reduce measurement error in treatment.

Columns (3)–(6) of Table 4 reveal that GDL laws improve several measures of long-run educational attainment. We find no significant evidence that adults who experienced a GDL law at age 16 are more or less likely to have completed less than a 10th-grade education. However, they are 0.4pp less likely to have completed only an 11th-grade education (or less). We find a similar-sized effect on the probability of high school completion inclusive of General Educational Development (GED) equivalency credentials. Teens who experience a GDL restriction are 0.4pp less likely to have no form of high school diploma by age 22-34. And if we exclude GED credentials, the estimates in column (6) show that adults who experienced a GDL law restricting access to driving at age 16 are 0.6pp more likely to have obtained a traditional high school diploma by the age of 22–34.[35]

We conclude that, by increasing high school attainment, GDL laws likely created significant impacts on the affected cohorts' career trajectories. Previous work has shown that an additional year of high school attainment increases college attendance, decreases the likelihood of unemployment, and increases average earnings (Oreopoulos 2009). These long-run findings are all the more striking because of the potential for attenuation bias resulting from measurement error in our assignment of treatment (stemming from the fact that we do not observe what state an individual resided in at age 16).

4.3   Heterogeneity Analysis

We return to the CPS sample of 16-year-olds and main specification (Equation 1) to explore heterogeneity across sub-populations. Estimates across eight different sub-populations are shown in Table 5, which also reports mean outcome values for each subgroup. For reference, column (1) shows the estimates for the full sample of 16-year-olds (replicating column (2) of Table 2). Columns (2) and (3) of Table 5 report the effects of GDL laws on dropout behavior separately for male and female teens. While the point estimate of β_3 appears slightly larger for men, a Wald test reveals that the estimates are not statistically different from one another.

We next examine heterogeneity by race and household income. Heterogeneity in ef-

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[34]It is also possible to use sample year and age to index Equation 5; this yields identical estimates of β.
[35]Individuals who complete a traditional high school diploma achieve significantly higher earnings, on average, than GED certificate holders (Ewert 2012).

fects 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 household may be less able to 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.

Columns (4) and (5) of Table 5 report the 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 close to zero. These results may reflect greater access to vehicles related to wealth or household income.

In columns (6) and (7) of Table 5, we split the sample into two halves based on real 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 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. This may explain the noisier and statistically insignificant estimate for this sub-group. An alternative explanation is greater variation in vehicle availability for teens in lower-income households. Note, however, that the difference in the estimates across the lower-income and higher-income groups is not statistically significant.

Table 5: The Effect of Minimum Unrestricted Driving Age on 16-Year-Old 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 (β_1)	0.0009	0.0031	-0.0010	0.0047	-0.0079	-0.0024	0.0049	0.0060	-0.0178***
	(0.0038)	(0.0053)	(0.0042)	(0.0044)	(0.0074)	(0.0027)	(0.0061)	(0.0039)	(0.0062)
School-Leaving Age $\leq$ 16 (β_2)	0.0195***	0.0228***	0.0164***	0.0198***	0.0226**	0.0182***	0.0231**	0.0181***	0.0262***
	(0.0047)	(0.0065)	(0.0056)	(0.0061)	(0.0095)	(0.0052)	(0.0099)	(0.0051)	(0.0091)
Min. Unres. Driving Age >16 $\times$ School-Leaving Age $\leq$ 16 (β_3)	-0.0110**	-0.0131**	-0.0094*	-0.0150***	-0.0007	-0.0090**	-0.0140	-0.0127**	-0.0059
	(0.0047)	(0.0065)	(0.0050)	(0.0055)	(0.0112)	(0.0034)	(0.0087)	(0.0048)	(0.0127)
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	52,727	22,469	37,598	37,598	59,227	15,969

TWFE OLS estimates 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; NPND laws; and state log real effective minimum wage. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

Finally, columns (8) and (9) of Table 5 show the effects of GDL laws estimated separately for teens living in urban and non-urban areas. These estimates reveal that the negative impact of GDL laws on high school dropout is largely driven by 16-year-olds residing in non-urban locations.[36] This may reflect a greater importance of car culture in non-urban settings. Overall, it appears that the positive unintended consequence of GDL laws decreasing high school dropout rates is driven by teens who would traditionally be categorized as lower-risk: non-URM, above median income, non-urban students.

5   Mechanism Analysis

We next investigate potential mechanisms to better understand why increasing the minimum driving age reduces the probability of high school dropout. 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/or leisure activities. We further tease this apart using variation in the intensity of GDL laws adopted across different states. We then investigate whether the indirect effects may be driven by reduced access to job opportunities under GDL laws by studying effects on teen employment.

5.1   Variation in GDL Intensity

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. The 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 dropout decisions. Because the intermediate license primarily restricts nighttime driving and carpooling, this type of GDL restriction should be less likely to hinder the teen's ability to commute to school. On the other hand, when a teen

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[36]The statistically significant estimate for the Placebo group (β_1) in urban areas indicates that the timing of GDL law adoption may have been correlated with a downward trend in urban dropout rates. However, those locales make up a small fraction of our overall sample and our main results are clearly robust to dropping them, as shown in column (8) of Table 5.

has no access to driving, we expect to see both an indirect channel from reduced access to labor and leisure activities and 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:

$$
\begin{aligned}
NotInSchool_{ist} = {} & \beta_1^A IntLicense_{st} + \beta_1^B NoLicense_{st} + \beta_2 CS_{st} \\
& + \beta_3^A IntLicense_{st} * CS_{st} + \beta_3^B NoLicense_{st} * CS_{st} \\
& + X_i'\nu + Z_{st}'\mu + D_s + D_t + \epsilon_{ist}.
\end{aligned} \tag{6}
$$

This specification is similar 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 have access to unrestricted, full-privilege licenses. Estimates from this expanded model are shown in Table 6.

Estimates for the Placebo group in the expanded model (β_1^A and β_1^B) are once again small and statistically insignificant under both levels of GDL restrictions. The reinforces our finding that GDL restrictions have no impact on high school dropout behavior when compulsory schooling laws are binding. The estimate of β_3^A indicates that the effect of having access to an intermediate license only reduces the probability of high school dropout by 1.2pp. Because the intermediate license is unlikely to hinder access to school, this negative effect represents only indirect channels. In other words, the reduction in access to labor and/or leisure activities caused by limiting 16-year-old driving leads to a 32% reduction in the probability of high school dropout among this age group.[37]

The estimate of β_3^B indicates that the effect of having no access to driving for 16-year-olds in states where dropout is legally permitted is very small and statistically insignif-

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[37]Many states allow targeted exemptions of the nighttime driving restrictions for intermediate license holders who are traveling to a place of work. Separately estimating the effect of driving restrictions among those states with the work exemptions yields a nearly identical point estimate for β_3^A of -0.0120 ($p = 0.018$). This suggests that a reduction in access to leisure activities, rather than employment, may be the primary source of the decrease in teen dropout behavior. Data on exemptions come from Argys, Mroz, and Pitts (2019); full results available upon request.

Table 6: Effects of Different Levels of Mobility Restrictions on 16-Year-Old Dropout

	Not In School = 1	
	(1)	(2)
GDL at 16:		
Intermediate License Only (β_1^A)	0.0035	0.0024
	(0.0043)	(0.0039)
No License (β_1^B)	0.0017	0.0007
	(0.0060)	(0.0054)
School-Leaving Age ≤ 16 (β_2)	0.0197***	0.0188***
	(0.0049)	(0.0047)
GDL at $16 \times$ School-Leaving Age ≤ 16:		
Intermediate License Only (β_3^A)	-0.0131***	-0.0122**
	(0.0044)	(0.0047)
No License (β_3^B)	-0.0008	-0.0015
	(0.0052)	(0.0055)
Additional Effect of No License if	0.0123***	0.0108**
School-Leaving Age ≤ 16 ($\beta_3^B - \beta_3^A$)	(0.0037)	(0.0041)
Controls	-	Y
Obs	75,196	75,196

TWFE OLS estimates 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; NPND laws; and state log real effective minimum wage. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

icant.[38] 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 net effect estimates, (β_3^B - β_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 1.08pp *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

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[38]Note that only twelve states ever fully restricted access to driving for 16-year-olds during the time period under study. Thus, estimation of β_1^B and β_3^B relies on a relatively small number of observations.

