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

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Abstract

We provide evidence that graduated driver licensing (GDL) laws, originally intended to improve public safety, impact human capital accumulation. Many teens use automobiles to access both school and employment. Because school and work decisions are interrelated, the effects of automobile-specific mobility restrictions are ambiguous. Restricting teen mobility significantly increases short-run schoolgoing and long-run educational attainment while reducing teen employment. We develop a multiple discrete choice model that rationalizes unintended consequences and reveals that school and work are weak complements. Thus, improved educational outcomes reflect decreased access to leisure activities rather than reduced labor market access.

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

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

1   Introduction

Teenagers make decisions regarding human capital accumulation that can permanently alter their lifetime economic trajectories. Of particular import is the decision of whether or not to complete high school. Additional years of high school education have been shown to increase adult earnings and lifetime wealth (Angrist and Krueger 1991; Oreopoulos 2007), as well as reduce 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.

In this paper, we study a policy that was targeted at improving teen car safety and show that it 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. We combine quasi-experimental variation in the timing of GDL laws with cross-state variation in compulsory schooling laws to identify the effects of teen driving restrictions on high school retention and teen employment.

Our primary 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 with the effect of GDL laws in states where compulsory schooling laws prohibit 16-year-olds from dropping out.[1] This latter group of states provide a plausible placebo test of the GDL effect on educational attainment. Using microdata from the Current Population Survey's Annual Social and Economic Supplement, estimates from an interacted difference-in-differences model show that GDL laws decrease the probability of high school dropout at age 16 by 1.15pp (a 30% reduction at the mean) only in state-years where they are legally able to leave high school.[2] We further show that, by age 17, these teens are still 0.95pp more likely be in school, suggesting that GDL laws encourage teens not only to postpone dropping out but to eventually complete high school. Indeed, using American Community Survey data, we estimate the interacted model on an adult sample and show that the

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[1]This age 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.

[2]We demonstrate that results are robust to bias from negative weights and dynamic effects using the imputation estimator of Borusyak, Jaravel, and Spiess (2021).

effect persists in the long run. Adults aged 22–34 who experienced a GDL restriction at age 16 and could legally drop out are 0.58pp less likely to eventually obtain a traditional high school diploma.

Identification of the interacted difference-in-differences model requires that the two interacting policies (GDL laws and compulsory schooling laws) be adopted independently as well as conform to parallel counterfactual trends. We provide visual and statistical evidence that adoption of GDL and compulsory schooling laws is not correlated. We then probe the parallel trends assumption by examining the evolution of dropout behavior prior to GDL adoption. We use event studies to examine pre-trends separately in states where compulsory schooling laws always allow dropout and in states that never permit dropout. Estimates reveal parallel pre-trends in both samples and a decline in high school dropout after GDL adoption only in those states that permit dropout.[3] This approach cleanly separates variation in GDL adoption from changes in compulsory schooling laws at the expense of power due to reductions in sample size. Increasing precision with an expanded sample of states that tighten compulsory schooling laws after adopting GDL laws, we show that pre-trends again appear to be parallel. This evidence supports the key identifying assumptions of the interacted difference-in-differences model and thus a causal interpretation of the primary results.

That restricting teen mobility improves educational attainment is surprising because reducing access to an activity should have a weakly negative effect on participation, ceteris paribus. However, driving restrictions may also limit access to other teen activities, such as employment or leisure. The indirect effects on high school completion stemming from changes in access to alternative activities could dominate the direct effect, depending on the magnitude of those indirect effects and substitution patterns between activities. Thus, the sign of the total impact of mobility restrictions on critical human capital accumulation during formative teen years is ex ante ambiguous. The positive estimates on high school retention and completion in our main set of analyses suggest that these indirect margins are important to the underlying teen decision-making process.[4]

To investigate links with alternative activities, we apply the same interacted difference-in-differences design to teen employment outcomes. The resulting estimates show that

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[3]These conclusions are robust to using the dynamic Borusyak, Jaravel, and Spiess (2021) imputation estimator.

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

GDL laws reduce 16-year-old labor force participation by 1.76pp (a 7.5% 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 results cannot distinguish how much of the reduction in teen labor force activity reflects the direct effect of restricting teens' ability to commute to jobs versus indirect effects caused by changes in schoolgoing or leisure activities.

To distinguish these channels, we develop and estimate a multiple discrete choice model. The model rationalizes the reduced-form findings by decomposing total effects into direct effects of GDL laws and indirect effects due to activities being substitutes or complements. In the model, teens may participate in school, work, both activities, or neither activity, and GDL laws may differentially impact each activity. The model is identified using exclusion restrictions based on compulsory schooling laws and labor market conditions. 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 leisure activities.[5] In contrast, the reduction in teen labor force participation due to GDL laws is entirely attributable to the direct effect of reduced commuting access. Differentiating direct and indirect channels not only clarifies and enriches our results, but is also broadly useful in designing future policies to better target teen behavior. The structural model findings suggest that future policies limiting teen mobility can preserve the benefit to educational attainment while avoiding negative teen employment effects by specifically targeting access to non-work, non-school activities.

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 teen

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

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 employment findings despite major differences between our research designs and data. Kennedy (2020) shows that mobility restrictions tied to school performance (in the form of "No Pass, No Drive" laws) do not impact high school graduation, but do delay the decision to drop out. 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.

As such, 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).[6]

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 also provide insight into 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. Thus, it is not reduced work access that increases high school retention. Our findings 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

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

1999).[7]

Finally, we contribute a structural framework for policy analysis that incorporates the interacted difference-in-differences design. This model distinguishes mechanisms, separating direct from indirect (substitution) effects. We show how the model can be adapted to contexts where there is no "outside option" unaffected by the policy of interest by using additional restrictions to set identify a normalizing parameter. 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).[8] Moreover, the model provides an alternative to, and ultimately reinforces, the design-based (i.e., reduced-form) approach. These two methods are complementary.

We describe the background and context for our study and detail data sources in Section 2. In Section 3, we describe the research 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, alternate estimators, and alternate datasets.[9] We also show that effects are long-lived and explore heterogeneity across subgroups and by GDL intensity. In Section 5, we investigate effects on teen employment outcomes. Section 6 unites education and employment decisions within a structural model to differentiate the various effects of GDL laws on teen activities. Section 7 concludes.

2 Context and Data

High teen driving fatality risk in the United States in the 1980s led to the implementation of a number of policies targeted at improving both car safety and limiting teen driving. 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 num-

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[7]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 (Montmarquette, Viennot-Briot, and Dagenais 2007; Dustmann and van Soest 2008).

[8]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).

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

ber of passengers who may ride with a teen driver. GDL laws have 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). GDL laws decrease fatalities primarily by decreasing teen driving rather than improving the quality of teen driving, implying restricted mobility (Karaca-Mandic and Ridgeway 2010; Gilpin 2019).

We develop a database of pertinent state-level GDL laws in the 50 states and DC from several sources, including the Federal Highway Administration's (FHWA) Highway Statistics and the Insurance Institute for Highway Safety (IIHS), covering the years 1990 to 2017.[10] Figure 1a shows counts of the number of states with various types of GDL laws over time.[11] 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. Much of the adoption of GDL laws occurred between 1996 and 2003.

To verify that GDL laws had a binding effect on teen automobile use, we link 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 suggests that GDL laws significantly restrict teen driving. We discuss this verification exercise in detail in Appendix A.

Our research design combines variation in GDL laws with variation in state-specific 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.[12] Figure 1b

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[10]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).

[11]Appendix Figure B.1 shows the same variation as in Figure 1 weighted by state populations (linearly interpolated between census years). Appendix Table B.1 details the years in which GDL laws are adopted.

[12]A precise accounting of these changes is in the replication package and available from the authors.

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

(a) Graduated Driver Licensing Adoption

Driver Licensing Restriction Intensity over Time

(b) Minimum Legal School-Leaving Age
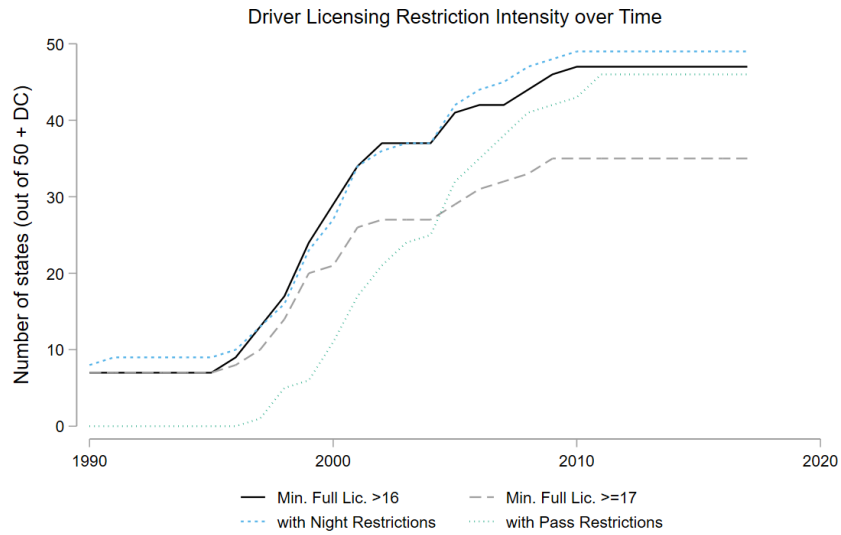
Minimum School-Leaving Age over Time
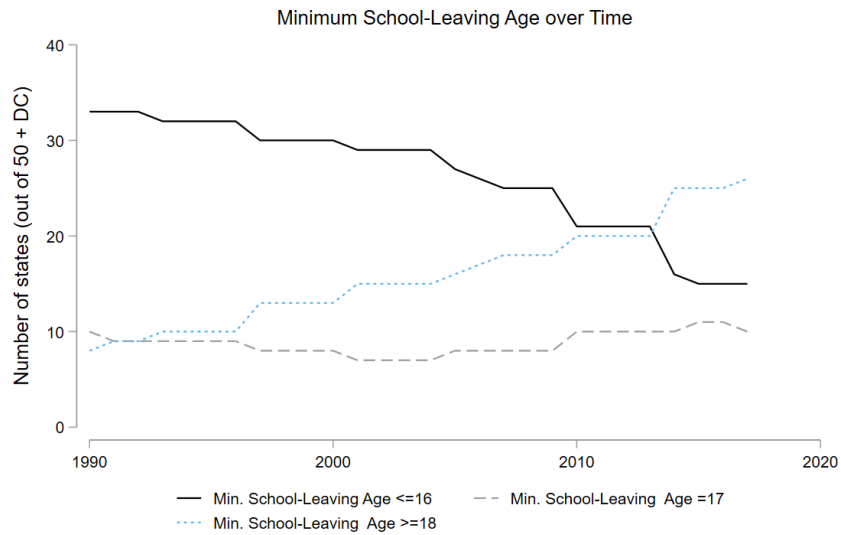
shows counts of the number of states with different minimum school-leaving ages from 1990 to 2017. Over this time period 25 states changed their minimum school-leaving age, in most cases from 16 to either 17 or 18.

We link the data on each state's GDL and CS laws to individual-level data on schooling and work decisions in the Current Population Survey (CPS) Annual Social and Economic

Supplement (ASEC) (Flood et al. 2023).[13] The CPS ASEC data are from an annual survey 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, and, if so, whether they were enrolled full- or part-time.[14] 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, although it incorporates measurement error from those teens who have already completed a high school degree and are not enrolled in college. 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.[15]

We draw 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.[16] 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.

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

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

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

[16]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	0.00	1.00
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.

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. Appendix Table B.2 breaks out average not in school and in labor force rates by period and by GDL treatment cohort and CS status at time of GDL change.

3   Empirical Strategy

Because the effect of mobility restrictions on teen dropout behavior is ex ante ambiguous, we first discuss the channels through which GDL laws might impact teen educational attainment. The introduction of a GDL law restricting teen access to driving may have a *direct* effect on dropout decisions if the restriction hinders teens' ability to commute to

school. Especially for low-income households or teens in rural areas 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 impact a teen's dropout decision *indirectly* by limiting access to labor and leisure activities. Previous studies indicate that driving restrictions decrease teen work and risky teen behavior (Deza and Litwok 2016; Deza 2019; Argys, Mroz, and Pitts 2019; Huh and Reif 2021).

The signs on the indirect effects depend on whether schooling and employment (or schooling and leisure) are complements or substitutes for teens. If work (or leisure) is seen as 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 (net) effect of GDL laws on high school dropout rates is positive in the absence of indirect effects, but may be either positive or negative if indirect effects are significant.

The discussion thus far has assumed that teens can easily choose to drop out of high school in response to mobility restrictions. This assumption likely fails in states that impose compulsory schooling (CS) laws that make it illegal for younger teens to drop out. CS laws thus create a plausible placebo test in state-years where the minimum school-leaving age is greater than the minimum age needed for an unrestricted driver's license. To the extent that compulsory schooling laws are well enforced, these policies effectively shut down the effects of the GDL laws on dropout behavior.

Our primary analysis investigates the effects of GDL law adoption on teen dropout decisions using an interacted difference-in-differences approach that incorporates the plausible placebo test created by CS laws. We estimate the following interacted difference-in-differences specification for the full 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).[18] We

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[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).

[18]We consider as driving restrictions: limits of the time of day that one can drive, limits on the number of

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 school). The vector X_i includes individual-level controls: gender, race/ethnicity indicators, mother's education, presence of father in household, and receipt of SNAP benefits. The variable Z_{st} includes controls for the state's minimum wage, unemployment rate, and presence of a "No Pass, No Drive" (NPND) law.[19] This specification 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).[20] Standard errors permit clustering at the state level. We estimate this model using probit maximum likelihood, though results are very similar for the equivalent linear probability model (see Appendix B).[21] We also implement the imputation estimator of Borusyak, Jaravel, and Spiess (2021), providing further evidence that these results are robust to the choice of estimator.

In Equation 1, the coefficient β_1 identifies the placebo effect of imposing mobility restrictions in states where 16-year-olds cannot legally drop out. We expect the value of β_1 to be close to zero. However, if CS laws are not strict, GDL laws may still impact high school dropouts (i.e., $\beta_1 \neq 0$). In this case, an effect is observable only if students experience direct or indirect effects of the mobility restriction that are large enough to incentivize disregarding CS statutes. For example, in rural areas where school attendance zones are expansive and school buses often require long commutes (Howley, Howley, and Shamblen 2001), the direct effect of GDL laws could be large enough to increase high school dropout rates, even in states with binding compulsory schooling laws.

The coefficient β_2 captures the impact of more lenient compulsory schooling laws (minimum school-leaving age below 17) in the absence of GDL laws. We expect this coefficient to be positive and significant. Finally, β_3 reflects the differential effect of increasing driving restrictions for teens who are legally able to drop out relative to teens who cannot. Of particular interest is the sum $\beta_1 + \beta_3$, which represents the total effect of GDL laws on those teens who are legally permitted to drop out of school. The sign of this net effect will be positive if GDL laws impose only direct effects on access to school, but may be either

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passengers, or limits on destinations. We do not consider a requirement of parental approval a restriction.

[19]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.

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

[21]Given that only 3.8% of 16-year-olds are not in school (Table 1), a probit specification avoids predicting probabilities outside the unit interval.

positive or negative if indirect effects (through labor or leisure) are significant.

3.1 Identification

Identifying the interaction effect between GDL laws and CS laws requires that (i) the two policies are adopted by states independently of one another and that (ii) it is possible to identify the effects of each policy individually on the same set of teens (Almond and Mazumder 2013; Johnson and Jackson 2019). If these conditions hold, then teens who are exposed to both a GDL restriction and a binding minimum dropout age are comparable to teens who are only exposed to one or the other restriction.

To probe whether these two policies are adopted independently, we first note that states rarely changed both policies simultaneously.[22] Most states adopt GDL laws between 1995 and 2001, during which few states changed restrictions to 16-year-old dropout. Instead, states gradually limited 16-year-old dropout throughout our sample window, with an increase in pace around 2010. Thus, the number of states with the interacted treatment ($GDL_{st} * CS_{st}$) increased in the late 1990s, and then began to decline slowly around 2010. As a second piece of evidence, we regress the GDL_{st} indicator on the CS_{st} variable, controlling for covariates. This exercise yields a statistically insignificant point estimate of -0.0062 (p-value = 0.885), indicating little correlation in the take-up of these policies.