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 6 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.

5.2 Teen Labor Force Participation

We next study the effect of GDL laws on teen labor force participation. This analysis provides insight as to whether the findings on high school dropouts are attributable, at least in part, to reduced access to job opportunities under GDL laws. We replace the dependent variable in the triple-difference model with several measures of labor market participation available in the CPS survey:

$$Work_{ist} = \alpha_1 GDL_{st} + \alpha_2 CS_{st} + \alpha_3 GDL_{st} * CS_{st} + X_i' \nu + Z_{st}' \mu + D_s + D_t + \epsilon_{ist}. \quad (7)$$

All right-hand side variable definitions are as in Equation 1.

Considering employment outcomes alters the interpretation of coefficients in Equation 7, as compulsory schooling laws no longer define an unaffected Placebo group. Teens can alter their labor force participation in response to GDL laws, regardless of the state's compulsory schooling regime. However, the triple-difference model is still useful as a tool to tease apart the direct and indirect mechanisms at play.
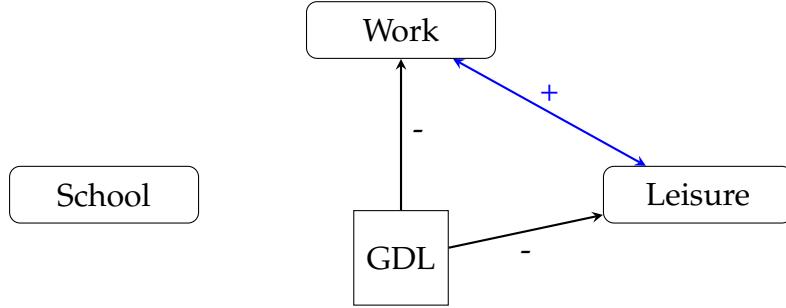
In Figure 4, we illustrate the potential direct and indirect channels through which GDL laws might impact teen labor force participation. In panel (a), we consider the case where a teen resides in a state with compulsory schooling laws that do not permit dropping out at age 16. In this case, the restriction 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 other activities, which we denote leisure. This indirect effect will have a positive effect on employment (assuming that labor and leisure are substitutes).[39] 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 α_1 in Equation 7 then captures the sum of the direct effect and the indirect effect from leisure when CS laws

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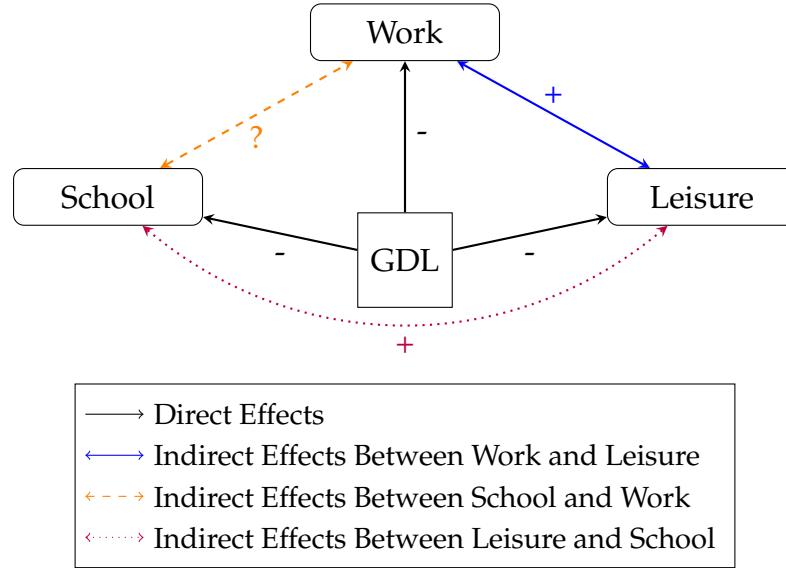
[39]Although Figure 4 implicitly assumes that work and leisure (and school and leisure) are substitutes, Equation 7 does not impose substitutability.

Figure 4: Direct and Indirect Effects of GDL Laws on Teen Work

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

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

prohibit 16-year-old dropout.

Panel (b) of Figure 4 illustrates 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. 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, work and school are complements, then the indirect effect from the school channel contributes *negatively* to labor force participation.[40] In Equation 7, α_3 captures this additional indirect channel between

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[40]Of course, teens also may choose to alter their schooling decisions in response to reduced access to

work and school. Another quantity of interest in this model is the sum of coefficients, $\alpha_1 + \alpha_3$, which will now capture the full reduced-form impact of GDL laws on teen work when CS laws are non-binding (i.e. the sum of all arrows in panel (b) of Figure 4).

Table 7: Effects of Minimum Unrestricted Driving Age on Teen Labor Market Outcomes

		Usually & Seeking	
	LFP = 1 (1)	Full Time (2)	Part Time (3)
Min. Unres. Driving Age >16 (α_1)	-0.0046 (0.0127)	-0.0057** (0.0024)	0.0012 (0.0117)
School-Leaving Age $\leq$ 16 (α_2)	0.0284* (0.0149)	0.0100*** (0.0030)	0.0183 (0.0141)
Min. Unres. Driving Age >16 $\times$ School-Leaving Age $\leq$ 16 (α_3)	-0.0181 (0.0134)	-0.0082** (0.0032)	-0.0099 (0.0131)
Total Effect of GDL if School-Leaving Age $\leq$ 16 ($\alpha_1 + \alpha_3$)	-0.0227** (0.0108)	-0.0139*** (0.0033)	-0.0087 (0.0106)
Mean of Outcome	0.24	0.02	0.23
Obs	75,196	75,196	75,196

TWFE OLS estimates using CPS ASEC data from 1990–2017. "Usually & Seeking" refers to usual work and, if unemployed, desired work. All specifications include state and year fixed effects as well as controls for: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; NPND laws; 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

Column (1) of Table 7 shows estimates of Equation 7 with an indicator for whether the 16-year-old is currently in the labor force as the outcome variable. The estimate of α_1 reveals that increasing the minimum driving age has a a very small and statistically insignificant effect on 16-year-old labor force participation when dropping out is disallowed. As discussed above, this represents the sum of the negative direct effect of GDL laws on teen labor force participation and the (likely) positive indirect effect stemming from reduced access to leisure activities. We can therefore interpret this null finding as an indication either that neither of these two effects is very large, or that they are approximately equal in magnitude (and opposite in sign).

The estimate of α_3 is much larger and negative, but not statistically significant (*p-*

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leisure activities. If school and leisure are substitutes for teens, then schoolgoing would increase as denoted by the red dotted line in panel (b) of Figure 4.

value = 0.18). The negative sign may suggest that allowing for the additional channel of high school dropout creates a negative indirect effect on teen labor force participation. However, this result is imprecise. The total effect of GDL laws when all direct and indirect channels are incorporated (measured by the sum $\alpha_1 + \alpha_3$) is a 2.3pp decrease in teen labor force participation, equivalent to a 9% reduction at the mean. Thus, teens respond to GDL-based mobility restrictions by reducing participation in the labor market.

Columns (2)–(3) of Table 7 differentiate effects on full- and part-time labor force participation. In column (2), we replace the dependent variable in Equation 7 with an indicator that the teen is either employed full-time, is seeking full-time employment, or is usually employed full-time. Notably, there is a significant negative effect of GDL laws on full-time labor force participation even when dropout is not permissible. This strongly suggests a negative direct effect of GDL restrictions on teens' ability to engage in full-time work. The additional effect (α_3) of GDL mobility restrictions on full-time work when school dropout is permitted is also negative and statistically significant, indicating that there is an indirect channel linking teens' decisions regarding schooling and work when they are faced with mobility restrictions. However, it is important to note that this reduction in full-time work is not necessarily driven by teens who would have dropped out of school in the absence of GDL laws. More than half (56%) of full-time teen workers in the CPS sample also report attending school.