Identification further relies on the assumption of parallel counterfactual trends: in the absence of GDL laws, states that were GDL adopters and those that were GDL non-adopters would have experienced similar trends in 16-year-old dropout behavior over time. We examine the validity of this assumption via event study in two separate sub-samples: (1) states where compulsory schooling laws allowed 16-year-olds to drop out for all years in the sample window; and (2) states where compulsory schooling laws always restricted 16-year-old dropout in the sample window. Focusing on these two sub-samples ensures clean variation in GDL laws and removes contamination from compulsory schooling laws, but drops a substantial portion of the data (the 18 states that changed the relevant minimum school-leaving age between 1990 and 2017). Thus, while the event study analyses provide an indirect test of parallel trends, the interacted difference-in-differences model (which uses all states) is better powered for estimating treatment effects.

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

We estimate the event studies corresponding to the following equation using both OLS and a heterogeneity-robust estimator:

$$NotInSchool_{ist} = \sum_{k=\min}^{-2} \theta_k GDL_{s,t+k} + \sum_{k=0}^{\max} \theta_k GDL_{s,t+k} + X_i'\nu + Z_{st}'\mu + D_s + D_t + \epsilon_{ist}, \quad (2)$$

where each $GDL_{s,t+k}$ is an indicator for k years from the adoption of a GDL law. The omitted category is $k = -1$. All other variables are defined as in Equation 1. We estimate Equation 2 separately on two sub-samples of the data. First, we create a "Dropout Always Legal" group of states where the minimum school-leaving age is 16 or younger throughout the entire sample window. The $\theta_{k<0}$ coefficients for this sub-sample identify differences in pre-trends in the dropout behavior of 16-year-olds who are legally permitted to drop out of high school. Under parallel trends, $\theta_{k\geq0}$ identify the effects of GDL mobility restrictions for this sub-sample. In this group of states, the minimum value of k in Equation 2 is -18 (we observe the last-treated state for 18 years before a GDL law is adopted) and the maximum value of k is 21 (we observe the earliest treated state for 21 years after GDL law adoption).

Second, we create a "Placebo/Dropout Never Legal" group of states where the minimum school-leaving age is older than 16 throughout the entire sample window. In this group of states, the minimum value of k in Equation 2 is -19 and the maximum value of k is 19. The θ_k coefficients for this sub-sample identify pre-trend differences in and a potential placebo effect of imposing mobility restrictions on dropout behavior in states where 16-year-olds cannot legally drop out.

Because dynamic two-way fixed effects estimators can be subject to negative weighting issues (Sun and Abraham 2021), we estimate Equation 2 using both an OLS estimator as well as the imputation estimator of Borusyak, Jaravel, and Spiess (2021) (hereafter denoted BJS). The BJS estimator retains attractive efficiency properties under heteroskedasticity and, importantly, recovers a well-defined average treatment effect on the treated (ATT) 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.

We prefer BJS to other recently developed heterogeneity-robust estimators as those estimators rely on comparisons based on subsets of pre-treatment periods, which in our setting tend to have quite small samples.[23] BJS is computationally more robust than esti-

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[23]CPS ASEC samples in 2000 and earlier are 35%–40% smaller than those after 2000. Moreover, post-

mators that individually estimate and aggregate all possible 2x2 difference-in-differences designs (such as Callaway and Sant'Anna 2021). The repeated cross-sectional data in our sample include many such 2x2 designs that are based on a small number of observations (especially in the split sub-samples of "Dropout Always Legal" and "Placebo/Dropout Never Legal" states) and are therefore noisy. The imputation approach uses more information to estimate st-specific treatment effects and so is more efficient.

Figure 2 plots estimates and confidence intervals of the event study coefficients, θ_k. Panel (a) displays estimates for states where dropout is legally permitted for 16-year-olds ("Dropout Always Legal" states).[24] Panel (b) displays estimates for states where 16-year-olds are legally required to stay in school ("Placebo/Dropout Never Legal" states).[25] We first note that both panels of Figure 2 report BJS estimates very similar to OLS estimates, indicating minimal contamination from treatment effect heterogeneity. We next turn to pre-trends analysis. OLS and BJS estimates for the three periods immediately preceding treatment are all small and statistically insignificant for "Dropout Always Legal" states in panel (a). In contrast, post-treatment point estimates are almost always negative for both estimators and often statistically significant, despite the relatively small sample sizes that power these individual-period estimates. While OLS estimates are statistically different from zero for $k \in \{-4, -5\}$, BJS estimates for those periods are quite small and statistically insignificant. In the "Placebo/Dropout Never Legal" sub-sample shown in panel (b), there is no evidence in any pre-treatment period of differential trends between the GDL law adopters and non-adopters. Overall, we do not find meaningful evidence of either confounding treatment effect heterogeneity or differential pre-trends in either of the sub-samples.

A considerable drawback to the event study specification is the splitting of the estimation sample and the exclusion of all data from the 18 states that increased the relevant school-leaving age during our observation window. This limits the model's power to esti-

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2000 sample increases were particularly large in states with high sampling errors for uninsured children, resulting in an even larger increase in the sample of 16-year-olds post-2000.

[24]While we estimate values of θ_k for all possible periods, $k = \{-18, 21\}$, we only display 5 pre-treatment and 15 post-treatment periods in Figure 2a as estimates further out ($k < -5$) are based on quite small samples. The errors for these estimates would compress the scale of the figure.

[25]When using OLS on this sub-sample, we estimate values of θ_k for all possible periods, $k = \{-19, 19\}$. However, because this sub-sample of "Placebo/Dropout Never Legal" states does not include any "never-treated" units (i.e. states that never adopted a GDL law), the BJS estimator requires dropping a reasonable number of pre-treatment periods to comprise the reference group. Also, the BJS estimator cannot estimate time-period fixed effects for periods when all units have already been treated. For these reasons, we limit k to 10 pre-treatment and 10 post-treatment periods for BJS estimation and display only these periods for both the OLS and BJS estimates in Figure 2b.

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

(a) States with school-leaving age ≤ 16 ("Dropout Always Legal")
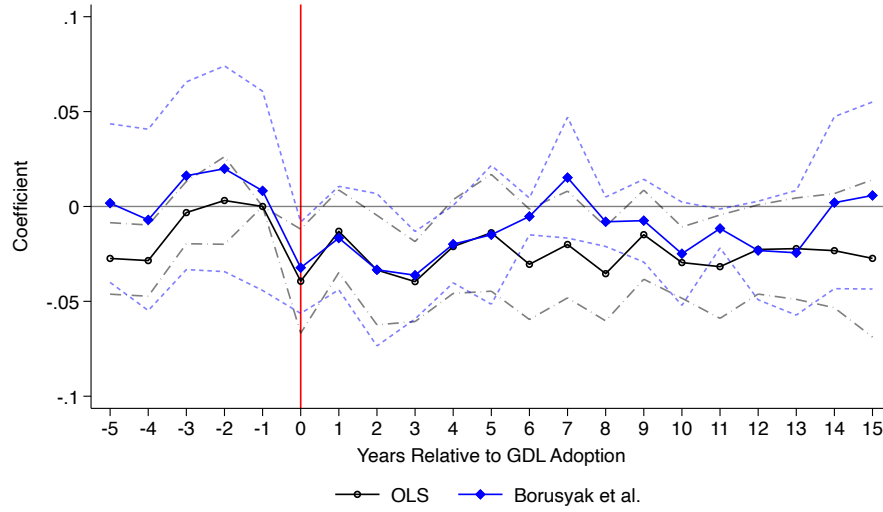
(b) States with school-leaving age > 16 ("Placebo/Dropout Never Legal")
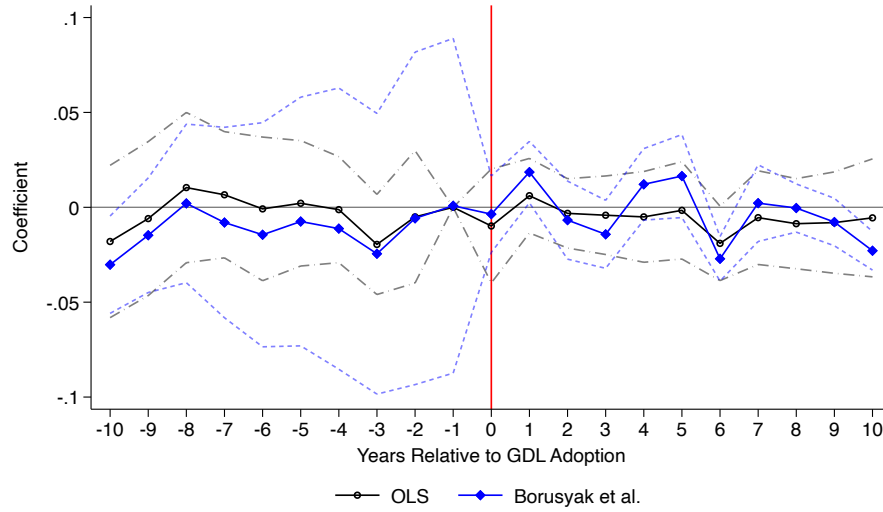
Coefficient estimates of θ_k and 95% confidence intervals in dashed lines using CPS ASEC data from 1990–2017. Controls include: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; NPND laws; state log real effective minimum wage, state fixed effects, and year fixed effects. Standard errors are clustered at the state level. In Panel (a), we estimate values of θ_k for all possible periods, $k = \{-18, 21\}$, but only display 5 pre-treatment and 15 post-treatment periods (see footnote 24). In Panel (b), we estimate values of θ_k for all possible periods, $k = \{-19, 19\}$, but only dispay 10 pre-treatment and 10 post-treatment periods (see footnote 25).