Column (3) replaces the dependent variable in Equation 7 with an indicator that the teen is either employed part-time, is seeking part-time employment, or is usually employed part-time. This results are all small and statistically significant, revealing no evidence of an impact of GDL laws on teen part-time labor force participation. However, this could obscure a situation in which GDL laws lead some teens to flow from full-time to part-time work and other teens who were previously engaged in part-time work to exit the labor force.

Many states that adopted a GDL law include specific exemptions that allow teens to drive at night if commuting to or from work. Such exemptions could lead to smaller or even null effects of GDL laws on teen labor force participation. However, estimates from a specification that reflects variation in the type of GDL law—replacing the binary measure of GDL restriction with two indicators, one for GDL laws with work exemptions and one for GDL laws with no exemptions—do not show meaningful differences. The total effect of GDL laws on teen labor force participation is -2.3pp ($p = 0.047$) in states with a work

exemption and -2.5pp ($p = 0.062$) in states without a work exemption.[41] Similarly, if we look at the impact on teens who report usually working or seeking full-time work, the total effect is -1.4pp in both states with a work exemption and states with no exemptions (both statistically significant at the 1% level). The small differences in these effect sizes likely reflects that, for most states, nighttime driving restrictions take effect at 11pm or later. Relatively little work activity occurs so late at night, limiting the importance of GDL work exemptions for teen labor force decisions.

Taken together, these results indicate that GDL restrictions create a negative *direct* effect on full-time labor force participation for teens.[42] If there is a similar direct effect on part-time work, it is offset by substitution away from leisure-type activities. The additional *indirect* effect of GDL laws on teen work from the schooling channel is consistently larger and negative than the direct effect on labor force participation. On net, when teens can legally drop out, they significantly reduce labor force participation, and specifically full-time labor supply, in response to 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, the estimates above and the main estimates on dropout behavior in Table 2 also incorporate the indirect channels stemming from reduced access to leisure activities. The estimated increase in schooling could represent substitution away from leisure activities. We next turn to a discrete choice model to better understand these findings.

6   Distinguishing Mechanisms with Model-Based Analysis

To capture the indirect channels linking GDL laws to teens' education, labor, and leisure decisions, we develop a multiple discrete choice model of teen behavior.[43] In the model, teens select work, school, both, or neither. Importantly, school and work can be either complements or substitutes, and the model allows us to distinguish that relationship from the potentially correlated preferences that teens have for each of these activities.

Denote work and school as A and B, respectively. Each teen i makes a discrete decision to work ($y_i^A \in \{0, 1\}$) and to attend school ($y_i^B \in \{0, 1\}$). Denote i's combined choice as

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[41]Data on exemptions come from Argys, Mroz, and Pitts (2019). Full results available upon request.

[42]Results are similar if we replace the dependent variables with indicators for employment rather than labor force participation.

[43]Our model is methodologically similar to Gentzkow (2007), but incorporates a policy evaluation framework and a non-standard normalization to enable parsing mechanisms. The model also recalls Montmarquette, Viennot-Briot, and Dagenais (2007) or a static version of Eckstein and Wolpin (1999).

(y_i^A, y_i^B), and the choice set $\mathcal{C} \equiv \{0, 1\}^2$. The indirect utility that i receives from each choice is:

$$V_i(0,0) = \tilde{\gamma}^0 GDL_{st}^0 \tag{8}$$

$$V_i(1,0) = \alpha^A + (\gamma^A + \tilde{\gamma}^0)GDL_{st}^A + x_{ist}'\lambda^A + z_{st}'\pi^A + f^A(s,\xi) + \delta_t^A + e_i^A \tag{9}$$

$$V_i(0,1) = \alpha^B + (\gamma^B + \tilde{\gamma}^0)GDL_{st}^B + x_{ist}'\lambda^B + z_{st}'\pi^B + f^B(s,\xi) + \delta_t^B + e_i^B \tag{10}$$

$$V_i(1,1) = V_i(1,0) + V_i(0,1) + \Gamma + (\gamma^\Gamma - \tilde{\gamma}^0)GDL_{st}^\Gamma. \tag{11}$$

Parameters γ^A, γ^B, and γ^Γ capture the impacts of GDL laws on the utility of each choice. Treatment (GDL) is indexed by $k_+ \in \{0, A, B, \Gamma\}$; although each teen experiences only one value of $GDL_{st}^{k_+}$ (equal to GDL_{st}), these vary when separating direct from indirect effects. The $\tilde{\gamma}^0$ term generalizes utility normalization in a way that permits decomposing effects, and Γ reflects whether work and school are complements or substitutes.[44]

Idiosyncratic preferences $e_i^\cdot$ reflect the latent utility associated with each activity, and may be correlated. Thus, e_i^A can be interpreted as motivation to work or labor force attachment and e_i^B as motivation for school or expected returns to schooling. The model includes individual and state characteristics x_{ist}: gender, race/ethnicity indicators, mother's education, presence of father in household, receipt of SNAP benefits, and the presence of NPND laws. The vector z_{st} includes state-year characteristics used for exclusion restrictions: $z_{st}' = \left[UR_{st}, \ln(MW_{st}), CS_{st}, GDL_{st}^B \times CS_{st} \right]$, where UR_{st} is the 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 $f^\cdot(s,\xi)$ terms represent correlated random effects (discussed below) and $\delta_t^\cdot$ are year dummies. Agents choose the bundle $(y_i^A, y_i^B) \in \mathcal{C}$ that maximizes utility $V_i(y_i^A, y_i^B)$.

6.1 Identification

We make the following assumptions to identify model parameters and total effects:

Assumption 1 (Bivariate Normal Idiosyncratic Preferences)**.** *Idiosyncratic preferences are*

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[44]In the absence of a GDL law, it is positive if school and work are relative complements and negative if they are relative substitutes. When a GDL law is active, the complementarity of substitutability is measured by $\Gamma + (\gamma^\Gamma - \tilde{\gamma}^0)$.

independent and 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 1 imposes the structure of a multinomial probit model (e.g., Goolsbee and Petrin 2004) onto the model of multiple discreteness (Gentzkow 2007).[45] Normality is not necessary; for example, we could instead use a finite number of discrete points to approximate any bivariate distribution (Train 2008). However, the parsimony of joint normality facilitates interpretation and discussion.

We use the same policy variation as in Section 4 to identify the structural policy parameters, but must address two additional challenges. First, because Γ and ρ both reflect how often teens choose work and school together, they are not separately identified without an additional assumption. We adopt exclusion restrictions requiring that certain variables shift the utility of only one activity.[46]

Assumption 2 (Exclusion Restrictions and Relevance). *Components of z may shift the utility of at most one of A or B, and at least one component of z has a non-zero effect. Specifically,*

$$\pi^{A'} = \left[\pi_{UR}^A, \pi_{MW}^A, 0, 0 \right],$$
$$\pi^{B'} = \left[0, 0, \pi_{CS \leq 16}^B, \pi_{GDL \times CS}^B \right],$$

and $\pi^A + \pi^B \neq \mathbf{0}$.

The assumption presumes that state unemployment rate and log real minimum wage can only impact the utility of employment. There is a substantial literature suggesting that these are relevant for teen employment outcomes.[47] Assumption 2 requires that these two

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[45]In non-linear models, identifying parameters is distinct from identifying partial effects (Wooldridge 2005). Normalizing the variance is one way to ensure uniqueness of the mapping between the two. Because utility is scaleless, we set $\text{Var}(e_i^A) = 1$.

[46]Intuitively, if many teens choose activities A and B together, that could indicate high Γ, high ρ, or both. However, an exogenous shift in the value of one activity (say, A) should increase probability of choosing B only if Γ is positive. In contrast, if the probability of choosing B is unchanged by an exogenous shock in A, than the large fraction selecting both A and B reflects high ρ (see Section I.D of Gentzkow 2007).

[47]Aaronson, Park, and Sullivan (2006) show that teen labor force participation is pro-cyclical. Several studies suggest that increases in minimum wages decrease the extensive margin of teen employment (Neumark and Wascher 1992; 1995; Zavodny 2000; Sen, Rybczynski, and Van De Waal 2011), though several

factors do not directly effect the value of schoolgoing. This is akin to using these two variables as instruments for employment in a regression of schooling on employment. They are invalid if changes in current labor market conditions systematically shift changes in the utility of schooling. Conversely, we require compulsory schooling (CS_{st}) and its interaction with GDL laws to have only a direct effect on the utility of schooling, disallowing a direct effect on work.[48] This assumption requires that compulsory schooling laws impact teen employment only through their effects on schooling decisions.[49]

The second challenge to identifying policy parameters is that fixed effects create statistical and practical challenges for estimation in non-linear settings. Statistically, including fixed effects induces an incidental parameters problem.[50] An alternative is to impose a correlated random effects (CRE) structure on the model:

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 $\overline{\cdot}_s$ indicates an average across observations in state s.

Econometrically, CRE can control for much of the endogeneity between treatment and outcomes. Algebraically, CRE control for the average levels of covariates, such that the γ^{k+} reflect *changes* in GDL_{st} rather than differences in levels. Thus, CRE are similar to the within transformation used to justify fixed effects. In fact, Mundlak (1978) shows that CRE and fixed effects models are algebraically identical in linear settings.[51]

6.2 Model Estimates

Assumptions 1–3 are sufficient to identify all model parameters except $\tilde{\gamma}_0$, which we discuss more in the next section. We estimate the model using maximum simulated likeli-

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papers argue that carefully controlling for local employment conditions attenuates this effect (Allegretto, Dube, and Reich 2011; Giuliano 2013).

[48]Oreopoulos (2009) and Anderson (2014) find significant effects of compulsory schooling on teen schooling outcomes, suggesting that this instrument is relevant.

[49]Additionally, while the parameters of multinomial probit models are generally identified, identification is often weak without exclusion restrictions (Keane 1992).

[50]Unit fixed effect estimates are inconsistent with finite time periods. Because the fixed effects are not separable in the likelihood function, this inconsistency propagates to other parameters (Lancaster 2000). Practically, adding many fixed effects increases the computational cost, impeding optimization.

[51]Indeed, when we reestimate our primary model (Equation 1) using correlated random effects instead of fixed effects, estimates of partial effects are very similar. See Wooldridge (2019) for a recent review.

Table 8: Key Model Parameters

		Work			School				
ρ	σ	γ^A	π^A_{UR}	π^A_{MW}	γ^B	$\pi^B_{CS\leq16}$	$\pi^B_{CS\times GDL}$	Γ	γ^Γ
-0.4769	0.0215	-0.0265	-0.0234	-0.3650	0.0004	-0.0050	0.0031	0.0113	-0.0020
(0.0020)	(0.0188)	(0.0006)	(0.0002)	(0.0024)	(4.43e-05)	(9.67e-05)	(6.33e-05)	(0.0002)	(4.11e-05)

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 inverse Hessian, and for ρ and σ additionally employ the delta method. Observations are weighted using sample weights.

hood. Table 8 shows estimates of key model parameters.[52]

The estimate of ρ (-0.48) indicates negative correlation in the 'types' of teens that choose school or work. Those with a high (utility) value from school are more likely to have a low value from work. Conversely, those with the highest utility from work are less likely to find school valuable. However, the small, positive estimate of Γ indicates that school and work are weak complements: decreasing access to school mildly decreases the value of work (and vice versa). This is a key piece of evidence that the effects of GDL laws on schoolgoing and on labor force participation do not primarily reflect substitution between these activities, a conclusion that cannot be established from the analysis in Sections 4 and 5 alone. It also highlights the importance of identifying the negative correlation in preferences for schoolgoing and work. Failing to account for $\rho < 0$ would make working while in high school appear more deleterious for schoolgoing.[53]

The policy parameters (γ and π) are qualitatively consistent with results in Section 4. 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 stan-

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[52]Appendix D contains additional estimation details, and Table D.1 assesses model fit by comparing how often a simulated choice matches the observed choice (averaged over 100 draws of **e**). The model returns choice shares that deviate by less than 0.02pp from the observed sample. Overall, the model correctly classifies in sample 62.27% of the time. Given the large number of idiosyncratic factors that we do not observe, we believe this to be reasonable.

[53]Montmarquette, Viennot-Briot, and Dagenais (2007) also find evidence that school and work are complementary for high-achieving teens. This is supported by Ruhm (1997), who shows that part-time work has no negative effect on educational outcomes, and Light (1999), who finds that the effect of high school employment on subsequent earnings for men is small and relatively short-lived. Eckstein and Wolpin (1999) and Montmarquette, Viennot-Briot, and Dagenais (2007) find evidence of negative correlation in preferences for school and work, although the latter also shows that a preference for good grades can undo some of this relationship. Eckstein and Wolpin (1999) find a negative psychic cost for 16-year-olds participating in both school and part-time work, although this declines with age. Relative to this literature, we provide clean, separate identification of ρ and Γ, lending credibility to the narrative that teens' preferences for schoolgoing and work are negatively related but that school and work are not substitutes, at least on average.

dard deviations of idiosyncratic preference ($|\gamma^A| > |\frac{\gamma^B}{\sigma}|$). 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. Moreover, π are all significantly different from zero. This suggests that, in conjunction with Assumption 2, the z_{st} contribute identifying variation. Finally, GDL laws mildly reduce the complementarity between schoolgoing and work, by about 18% ($\approx \gamma^\Gamma/\Gamma$). This implies that restricting mobility makes it less attractive for teens to pursue both schooling and employment together.

6.3   Estimating and Decomposing Effects

We now use the model to decompose the impacts of GDL laws into direct and indirect components. We denote direct effects as θ^k_{Dir} and require the sum of direct and indirect effects to equal the total effects; precise definitions are in Appendix D.

The parameter $\tilde{\gamma}^0$ is key to the decomposition. This parameter generalizes the usual requirement to normalize the utility of one choice in discrete choice models, which is nested and occurs when $\tilde{\gamma}^0 = 0$. The value of $\tilde{\gamma}_0$ does not affect model fit or identification of parameters or total effects, but imposing $\tilde{\gamma}^0 = 0$ makes the implicit assumption that the utility of the neither-work-nor-school choice (which includes all leisure activities available to the teen) is unaffected by GDL laws. However, the literature relating changes in teen risky behaviors to laws restricting teen mobility suggests that the value of (or access to) the neither-work-nor-school option declines when driving is limited (Barua and Vidal-Fernandez 2014; Deza and Litwok 2016; Deza 2019; Huh and Reif 2021; Barua, Hoefer-Marti, and Vidal-Fernandez 2024). Accordingly, we interact an auxiliary parameter, $\tilde{\gamma}^0$, with the policy, $V_i(0,0) = \tilde{\gamma}^0 GDL^0_{st}$, to capture the impact of GDL laws on the utility of the normalized option. Then, $\tilde{\gamma}_0$—in combination with γ^A, γ^B, and γ^Γ—redistributes the utility impact of GDL laws on each activity.

Because model fit is invariant to $\tilde{\gamma}_0$, the observed choice data and prior assumptions cannot identify it. However, set identification of $\tilde{\gamma}_0$ is possible with assumptions on the sign and relative size of the direct effects of GDL laws on each activity:

Assumption 4 (Normalization). *Let $\tilde{\gamma}^0$ be such that (i) the indirect utility impacts of GDL laws on neither, work, and school are weakly negative and that (ii) the direct effect on schooling is no larger in magnitude than the direct effect on work.*

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Table 9: Decomposition of GDL Law Effects by Activity

Total effect of GDL on schoolgoing	1.07pp	
A. Upper-bound effects	Effect	% of Total
Direct	0pp	0.0%
Indirect	1.07pp	
via Work	0.16pp	14.8%
via Neither	0.91pp	85.2%
B. Lower-bound effects		
Direct	-0.92pp	-86.4%
Indirect	1.99pp	
via Work	0.44pp	41.1%
via Neither	1.55pp	145.3%

These are the simulated total, direct, and indirect effects of policy counterfactuals using parameters shown in Table 8 averaged over 100 draws of $\mathbf{e}_i$ 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.

Each restriction within Assumption 4 is independently reasonable.[54] Direct effects should be weakly negative because GDL laws do not increase access to any activity; each activity has become weakly harder to access. Moreover, 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.