Figure 3: Pre-Treatment Effects of Minimum Unrestricted Driving Age on 16-Year-Old Dropout in "Dropout Currently Legal" States
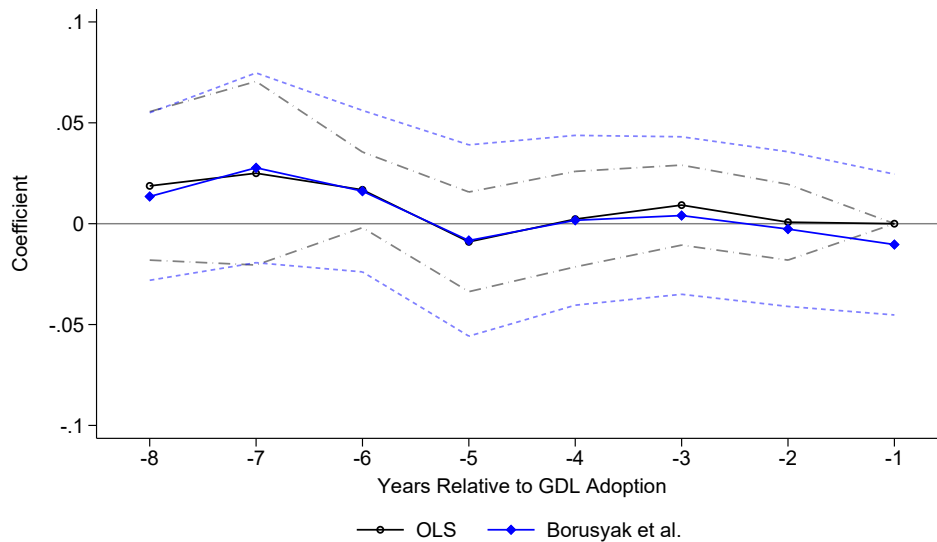
Coefficient estimates of θ_k and 95% confidence intervals in dashed lines using CPS ASEC data from 1990–2017. Controls include: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; NPND laws; state log real effective minimum wage, state fixed effects, and year fixed effects. Standard errors are clustered at the state level.

mate individual treatment-time effects, potentially failing to reject the null-hypothesis of differential pre-trends even when they are present. We mitigate this shortcoming and provide a second indirect test of the parallel trends assumption by expanding the "Dropout Always Legal" sub-sample to include states that only increased their minimum school-leaving age *after* the implementation of a GDL law. This grants more power to precisely estimate individual period effects in the pre-treatment periods.[26]

Figure 3 plots estimates and confidence intervals of the event study coefficients, θ_k, only for the pre-treatment periods using this expanded "Dropout Currently Legal" sub-sample of states.[27] Both the OLS and BJS estimators produce small, statistically insignificant pre-treatment point estimates. This provides strong support for the identifying assumption of parallel counterfactual trends.

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. In

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[26]We do not consider the post-treatment periods in this extended sample as they are potentially contaminated by states that made 16-year-old dropout illegal after adopting a GDL law.

[27]We estimate values of θ_k for all possible periods, $k = \{-18, 21\}$, but only display those for $k = \{-8, -1\}$.

panel (a), though post-treatment point estimates are not statistically significant in every year, there is a clear decline in the probability of 16-year-old dropout that coincides with GDL law adoption. Increasing the minimum driving age in states where 16-year-olds can legally drop out reduces the probability that these teens are no longer in school by between 0.4 and 3.7pp. Panel (b) estimates suggest, on the other hand, that there is no impact of GDL laws on dropout behavior in states where 16-year-olds are legally required to stay in school, confirming the placebo test. We explore these results further using the interacted difference-in-differences model, which expands the estimation sample to include observations in all state-years, in Section 4.

4 Education Results

Table 2 presents the average marginal effects corresponding to each coefficient in Equation 1.[28] In column (1), we estimate the model excluding control variables (X_i and Z_{st}). Column (2) presents our preferred specification, which includes all covariates. These estimates demonstrate that results are not sensitive to or driven by the inclusion of covariates. Estimates of β_1 (the placebo test) are very small and are statistically insignificant, further confirming that there is no effect of GDL laws on 16-year-old dropout behavior in states where dropout is prohibited.

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 larger in states where dropout is legally permitted at that age). Moreover, these estimates are quantitatively similar to those in previous studies that analyze the impacts of compulsory schooling laws (Anderson 2014; Oreopoulos 2009).[29] As we use more recent data than those papers, this provides some evidence that compulsory schooling laws continue to be impactful for educational attainment.[30]

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[28]Results estimated using a linear probability model are shown in Appendix Table B.3 and are qualitatively and quantitatively similar.

[29]Oreopoulos (2009) finds that a school-leaving age below 16 increases the fraction of 20- to 24-year-olds reporting less education than a high school degree by 1.3pp. Our estimate in Table 2 is slightly larger (1.9pp), 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 having less than a HS diploma at age 22-34. Our estimates are also bounded on the upper end by Anderson (2014), who finds that a school-leaving age of 18 or older reduced high school dropout rates by 2pp.

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

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

	Not In School = 1						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Min. Unres. Driving Age >16 (β_1)	0.0022 (0.0043)	0.0014 (0.0040)	0.0014 (0.0042)	0.0033 (0.0051)			
School-Leaving Age $\leq$ 16 (β_2)	0.0200*** (0.0049)	0.0191*** (0.0049)					
Min. Unres. Driving Age >16 $\times$ School-Leaving Age $\leq$ 16 (β_3)	-0.0132*** (0.0050)	-0.0129** (0.0052)	-0.0123** (0.0058)	-0.0200*** (0.0077)			
Effect of GDL if **School-Leaving Age $\leq$ 16** ($\beta_1 + \beta_3$)	-0.0110** (0.0051)	-0.0115** (0.0052)	-0.0109** (0.0054)	-0.0167* (0.0086)	-0.0109** (0.0047)	-0.0111** (0.0045)	-0.0113** (0.0046)
Estimator	Probit	Probit	Probit	Probit	Imputation	Imputation	Imputation
School-Leaving Age	As Observed		Fixed in Yr. of GDL Change	Never Switchers Only		As Observed	
Controls	-	Y	Y	Y	-	Y	Y
Exclude Always Treated	-	-	-	-	Y	Y	Y
Exclude Never Treated	-	-	-	-	-	-	Y
Obs	75,196	75,196	75,196	46,567	50,729	50,729	46,853