The total effect corresponding to the design-based treatment effects estimated in Section 4 is presented in the top row of Table 9.[55] The model predicts that adopting a GDL law when school-leaving is legal increases the probability of being enrolled in school by 1.1pp.[56] This result is very close to point estimates in Table 2, despite the imposition of additional structure from distributional assumptions, correlated preferences, and exclusion restrictions. Panels A and B of Table 9 show the decomposition of this total effect into direct and indirect channels for extreme values of $\tilde{\gamma}_0$, bounding the size of these channels. We further separate the indirect effect into its root causes in italics: changes in the utility of work and in the utility of the neither-work-nor-school option.

Panel A of Table 9 shows the decomposition assuming the direct effect of GDL laws

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[54]This assumption is stated formally Appendix Section D.5.

[55]We report education effects here; Appendix Table D.2 includes effects for all three activities.

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

on the utility of schoolgoing is zero. Mechanically, the total effect must therefore come entirely from the indirect channel. The decomposition in this scenario reveals that only 15% of the total schooling effect of GDL law adoption is due to changes in work access. Thus, the large majority of the effect of GDL laws on schoolgoing is due to a reduction in the utility of the neither-work-nor-school option at this bound.

Panel B of Table 9 instead assumes that the direct effects of GDL laws on labor force participation and school enrollment are exactly equal. At this lower bound, the impact of GDL laws on the utility of schoolgoing generates a direct effect of -0.9pp, but this is counteracted by a large indirect effect, again predominately due to the reduction in the utility of the neither-work-nor-school option. A larger portion of the total effect is due to the indirect channel stemming from reduced access to work, but this share is still less than half (41%) of the total effect.

In summary, we conclude that any direct effects of GDL laws on schoolgoing are generally small and are greatly outweighed by indirect effects. Moreover, the indirect effects of GDL laws on schoolgoing are not generally attributable to reduced work access. The reduced access to employment created by GDL laws can only explain between 15% and 41% of the total observed increase in teen schoolgoing. Given the availability of alternative modes of travel to school, we view it as likely that the true decomposition is somewhat closer to the lower end of that spectrum. However, regardless of the scenario, the majority of the total effect of the GDL policy on schoolgoing is attributable to the reduced utility of the neither-work-nor-school (leisure) option.

These results demonstrate how the model can be used to clarify the channels by which interventions in one activity spill over onto other activities; it rationalizes unintended consequences and provides a framework to think about when they might occur. We find that GDL laws restricting access to non-school activities have large spillover effects on schoolgoing. However, because we find that teens do not treat employment as a substitute for schooling, restricting access to work is not a primary source of unintended consequences on high school dropouts. Thus, future policies that specifically target non-school, non-work activities would likely preserve the reduction in dropouts without inducing a negative effect on teen employment.

7 Conclusion

We investigate graduated driver licensing laws with a triple-difference research design to study the effects of mobility restrictions on human capital outcomes for 16-year-olds in the United States. A robust set of results indicate that GDL laws—which restrict teen automobility—actually decrease high school dropout by about 1.1–1.3pp (a 29–35% reduction from the mean). This finding is not limited to the short-run. We estimate that by ages 22–34, those affected individuals are 0.6pp more likely to have obtained a traditional high school diploma. This suggests that increasing the age at which teens can obtain a full-privilege drivers license not only causes teens to postpone dropout decisions, it can induce them to complete a high school degree.

This potentially surprising result contrasts with evidence from large, middle-income cities that transit expansions increasing school access improve educational outcomes (Dustan and Ngo 2018; Asahi and Pinto 2022; Alba-Vivar 2024). Our results instead suggest that access to other activities may have decreased even more than access to school in the U.S. 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 2.3pp (9% 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 driving safety, these laws 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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Appendices

A   Additional Figures and Tables

Figure A.1: Teen Driving Restrictions & Minimum School-Leaving Age from 1990–2017 (population weighted)

(a) Graduated Driver Licensing Adoption

(b) Minimum Legal School-Leaving Age
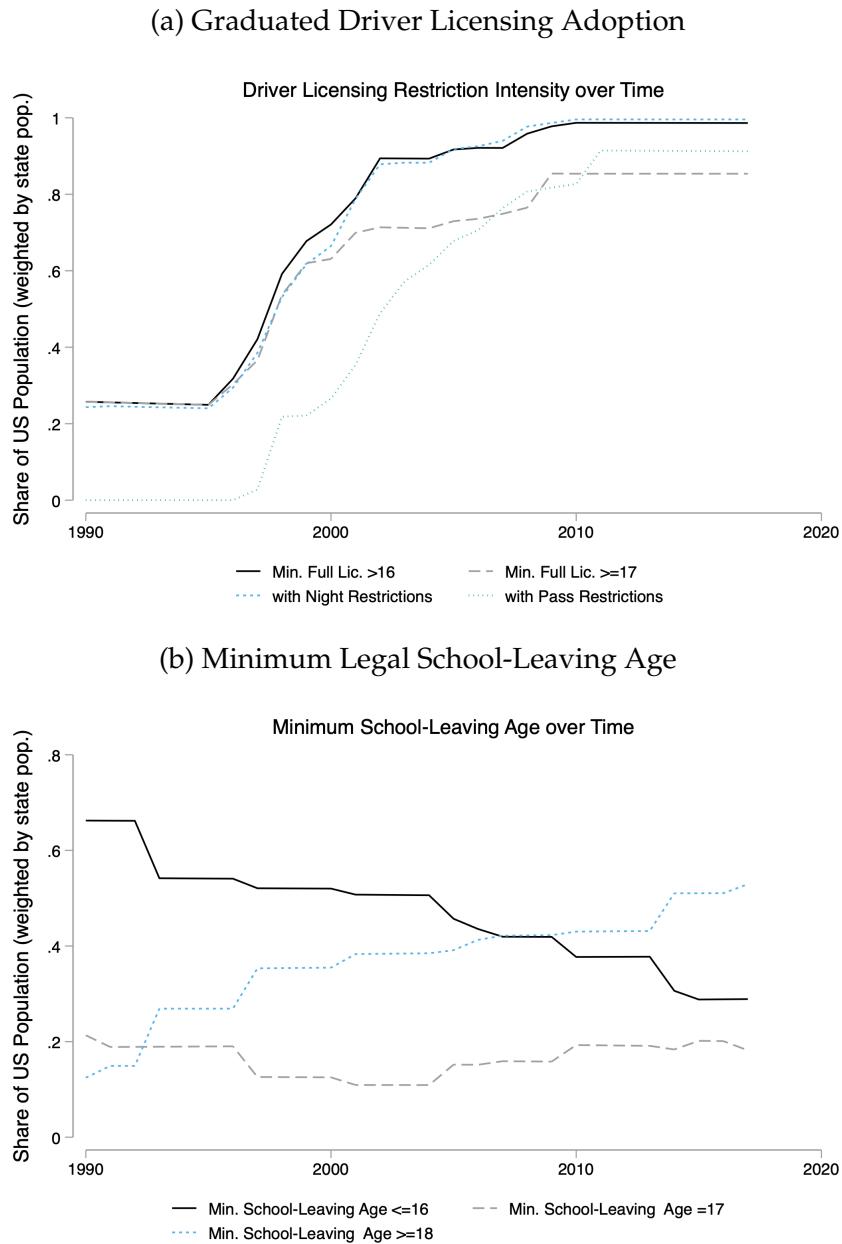
Figure A.2: Prevalence of the "Interacted" Treatment over Time

States with both GDL restrictions and less restrictive CS laws

Number of states (out of 50 + DC)

Min. Full Lic. >16

Min. School-Leaving Age <=16

Min. Full Lic. >16 & Min. School-Leaving Age <=16
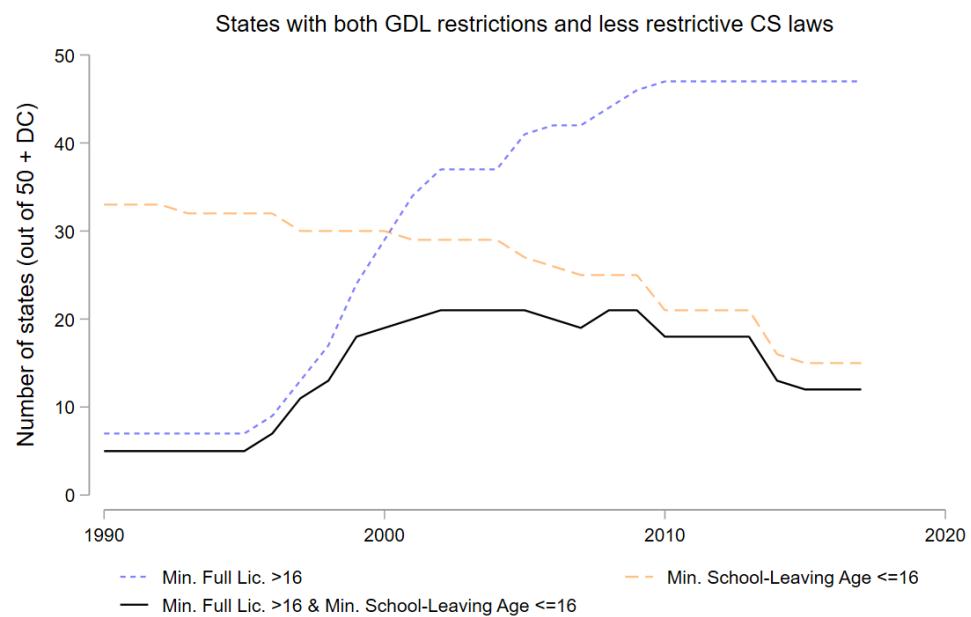
Table A.1: GDL Laws and Compulsory Schooling Laws in 1990 and Changes Thereafter (through 2017)

| State	Year	Min. Unres. Driving Age > 16	School-Leaving Age ≤ 16	State	Year	Min. Unres. Driving Age > 16	School-Leaving Age ≤ 16
Alabama	1990	0	1	Missouri	1990	0	1
Alabama	2002	1	1	Missouri	2001	1	1
Alabama	2010	1	0	Missouri	2010	1	0
Alaska	1990	0	1	Montana	1990	0	1
Alaska	2005	1	1	Nebraska	1990	0	1
Arizona	1990	0	1	Nebraska	1999	1	1
Arizona	2008	1	1	Nebraska	2005	1	0
Arkansas	1990	0	0	Nevada	1990	0	0
Arkansas	2009	1	0	Nevada	2005	1	0
California	1990	0	1	New Hampshire	1990	0	1
California	1993	0	0	New Hampshire	1998	1	1
California	1998	1	0	New Hampshire	2010	1	0
Colorado	1990	0	1	New Jersey	1990	1	1
Colorado	1999	1	1	New Mexico	1990	0	0
Colorado	2007	1	0	New Mexico	2000	1	0
Connecticut	1990	0	1	New York	1990	1	1
Connecticut	1997	1	1	North Carolina	1990	0	1
Connecticut	2001	1	0	North Carolina	1997	1	1
Delaware	1990	0	1	North Dakota	1990	0	1
Delaware	1999	1	1	Ohio	1990	0	0
District of Columbia	1990	0	0	Ohio	1999	1	0
District of Columbia	2001	1	0	Oklahoma	1990	0	0
Florida	1990	0	1	Oklahoma	2005	1	0
Florida	1996	1	1	Oregon	1990	0	0
Georgia	1990	0	1	Oregon	2000	1	0
Georgia	1997	1	1	Pennsylvania	1990	1	0
Hawaii	1990	0	0	Rhode Island	1990	0	1
Hawaii	2006	1	0	Rhode Island	1999	1	1
Idaho	1990	0	1	Rhode Island	2014	1	0
Illinois	1990	1	1	South Carolina	1990	0	0
Illinois	2005	1	0	South Carolina	2002	1	0
Indiana	1990	0	1	South Dakota	1990	0	1
Indiana	1998	1	1	South Dakota	2010	0	0
Indiana	2006	1	0	Tennessee	1990	0	0
Iowa	1990	0	1	Tennessee	2001	1	0
Iowa	1999	1	1	Texas	1990	0	0
Kansas	1990	0	1	Texas	2002	1	0
Kansas	1997	0	0	Utah	1990	0	0
Kansas	2010	1	0	Utah	1999	1	0
Kentucky	1990	0	1	Vermont	1990	0	1
Kentucky	1996	1	1	Vermont	2000	1	1
Kentucky	2014	1	0	Virginia	1990	0	0
Louisiana	1990	1	0	Virginia	1998	1	0
Maine	1990	0	0	Washington	1990	0	0
Maine	2000	1	0	Washington	2001	1	0
Maryland	1990	1	1	West Virginia	1990	0	1
Maryland	2015	1	0	West Virginia	2001	1	1
Massachusetts	1990	1	1	West Virginia	2014	1	0
Michigan	1990	0	1	Wisconsin	1990	0	0
Michigan	1997	1	1	Wisconsin	2000	1	0
Michigan	2014	1	0	Wyoming	1990	0	1
Minnesota	1990	0	1	Wyoming	2005	1	1
Minnesota	2008	1	1				
Minnesota	2014	1	0				
Mississippi	1990	0	1				
Mississippi	1997	0	0				
Mississippi	2009	1	0				

Table A.2: The Effect of Minimum Unrestricted Driving Age on 16-Year-Old Dropout, Difference-in-Differences

	Not In School = 1		
	(1)	(2)	(3)
Min. Unres. Driving Age >16	-0.0106*	-0.0088	-0.0109*
	(0.0058)	(0.0064)	(0.0057)
Estimator	TWFE	BJS	Probit
Exclude Always Treated	-	Y	-
Obs	36,147	26,431	36,147

TWFE OLS estimates, imputation estimates of Borusyak, Jaravel, and Spiess (2024), and average marginal effects from probit regression using CPS ASEC data from 1990–2017. Sample is restricted to state-years where the minimum legal dropout age is 16 or younger. All specifications include state and year fixed effects as well as controls for: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; NPND laws; state unemployment rate; and state log real effective minimum wage. Standard errors are clustered at the state level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

B 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.[57] 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 U.S. 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:

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

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 B.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 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

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

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 B.1: Effect of Minimum Driving Age on Fatal Car Accidents with 16-Year-Old 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 B.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 slight 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.[58] When taken in conjunction with our results, it appears that GDL laws did, in fact, restrict teen mobility.

---

[58]Relatedly, Severen and Van Benthem (2022) 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.

C   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 these data are aggregated 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:

$$DropoutRate_{dst} = \beta GDL_{st} + X_{dt}'\nu + Z_{st}'\mu + D_d + D_t + \epsilon_{dst}, \tag{C.1}$$

where $DropoutRate_{dst} \in [0, 1]$ is the high school dropout rate for school district d in state s in year t. Table C.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, an indicator for "No Pass, No Drive" restrictions, and 3-month average unemployment rate. The model 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 C.1 using TWFE OLS and estimate standard errors clustered at the state level.

Table C.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.

Column (1) of Table C.2 shows that a one-year increase in the minimum unrestricted driving age leads to a 0.43pp reduction in high school dropout rates. This is equivalent to a 13% 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 unrestricted driving age, and thus restricting teen mobility, is then associated with a 0.33pp reduction in the high school dropout rate (a 10% 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

Table C.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	-0.0043***						
	(0.0011)						
Min. Unres. Driving Age >16		-0.0033*	-0.0044**	-0.0025	-0.0047**	-0.0060**	-0.0056**
		(0.0018)	(0.0021)	(0.0019)	(0.0020)	(0.0026)	(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; NPND laws; and district and year fixed effects. Standard errors are clustered at the state level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

(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 0.44pp. Columns (4)-(7) show that the effects of increasing the minimum driving age to over 16 are largest for 11th-grade dropout rates (a 15% reduction from the mean). It is during 10th and 11th grade that many teenagers obtained full-privilege licenses prior to GDL laws (as teens generally turn 16 during those years). These results indicate that imposing restrictions on teen mobility leads to a sizable reduction in high school dropout rates of 10-15%.