Average marginal effects from probit regression (columns 1–4) and from the imputation estimator of Borusyak, Jaravel, and Spiess (2021) (columns 5–7) using CPS ASEC data from 1990–2017. All specifications include state and year fixed effects. Controls in columns (2)–(4) and (6)–(7) 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. Column (3) also includes indicators for the state minimum legal dropout age. Column (3) fixes school-leaving age to its level when the state increased minimum unrestricted driving age to >16, while column (4) limits the sample to states that never changed school-leaving age. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

Estimates of β_3 indicate that the differential effect of GDL laws on dropout behavior for 16-year-olds in states where dropout is legally permitted (relative to states where dropout is not legal) is negative and statistically significant. The total (or net) effect of GDL laws on teen dropout behavior is estimated by the sum of coefficients, $\beta_1 + \beta_3$. This sum reveals that increasing the minimum driving age in states where 16-year-olds can legally drop out reduces the probability that these teens are no longer in school by approximately 1.15pp, a 30% reduction from the mean.[31] This negative point estimate reveals that teens respond to reduced access to driving by staying in school longer. The negative sign of this net effect indicates that any direct effect of GDL laws on high school attendance (by decreasing access to school) is more than completely offset by the indirect effects of GDL laws operating through reduced access to other activities (labor, leisure, or both).

We next employ several robustness checks of this main finding. Additional results show that we can relax separability assumptions on the effects of GDL and CS laws. We

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[31]This is smaller than the simple difference-in-differences estimates shown in Table C.1, indicating that, if there is bias from the simultaneous changes in compulsory schooling laws (de Chaisemartin and D'Haultfœuille 2023), it is attenuating estimates of β_3 toward zero.

also address the literature on staggered adoption differences-in-differences models and show that our results are robust to an alternative estimator that does not rely on an assumption of homogenous treatment effects. In Section 4.1, we further show that our results can be replicated using an alternative dataset. In Section 4.2, we test whether the effects on human capital accumulation persist to the medium- and long-run. We then examine heterogeneity in effects by the demographics of the treated population (Section 4.3) and by variation in the intensity of GDL regulation (Section 4.4).

Contamination from Changes in CS Laws. To mitigate any potential for conflating effects of changes in CS laws in the interacted difference-in-differences design, we employ multiple robustness checks.[32] 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.[33] Results, shown in column (3) of Table 2, are nearly identical to the preferred specification in column (2).[34] Second, we estimate Equation 1 on the subsample of states that did not change their minimum school-leaving age during the time period under study.[35] Results, shown in column (4) of Table 2, are a bit larger in magnitude than in the main specification and remain statistically significant at the 10% level, despite the reduced sample size.

Bias from Treatment Effect Heterogeneity. A growing literature has revealed that two-way fixed effects estimation of staggered adoption difference-in-differences research designs does not generally identify the average treatment effect on the treated (ATT) when treatment effects are heterogeneous or dynamic (e.g., de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021). While several solutions have been proposed, none (thus far) fit our setting of repeated cross-sectional data with an interacted difference-in-differences

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[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 (thanks to Bell, Costa, and Machin 2022) and show that our main results are robust to dropping states with employment exemptions in their CS laws. These results are available upon request.

[33] In this specification we also control separately for the actual time-varying school-leaving age.

[34] Note that in these alternate specifications, the coefficient β_2 is absorbed by the state fixed effects.

[35] That is, the union of "Dropout Always Legal" and "Placebo/Dropout Never Legal" sub-samples used to estimate Equation 2.

design where policy interactions turn on and off again. Because no currently available estimators precisely fit our design, we recast our primary design in order to apply the BJS imputation estimator.

For this exercise, treatment is the interaction term $GDL_{st} * CS_{st}$, though CS_{st} also enters as a control variable. The sample excludes observations in always-treated states and in state-years after the interacted treatment turns off (see Appendix C for a detailed discussion of this estimation strategy). We present the BJS estimates in columns (5)-(7) of Table 2. In Column (5), we omit all controls except CS_{st}. Column (6) includes all controls. Column (7) omits never-treated units to test whether our results hinge on comparisons to states that are subject to different trends than those that eventually adopt GDL laws; they do not. Estimates across these three columns are strikingly similar to our main results and are statistically significant, despite the smaller sample sizes.

We also estimate two models similar to our preferred specification that consider subsets of the time variation used in the full analysis and allow for some dynamism in treatment effects. The results (shown in Table C.2 and Table C.3 of Appendix C) provide evidence that our main results are not being driven by long-run dynamics in the treatment effects of GDL laws and that effects remain relatively constant over time. Taken together, these exercises indicate that these results are robust to dynamics, to arbitrary treatment effect heterogeneity, and to reasonable restrictions on the control group.

DD Estimates. We employ a simpler difference-in-differences approach that is analogous to aggregating all pre- and post-treatment years of the event study model in Equation 2. Appendix C reports results from this model estimated separately on the "Dropout Always Legal" and the "Placebo/Dropout Never Legal" sub-samples. Probit and BJS estimates confirm that GDL laws reduce the probability of 16-year-old dropout, with estimates varying between -1.6pp and -1.9pp for the "Dropout Always Legal" states (given the much smaller samples, one of the four reported estimates is marginally insignificant). Effects in placebo states are quantitatively small and statistically insignificant.

4.1 Alternative Dropout Data

To further support these findings, we analyze 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.

21

A key advantage of this dataset is that, because it includes data by school district, we can include school district fixed effects to control for time-invariant differences between places within states; we discuss this data and analysis in detail in Appendix D.

We find that the implementation of GDL laws leads to a 0.43pp reduction in 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 exclusively on the immediate impacts of GDL laws on the behavior of 16-year-olds. To determine whether GDL laws have lasting effects on educational attainment, we next look for medium-run effects on the dropout behavior of individuals once they have turned 17.

We create an alternate sample from the CPS ASEC of 17-year-olds and estimate a slightly modified version of our preferred interacted difference-in-differences model:

$$NotInSchool_{ist} = \beta_1 GDL_{st-1} + \beta_2 CS_{st} + \beta_3 GDL_{st-1} * CS_{st} + X_i' \nu + D_s + D_t + \epsilon_{ist}, \quad (3)$$

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). The compulsory schooling laws are captured by CS_{st}, which is an indicator that 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), but drop other controls for consistency with the longer-run results below.

Column (1) of Table 3 reports the average marginal effects from estimating Equation 3 on the main estimation sample of 16-year-olds but using only the limited gender and race/ethnicity controls (for comparison). Column (2) displays the average marginal effects 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 experienced the driving restriction. There is no evidence of an impact of GDL laws in placebo states where school attendance is compulsory. On the other hand, GDL laws create a 1.0pp decline in the probability of dropping out of school where dropout is legal at age 17. These results are very similar to the effects on 16-year-olds, even though we expect some attenuation due to high school completion by 17-year-olds. This suggests that GDL laws 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, we next turn to the 2008–2019 American Community Survey (ACS) data to examine educational attainment among US-born respondents aged 22 to 34.[36] 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:

$$y_{isca} = \beta_1 GDL_{sc} + \beta_2 CS_{sc} + \beta_3 GDL_{sc} * CS_{sc} + X_i'\nu + D_{sa} + D_{ca} + \epsilon_{isca}. \tag{4}$$

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-age fixed effects, D_{ca}, control for general age-specific trends or cohort trajectories.[37]

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[36] 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 their secondary education.

[37] It is also possible to use sample year and age to index Equation 4; this yields identical estimates of β.

Table 3: Effects of Minimum Unrestricted Driving Age on Long-Run Educational Attainment

	CPS Sample		ACS Sample					
	Not In School = 1				Did Not Complete HS or GED		Did Not Complete HS (excl. GED)	
	At Age 16	At Age 17	Max Grade ≤ 10					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Min. Unres. Driving Age >16 (β_1)	0.0021	0.0060	-0.0011	-0.0008	-0.0001	0.0006	-0.0000	0.0006
	(0.0043)	(0.0052)	(0.0014)	(0.0014)	(0.0016)	(0.0017)	(0.0025)	(0.0027)
School-Leaving Age Allows Dropout (β_2)	0.0197***	0.0111**	0.0034**	0.0048***	0.0048*	0.0069***	0.0077*	0.0106**
	(0.0050)	(0.0054)	(0.0016)	(0.0016)	(0.0025)	(0.0024)	(0.0043)	(0.0043)
Min. Unres. Driving Age >16 × School-Leaving Age Allows Dropout (β_3)	-0.0132***	-0.0155**	-0.0023	-0.0028	-0.0031	-0.0041*	-0.0048*	-0.0064**
	(0.0051)	(0.0064)	(0.0016)	(0.0018)	(0.0020)	(0.0022)	(0.0025)	(0.0028)
Effect of GDL if School-Leaving Age Allows Dropout ($\beta_1 + \beta_3$)	-0.0111**	-0.0095*	-0.0034***	-0.0036**	-0.0032*	-0.0035	-0.0049**	-0.0058**
	(0.0051)	(0.0055)	(0.0012)	(0.0014)	(0.0019)	(0.0022)	(0.0024)	(0.0027)
Mean of Outcome	3.8%	6.2%	4.0%	4.4%	7.9%	8.7%	12.3%	13.3%
Obs	75,196	73,187	4,779,503	3,264,783	4,779,503	3,264,783	4,779,503	3,264,783
Limit Sample to Those Residing in Birth State	–	–	No	Yes	No	Yes	No	Yes

Columns 1 and 2 represent average marginal effects from probit regression 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 3–8 are coefficients from a linear probability model 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

Columns (3)–(8) of Table 3 reveal that GDL laws improve several measures of long-run educational attainment in settings where teen dropout is permitted. Odd columns use the full ACS estimation sample, while even columns restrict the sample to those living in their state of birth at the time of survey in order to reduce measurement error in treatment. Adults who experienced a GDL law at age 16 are 0.3pp–0.4pp less likely to have no more than a 10th-grade education. There is a similar (though less precise) effect on the probability of high school completion inclusive of GED diplomas. Excluding GEDs, adults who experienced GDL laws at age 16 in states where dropout was permitted are 0.5pp-0.6pp more likely to have obtained a traditional high school diploma. These long-run findings are all the more striking because of the significant likelihood of 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 use our preferred specification (Equation 1) to explore heterogeneity across sub-populations. Estimated marginal effects are shown in Table 4, which also reports mean outcome values for each subgroup, and Figure 4. The top-left panel of Figure 4 reports the effects of GDL laws on 16-year-old dropout separately for males and females. The top three estimates show the effects of GDL laws in states where dropout is not legal (β_1) for the full sample, for male teens only, and for female teens only. The bottom three estimates show the effects of GDL laws in states where dropout is legal for 16-year-olds ($\beta_1 + \beta_3$) for those same three populations. It is clear from these estimates that there are no meaningful differences in the effects of GDL laws by sex, and a Wald test reveals that the estimates are also not statistically different.[38]

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

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

Figure 4: The Effect of Minimum Unrestricted Driving Age on 16-Year-Old Dropout for Sub-Populations
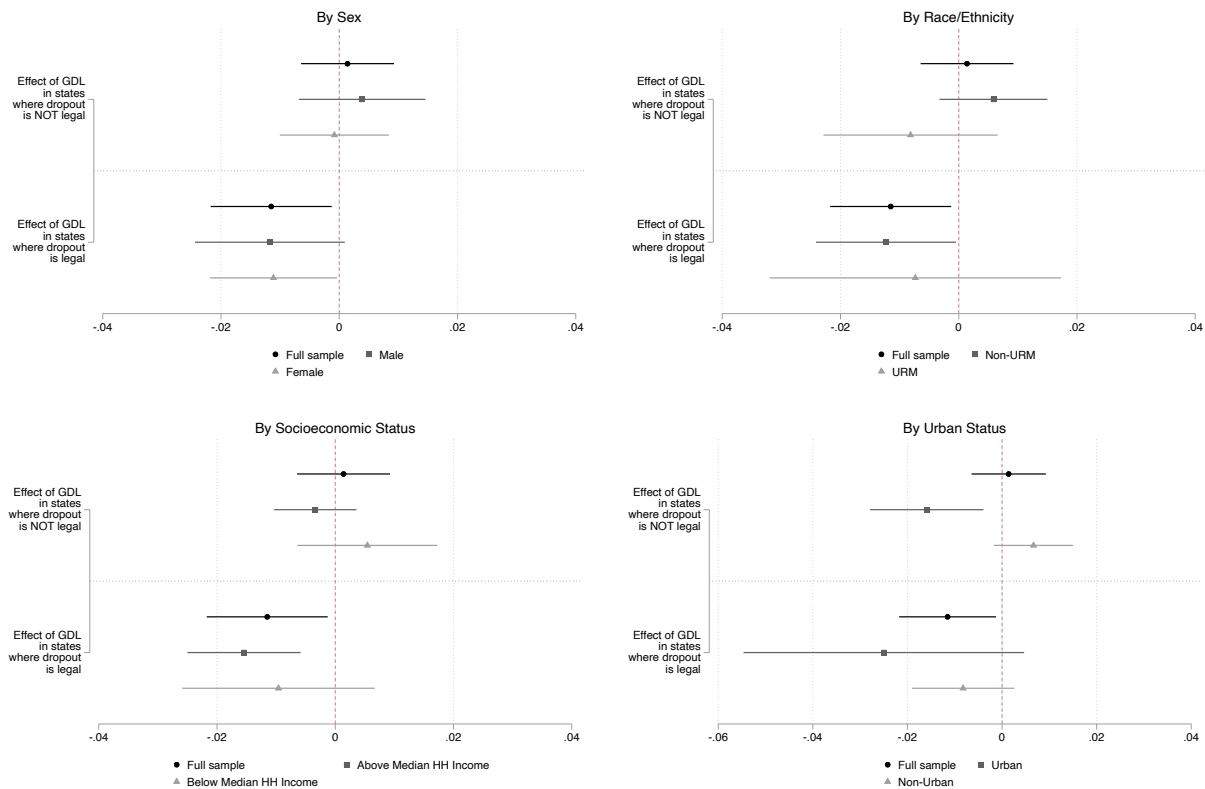
Average marginal effects from probit regression using CPS ASEC data from 1990–2017. Bars show 95% confidence intervals. All specifications include state and year fixed effects. Controls include: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; NPND laws; and state log real effective minimum wage. Standard errors are clustered at the state level.

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

Table 4: 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)	0.0014 (0.0040)	0.0039 (0.0055)	-0.0008 (0.0047)	0.0059 (0.0047)	-0.0081 (0.0075)	-0.0034 (0.0035)	0.0054 (0.0060)	0.0066 (0.0043)	-0.0159*** (0.0061)
School-Leaving Age $\leq$ 16 (β_2)	0.0191*** (0.0049)	0.0238*** (0.0068)	0.0149*** (0.0057)	0.0194*** (0.0058)	0.0242** (0.0109)	0.0152*** (0.0049)	0.0234** (0.0101)	0.0172*** (0.0049)	0.0307*** (0.0114)
Min. Unres. Driving Age >16 $\times$ School-Leaving Age $\leq$ 16 (β_3)	-0.0129** (0.0052)	-0.0156** (0.0071)	-0.0103* (0.0054)	-0.0182*** (0.0061)	0.0008 (0.0113)	-0.0121*** (0.0047)	-0.0150* (0.0090)	-0.0149*** (0.0053)	-0.0091 (0.0149)
Effect of GDL if School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	-0.0115** (0.0052)	-0.0117* (0.0065)	-0.0111** (0.0055)	-0.0123** (0.0060)	-0.0073 (0.0126)	-0.0154*** (0.0049)	-0.0096 (0.0083)	-0.0082 (0.0055)	-0.0250* (0.0151)
Mean Outcome	0.038	0.040	0.035	0.032	0.050	0.024	0.051	0.035	0.046
Obs	75,196	38,587	36,609	52,641	22,441	37,598	37,598	59,227	15,897

Average marginal effects from probit regression using CPS ASEC data from 1990–2017. All specifications include state and year fixed effects. Controls include: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; 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

In the bottom-left panel of Figure 4 (and columns (6)–(7) of Table 4), we split the sample into two halves based on household income (as reported in the CPS). The median household income is $53,236 (in 1999 dollars). Sixteen-year-olds in lower-income households are more than twice as likely to be observed as not in school than those in higher-income households. However, the estimated effects of GDL laws are noticeably smaller and less precise for the lower-income sub-sample (despite having the same sample size). This provides some support to the hypothesis that teens from lower-income backgrounds are more likely to experience direct effects of the GDL laws making travel to school more difficult and therefore increasing the probability of dropout. Those (positive) direct effects would then counterbalance the (negative) indirect effects and lead to a combined effect that is closer to zero. An alternative explanation is greater vehicle availability for teens in higher-income households, for whom GDL laws decrease the probability of high school dropout by 64% at the mean. Note, however, that the difference in the estimates across the lower-income and higher-income groups is not statistically significant.