D   Details of the Model-Based Analysis

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

D.1   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, Ω is positive definite matrix, therefore allowing for a Cholesky factorization of Ω (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, Ω^{Extended} is not generally positive definite and so Cholesky factorization of Ω^{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 Ω^{Extended}, we can instead rely just on Ω and thus the model can be estimated using a GHK (Geweke, Hajivassiliou, and Keane) simulator.[59] 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 8–11) 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

---

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

model's choice probabilities can be expressed in the following form:

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

where η_k and $\eta_{k'}$ are random variables distributed i.i.d. standard normal and κ are constants that potentially depend upon realizations of η. 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 Ω 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 η_1 and η_2 represent i.i.d. standard normal variables. Rewrite the model in light of this equivalence in distribution (suppressing notation denoting individual i):

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

Straightforward substitution of data and coefficients for V_1, V_2, and Γ show equivalence to the primary model (Equations 8–11). Specifically, if $V_1 = \alpha^A + \gamma^A GDL_{st}^A + x'_{ist}\lambda^A + z'_{st}\pi^A + f^A(s, \xi) + \delta_t^A$, $V_2 = \alpha^B + \gamma^B GDL_{st}^B + x'_{ist}\lambda^B + z'_{st}\pi^B + f^B(s, \xi) + \delta_t^B$, and $\Gamma_{12} = \Gamma + \gamma^\Gamma GDL_{st}^\Gamma$, 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 η_1 and η_2 variables. We show this sequentially for each choice in the choice set. First, the probability of choosing

neither activity is:

$$\Pr(\emptyset) = \Pr\left(V_1 + \eta_1 < 0 \cap V_2 + c\eta_1 + d\eta_2 < 0 \cap V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 < 0\right)$$

$$= \Pr(\eta_1 < -V_1) \cdot \Pr\left(V_2 + c\eta_1 + d\eta_2 < 0 \cap V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 < 0 \mid \eta_1 < -V_1\right)$$

$$= \Pr(\eta_1 < -V_1) \cdot \Pr\left(\eta_2 < \frac{\min\{0, -(V_1 + \Gamma + \eta_1)\} - V_2 - c\eta_1}{d} \mid \eta_1 < -V_1\right)$$

$$= \Phi(-V_1) \int_{-\infty}^{-V_1} \Phi\left(\frac{\min\{0, -(V_1 + \Gamma + \eta_1)\} - V_2 - c\eta_1}{d}\right) \phi(\eta_1) d\eta_1,$$

where ϕ and Φ 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:

$$\Pr(A) = \Pr\left(0 < V_1 + \eta_1 \cap V_2 + c\eta_1 + d\eta_2 < V_1 + \eta_1 \cap V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 < V_1 + \eta_1\right)$$

$$= \Pr(\eta_1 > -V_1)$$

$$\cdot \Pr\left(V_2 + c\eta_1 + d\eta_2 < V_1 + \eta_1 \cap V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 < V_1 + \eta_1 \mid \eta_1 > -V_1\right)$$

$$= \Pr(\eta_1 > -V_1) \cdot \Pr\left(\eta_2 < \frac{\min\{V_1 + \eta_1, -\Gamma\} - V_2 - c\eta_1}{d} \mid \eta_1 > -V_1\right)$$

$$= (1 - \Phi(-V_1)) \int_{-V_1}^{\infty} \Phi\left(\frac{\min\{V_1 + \eta_1, -\Gamma\} - V_2 - c\eta_1}{d}\right) \phi(\eta_1) d\eta_1.$$

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

$$\Pr(B) = \Pr\left(0 < V_2 + c\eta_1 + d\eta_2 \cap V_1 + \eta_1 < V_2 + c\eta_1 + d\eta_2\right.$$

$$\left. \cap V_1 + V_2 + \Gamma + (1+c)\eta_1 + d\eta_2 < V_2 + c\eta_1 + d\eta_2\right)$$

$$= \Pr\left(0 < V_2 + c\eta_1 + d\eta_2 \cap V_1 + \eta_1 < V_2 + c\eta_1 + d\eta_2 \cap V_1 + \Gamma + \eta_1 < 0\right)$$

$$= \Pr(\eta_1 < -V_1 - \Gamma) \cdot \Pr\left(0 < V_2 + c\eta_1 + d\eta_2 \cap V_1 + \eta_1 < V_2 + c\eta_1 + d\eta_2 \mid \eta_1 < -V_1 - \Gamma\right)$$

$$= \Pr(\eta_1 < -V_1 - \Gamma) \cdot \Pr\left(\eta_2 > \frac{\max\{0, V_1 + \eta_1\} - V_2 - c\eta_1}{d} \mid \eta_1 < -V_1 - \Gamma\right)$$

$$= \Phi(-V_1 - \Gamma) \int_{-\infty}^{-V_1-\Gamma} \left(1 - \Phi\left(\frac{\max\{0, V_1 + \eta_1\} - V_2 - c\eta_1}{d}\right)\right) \phi(\eta_1) d\eta_1.$$

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

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

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. □

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 (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.

D.2 Estimation Details

The likelihood function for individual i, given that i chooses $c \in \mathcal{C}$ is:

$$P_i^c(w_i; \vartheta, \sigma, \rho) = \int \mathbb{1}\left[V_i(c, w_i; \vartheta, \mathbf{e}) \geq V_i(c', w_i; \vartheta, \mathbf{e}), \forall c' \in \mathcal{C}\right] f(\mathbf{e}; \sigma, \rho)d\mathbf{e},$$

where w_i collects data for i, ϑ collects all model parameters except σ, ρ, and $\tilde{\gamma}_0$, and $f(\mathbf{e}; \sigma, \rho)$ is the pdf of the bivariate random variable distribution $N(0, \Omega)$ evaluated at $\mathbf{e}$. Joint normality and multiple discreteness imply that choosing c does not generally correspond to a rectangular subset of $\mathbf{e}$ and so analytic or fast computational functions are unavailable to calculate $P_i^c(w_i; \vartheta, \sigma, \rho)$. We therefore approximate this term by simulating the likelihood as

$$\hat{P}_i^c(w_i; \vartheta, \sigma, \rho) = \frac{1}{R} \sum_r \mathbb{1}\left[V_i(c, w_i; \vartheta, \mathbf{e}_r) \geq V_i(c', w_i; \vartheta, \mathbf{e}_r), \forall c' \in \mathcal{C}\right],$$

where $\mathbf{e}_r$ is one of R draws for each i from $f(\mathbf{e}; \sigma, \rho)$.

We estimate the model using maximum simulated likelihood to recover all parameters except $\tilde{\gamma}_0$ (these parameters are sufficient to estimate total effects and to determine $\mathcal{G}$ given Assumption 4). Specifically, we select

$$\vartheta^*, \sigma^*, \rho^* = \arg\max \sum_i \omega_i \ln \hat{P}_i^c(w_i; \vartheta, \sigma, \rho),$$

where ω_i are sample weights, via a multistep procedure the prioritizes finding σ, ρ, Γ, γ^{k+}, α^k, and π^k first to limit dimensionality.

To simulate η_1, we use Halton draws. We estimate the model by maximum simulated likelihood in several steps, using Julia and the Optim package (Mogensen and Riseth 2018). Because ρ and σ cannot take on all real values, we transform them as:

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

To facilitate estimation, we sometimes estimate a **simple model** which consists just of those parameters shown in Table 8 as well as α^A and α^B. We also sometimes use a 20% sample of our data, which we term **small data**.

Our optimization procedure consists of several steps:

1. Grid search over $\{\rho, \sigma\} \in [-1, 1] \times \mathbb{R}_{++}$ using the **simple model** with $R = 100$

conditional on each $\{\rho, \sigma\}$, **small data**, and a Newton Trust Region algorithm. Select $\{\rho, \sigma\}$ that are local minima and do not lead to the other parameter values diverging.

2. Using each likelihood-minimizing $\{\tilde{\rho}, \tilde{\sigma}\}$ from the grid search as starting values, estimate the **simple model** including $\{\tilde{\rho}, \tilde{\sigma}\}$ with $R = 100$ and $R = 400$, **small data**, and a Newton Trust Region algorithm.

3. Set starting values for parameters in the **full model** that have a corresponding parameter in the **simple model** to the minimizing value from the prior step, and set all other starting values to zero. Maximize the simulated likelihood of the **full model** with $R = 100$, using **small data**, and the L-BFGS optimization routine, until a convergence tolerance of 1e-4.