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

4.4 Variation in GDL Intensity

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

As discussed in Section 2, GDL laws create an intermediate licensing level that restricts nighttime driving 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 the dropout decision. Because the intermediate license primarily restricts nighttime driving and carpooling, it seems less likely that this type of GDL restriction would hinder the teen's ability to commute to school. On the other hand, when a teen has no access to driving, we expect to see both an indirect channel from reduced access to labor and leisure activities 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:

$$
\begin{aligned}
NotInSchool_{ist} = {} & \beta_1^A IntLicense_{st} + \beta_1^B NoLicense_{st} + \beta_2 CS_{st} \\
& + \beta_3^A IntLicense_{st} * CS_{st} + \beta_3^B NoLicense_{st} * CS_{st} \\
& + X_i'\nu + Z_{st}'\mu + D_s + D_t + \epsilon_{ist}.
\end{aligned}
\tag{5}
$$

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. The marginal effects estimates from this expanded model are shown in Table 5.

Estimates of the placebo test 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 estimate of β_3^A indicates that the differential effect of having access to an intermediate license only for 16-year-olds in states where dropout is legally permitted is negative and statistically significant. The total effect of the restriction to an intermediate license on teen dropout behavior is estimated by the sum of coefficients, $\beta_1^A + \beta_3^A$. This sum reveals that limiting teen driving access to only the intermediate license level reduces the probability of high school dropout by 0.99pp in states where compulsory schooling laws are non-binding. 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

Table 5: 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)	0.0038	0.0029
	(0.0044)	(0.0041)
No License (β_1^B)	0.0023	0.0018
	(0.0058)	(0.0052)
School-Leaving Age $\leq$ 16 (β_2)	0.0190***	0.0183***
	(0.0050)	(0.0050)
GDL at 16 $\times$ School-Leaving Age $\leq$ 16:		
Intermediate License Only (β_3^A)	-0.0137***	-0.0134***
	(0.0048)	(0.0051)
No License (β_3^B)	-0.0020	-0.0035
	(0.0060)	(0.0063)
Effect of Intermediate License Only if	-0.0099*	-0.0106**
School-Leaving Age $\leq$ 16 ($\beta_1^A + \beta_3^A$)	(0.0051)	(0.0052)
Effect of No License if	0.0003	-0.0017
School-Leaving Age $\leq$ 16 ($\beta_1^B + \beta_3^B$)	(0.0086)	(0.0083)
Additional Effect of No License if	0.0102**	0.0089*
School-Leaving Age $\leq$ 16 ($\beta_1^B + \beta_3^B$)- ($\beta_1^A + \beta_3^A$)	(0.0048)	(0.0047)
Controls	-	Y
Obs	75,196	75,196

Average marginal effects from probit regression using CPS ASEC data from 1990–2017. All specifications include state and year fixed effects. Controls in column (2) are: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; 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

and/or leisure activities caused by limiting 16-year-old driving privileges leads to a 26% reduction in the probability of high school dropout among this age group.

The estimate of β_3^B indicates that the differential effect of having no access to driving for 16-year-olds in states where dropout is legally permitted (vs. states where the CS laws are binding) is negative but statistically insignificant. The total effect of the restriction to no license on teen dropout behavior is estimated by the sum of coefficients, $\beta_1^B + \beta_3^B$. This sum is almost precisely zero.[39] This estimate suggests that the negative effect of the GDL law on high school dropout stemming from reduced access to alternate activities is offset

---

[39]Note that only 12 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.

by a positive direct effect stemming from reduced ability to commute to school when teen access to driving is completely removed.

Also of interest here is the difference between the two total effect estimates, $(\beta_1^B + \beta_3^B)$ - $(\beta_1^A + \beta_3^A)$. This difference identifies the *additional* effect of going from a GDL law that restricts teens to an intermediate license only to a GDL law that fully restricts teen driving (at age 16). This estimate, a 0.89pp *increase* in the probability of high school dropout, again suggests that there is a significant direct effect of the GDL laws on teens' ability to commute to school that can lead to an increase in high school dropout if teen access to driving is completely removed. Note, however, that interpreting this point estimate solely as the direct effect requires the strong assumption that the indirect effect of fully restricting teen driving is no larger than the indirect effect of the intermediate license alone. Therefore, we take the estimates in Table 5 as merely an indication that both direct and indirect channels exist for this policy and rely on structural estimation to provide a more formal effect decomposition in Section 6.

5 Employment Results

We next study the effects of GDL laws on teen employment. 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 Equation 1 with an indicator for whether a 16-year-old is currently in the labor force:

$$LFP_{ist} = \beta_1 GDL_{st} + \beta_2 CS_{st} + \beta_3 GDL_{st} * CS_{st} + X_i'\nu + Z_{st}'\mu + D_s + D_t + \epsilon_{ist}. \quad (6)$$

All other variable definitions are unchanged.

In Figure 5, we illustrate the potential direct and indirect channels through which GDL laws might impact teen labor force participation. In panel (a), we consider the case where the teen resides in a state with compulsory schooling laws that do not permit dropping out at age 16. In this case, the restriction on teen driving imposed by the GDL laws will have a negative direct effect on employment. However, the GDL laws may also impact teen employment indirectly by limiting access to leisure activities. This indirect effect will have a positive effect on employment.[40] Because of the binding compulsory schooling laws, there is no effect of the GDL laws on the teen's schooling decision (and therefore,

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[40]Figure 5 implicitly assumes that work and leisure are substitutes. We do not impose substitutability for estimation of Equation 6.

Figure 5: Direct and Indirect Effects of GDL Laws on Labor Force Participation

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

(b) When Teens Can Legally Drop Out, $CS_{st} = 1$
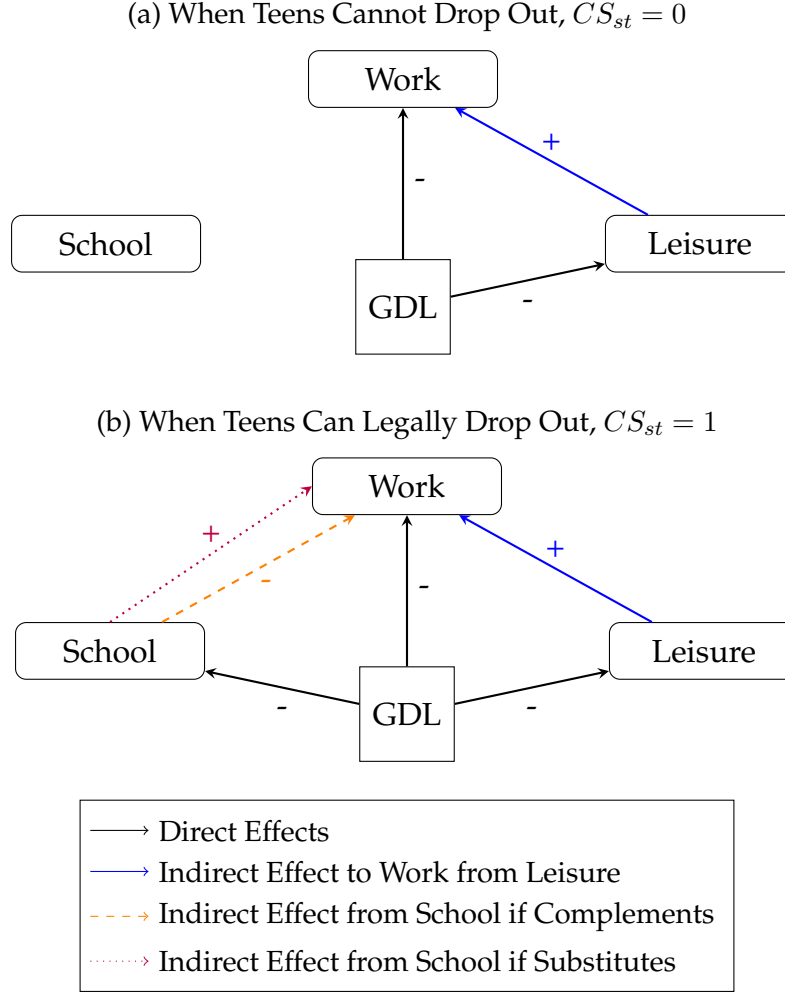
no indirect effect on teen employment coming through that channel). The coefficient β_1 in Equation 6 captures the sum of the direct effect and the indirect effect from leisure when CS laws prohibit 16-year-old dropout.