4. Set starting values as the minimizer from the prior step. Maximize the simulated likelihood of the **full model** with $R = 100$, using **all data**, and the L-BFGS optimization routine, until a convergence tolerance of 1e-8.

5. Set starting values as the minimizer from the prior step. Maximize the simulated likelihood of the **full model** with $R = 250$, using **all data**, and the L-BFGS optimization routine, until a convergence tolerance of 1e-8.

6. Examine the Hessian of our model to ensure positive definiteness. To do so, we take three numerical approximations of the Hessian, invert each, take the diagonal of each, and take the element-wise maximum of these three vectors. This vector is not strictly positive, so we return to minimization.

7. Set starting values as the minimizer from Step 5. Maximize the simulated likelihood of the **full model** with $R = 250$, using **all data**, and the L-BFGS optimization routine, until a convergence tolerance of 1e-10.

8. Examine the Hessian of our model to ensure positive definiteness. To do so, we take three numerical approximations of the Hessian, invert each, take the diagonal of each, and take the element-wise maximum of these three vectors. This vector is strictly positive, so we take the element-wise square root of the vector, and treat that as the standard error.

Steps 1 and 2 focus on the **simple model** to help ensure that we are finding a feasible global maximum in our key structural parameters, and not being captured by other local

maxima or selecting initial values that lead to diverging parameter values (e.g., $\sigma \to \infty$). Newton-type algorithms are well suited to this smaller parameter space where the likelihood is locally concave. Step 3 is meant to quickly get the full parameter vector to reasonable starting values, hence the loose tolerance. Throughout Steps 3–5, we use an L-BFGS algorithm because it is better suited to higher dimensions. Step 4 introduces all the data, and Step 5 increases the number of simulations. Step 6 is a check on convergence, which fails. Step 7 thus searches to a higher precision, which Step 8 checks convergence for and returns standard errors.

D.3   Model Fit

Table D.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 D.1, the model correctly classifies those in the sample 62.23% of the time. Given the large number of individual characteristics that we do not observe, we believe this to be a reasonable approximation.

Table D.1: Model Fit

	Totals	True $\mathcal{P}^{(0,0)}$	True $\mathcal{P}^{(1,0)}$	True $\mathcal{P}^{(0,1)}$	True $\mathcal{P}^{(1,1)}$
Model $\mathcal{P}^{(0,0)}$	2.472%	0.083%	0.040%	1.908%	0.441%
Model $\mathcal{P}^{(1,0)}$	1.315%	0.036%	0.022%	0.943%	0.315%
Model $\mathcal{P}^{(0,1)}$	74.252%	1.877%	0.972%	56.188%	15.215%
Model $\mathcal{P}^{(1,1)}$	21.960%	0.458%	0.295%	15.232%	5.975%

The totals row (top): 2.454%, 1.329%, 74.271%, 21.946% for True $\mathcal{P}^{(0,0)}$, True $\mathcal{P}^{(1,0)}$, True $\mathcal{P}^{(0,1)}$, True $\mathcal{P}^{(1,1)}$ respectively.

This table shows the shares of each observed and simulated outcome of the model using parameters shown in Table 8 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.

D.4 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:

$$\mathcal{P}^c(GDL_{st}^0, GDL_{st}^A, GDL_{st}^B, GDL_{st}^\Gamma, \tilde{\gamma}^0) =$$
$$n^{-1} \sum_i \mathbb{E}_e 1[V_i(c) \geq V_i(c')|GDL_{st}^0, GDL_{st}^A, GDL_{st}^B, GDL_{st}^\Gamma, \tilde{\gamma}^0],$$

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 $GDL_{st}^k = 1$ compared to when $GDL_{st}^k = 0$, $\forall k, s, t$:

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

The total effect is invariant to the value $\tilde{\gamma}^0$, so $\theta_{\text{Tot}}^k = \theta_{\text{Tot}}^k(\tilde{\gamma}^0), \forall \tilde{\gamma}^0$, 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^Γ on working,

or GDL^0, GDL^A and GDL^Γ on schoolgoing.

We define these effects in a consistent manner that additively decomposes the total effects into the two types of channels.[60] Specifically:

Neither activity effects

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

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

Employment effects

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

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

Schooling effects

$$\theta_{\text{Dir}}^{B} = \mathcal{Q}^{B}(0,0,1,0,\tilde{\gamma}^0) - \mathcal{Q}^{B}(0,0,0,0,\tilde{\gamma}^0) \quad \text{Direct effect on schoolgoing}$$

$$\theta_{\text{Ind}}^{B} = \mathcal{Q}^{B}(1,1,1,1,\tilde{\gamma}^0) - \mathcal{Q}^{B}(0,0,1,0,\tilde{\gamma}^0) \quad \text{Indirect effect on schoolgoing}$$

Table 9 and Appendix Table D.2 include 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).

D.5   Set Identification with Assumption 4

The following provides a precise definition of the assumptions that set identify the auxiliary parameter, $\tilde{\gamma}_0$. The two assumptions do not explicitly give a range of valid values for $\tilde{\gamma}_0$, but instead define that range implicitly. In the definition below, we write the direct and indirect effects above so as to take the auxiliary parameter as an argument.

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

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

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

D.6 Additional Model Results

The total effect corresponding to the design-based treatment effects estimated in Section 4 is presented in the top row of Table D.2 (this is an expanded version of Table 9 in the main text, which reports the results for the School activity alone). The model predicts that adopting a GDL law when school-leaving is legal increases the probability of being enrolled in school by 1.07pp and decreases the probability of labor force participation by 0.81pp.[61] These results are roughly in line with those in prior sections, though the magnitudes differ a bit for the employment results. 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 -0.86pp, or about 36% from the mean.[62] We interpret this neither option as reflecting teen preferences for all forms of leisure activity, which encompass both risky behaviors and 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 driving on risky behaviors (Deza and Litwok 2016; Deza 2019; Huh and Reif 2021).

Table D.2 shows the decomposition of each total effect into direct and indirect channels for $\tilde{\gamma}_0$ at the upper and lower bounds of $\mathcal{G}$ (see Assumption 4). It also separates the indirect effects for work and school into their root causes in italics.

Panel A of Table 9 shows the decomposition assuming $\tilde{\gamma}^0 = \sup \mathcal{G} = -0.00356$. The effects of GDL laws on schoolgoing are discussed in the main text. In contrast to the schooling results, the total effect of GDL laws on teen employment is entirely attributable to a direct effect, with only a small countervailing indirect effect 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 9 instead assumes $\tilde{\gamma}^0 = \inf \mathcal{G} = -0.00554$. The decomposition of the total effect of GDL laws on teen employment is similar to that in Panel A, with only a slightly larger indirect effect due to schoolgoing. The direct effect on neither-work-nor-school is quite large in this scenario, and is somewhat counteracted by indirect effects due to smaller impacts on the utilities of work and school.

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

[62]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 D.2: 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	-0.86pp		-0.81pp		1.07pp	
A. Upper-bound renormalization $\tilde{\gamma}^0 = \min\{0, -\gamma^A, -(\gamma^B + \pi^B_{CS \times GDL})\}.$						
Direct	-0.91pp	106.2%	-0.87pp	106.5%	0pp	0.0%
Indirect	0.05pp		0.05pp		1.07pp	
via Neither	-		0.01pp	-1.1%	0.91pp	85.2%
via Other activity	0.05pp	-6.2%	0.04pp	-5.4%	0.16pp	14.8%
B. Lower-bound renormalization $\tilde{\gamma}^0 : \theta^A_{\text{Dir}} = \theta^B_{\text{Dir}}.$						
Direct	-1.30pp	151.6%	-0.92pp	113.4%	-0.92pp	-86.4%
Indirect	0.44pp		0.11pp		1.99pp	
via Neither	-		0.01pp	-1.5%	1.55pp	145.3%
via Other activity	0.44pp	-51.6%	0.10pp	-12.0%	0.44pp	41.1%

These are the simulated total, direct, and indirect effects of policy counterfactuals using parameters shown in Table 8 averaged over 100 draws of $\mathbf{e}_i$ 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.