In panel (b) of Figure 5, we illustrate the case where CS laws are non-binding and 16-year-olds are legally permitted to drop out of school. This adds an additional channel through which GDL laws can impact teen labor force participation. Namely, the reduction in access to school may have an indirect effect on teen employment decisions. If teens view work and school as substitutes, then the indirect effect caused by reduced access to school contributes *positively* to labor force participation. If instead, they view work and school as complementary activities, then the indirect effect from the school channel contributes *negatively* to labor force participation. In Equation 6, β_3 captures this additional

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indirect channel between work and school.

Columns (1)–(2) of Table 6 show estimated average marginal effects from the interacted difference-in-differences model of Equation 6. Increasing the minimum driving age has a small, statistically insignificant effect on 16-year-old labor force participation in states where dropping out is disallowed. As discussed above, this estimate reveals the sum of the (negative) direct effect of GDL laws on teen labor force participation and the (positive) indirect effect stemming from reduced access to leisure activities. We can therefore interpret this null finding as an indication either that neither of these two effects is very large, or that they are approximately equal in magnitude (and opposite in sign).

Conversely, GDL laws significantly decrease labor force participation by 1.76pp in states where teens are legally able to drop out. At the mean, this is a 7.5% reduction in 16-year-old labor force participation (about one quarter of 16-year-olds work in this sample; see Table 1).[41] This negative estimate indicates that allowing for the additional channel of high school dropout creates a negative indirect effect on teen labor force participation. In other words, when GDL laws reduce access to school, the negative direct effect on school-going also leads to a negative indirect effect on the propensity to work. This is consistent with a model in which teens view work and school as complementary activities, rather than as substitutes.

As with the education analysis, we show that these employment results are not being confounded by the evolution of CS laws.[42] Column (3) replaces CS_{st} with a time-invariant measure that fixes the CS law at its value in the year that the GDL laws first bind for 16-year-olds or in 1990 (for states that did not change their CS law in the sample). Column (4) restricts the sample to states that did not change their minimum school-leaving age during the sample. Results from both exercises are stronger than baseline estimates. We also estimate the recast, interacted difference-in-differences design using the BJS estimator (see Appendix C for details). The estimates, shown in columns (5)–(7) of Table 6, are similar to our main results and are actually larger and more precise.[43]

We differentiate effects on full- and part-time employment in Appendix Table B.5.

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[41]Results are similar if we replace the dependent variable with an indicator for employment rather than labor force participation. Moreover, results from a linear probability model are similar but larger in magnitude and substantively more significant (see Appendix Table B.4).

[42]We also find that results are not confounded by measurement error in the CS_{st} variable caused by CS law employment exemptions. The results in Table 6 are qualitatively unchanged when we drop states that have employment exemptions in their CS laws. These results are available upon request.

[43]In part, effects may be larger because employment levels are lower in the later years that are dropped for the BJS exercise. Note also that the BJS exercises assume that $\beta_1 = 0$. This assumption is supported by the nearly-zero point estimates of β_1 in columns (1)–(3) of Table 6.

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

| | In Labor Force = 1 | | | | | | |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Min. Unres. Driving Age >16 (β_1)	-0.0032 (0.0100)	-0.0024 (0.0110)	0.0007 (0.0103)	-0.0105 (0.0132)			
School-Leaving Age ≤ 16 (β_2)	0.0238 (0.0156)	0.0173 (0.0161)					
Min. Unres. Driving Age >16 $\times$ School-Leaving Age ≤ 16 (β_3)	-0.0134 (0.0127)	-0.0152 (0.0133)	-0.0206* (0.0125)	-0.0172 (0.0189)			
Marginal Effect of GDL if School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	-0.0166* (0.0097)	-0.0176* (0.0099)	-0.0200* (0.0105)	-0.0277** (0.0142)	-0.0295*** (0.0101)	-0.0273*** (0.0098)	-0.0281*** (0.0101)
Estimator	Probit	Probit	Probit	Probit	Imputation	Imputation	Imputation
School-Leaving Age	\multicolumn{2}{c}{As Observed}	Fixed in Yr. of GDL Change	Never Switchers Only		\multicolumn{2}{c}{As Observed}
Controls	-	Y	Y	Y	-	Y	Y
Exclude Always Treated	-	-	-	-	Y	Y	Y
Exclude Never Treated	-	-	-	-	-	-	Y
Obs	75,196	75,196	75,196	46,567	50,729	50,729	46,853

Average marginal effects from probit regression (columns 1–4) and from the imputation estimator of Borusyak, Jaravel, and Spiess (2021) (columns 5–7) using CPS ASEC data from 1990–2017. All specifications include state and year fixed effects. Controls in columns (2)–(4) and (6)–(7) 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. Column (3) also includes indicators for the state minimum legal dropout age. Column (3) fixes school-leaving age to its level when the state increased minimum unrestricted driving age to >16, while column (4) limits the sample to states that never changed school-leaving age. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

Between half and three-quarters of the overall employment effect comes from reduced full-time employment. This effect on full-time work is not necessarily driven by teens who have dropped out of school: more than half (56%) of full-time teen workers also report attending school.[44] Notably, there is a significant negative effect of GDL laws on full-time employment regardless of whether dropout is permissible or not. This strongly suggests a negative direct effect of GDL laws on full-time work.

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

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[44]This concentration on full-time employment does not necessarily imply that effects on part-time employment are insubstantial. It could well be that GDL laws lead some teens to flow from full-time to part-time work and other teens with part-time work to exit the labor force.

well. We next turn to a formal discrete choice model to better understand these findings.

6 Distinguishing Channels with Model-Based Analysis

The positive effect of GDL laws on high school retention likely reflects the indirect consequences of work and leisure decisions, which dominate any direct effects on access to school. Which of these indirect channels is most important depends on substitution patterns between school and work. If school and work are highly substitutable, then the negative effect of GDL laws on high school dropout rates reflects a reduction in teen labor force participation. If not, then the change in dropout behavior can be attributed to changes in access to other activities.

Distinguishing between direct and indirect channels has important implications for policy design. To do so, we develop a model that adapts the multiple discrete product choice model of Gentzkow (2007) to the context of teen schooling and employment decisions.[45] Agents select work, school, both work and school, or neither activity. School and work can be complements or substitutes.[46] Agents have potentially correlated idiosyncratic preferences for these activities. Overall, the model has similarities to Montmarquette, Viennot-Briot, and Dagenais (2007) or a static version of Eckstein and Wolpin (1999), though we focus on estimating and decomposing treatment effects and cleanly identifying school-work spillovers.

6.1 Model Description

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

$$V_i(0,0) = \tilde{\gamma}^0 GDL_{st}^0 \tag{7}$$

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

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

$$V_i(1,1) = V_i(1,0) + V_i(0,1) + \Gamma + (\gamma^\Gamma - \tilde{\gamma}^0)GDL_{st}^\Gamma, \tag{10}$$

---

[45]An alternative model is bivariate probit with both outcomes endogenous. However, Lewbel (2007) shows that such a model is generally incoherent and incomplete.

[46]Because the activities do not have observed pecuniary costs, they are substitutes (complements) in the sense that restricting access to one activity increases (reduces) demand for the other activity.

where γ^{k_+} are the parameters of interest intended to capture the utility effect of the graduated driver license policy, for $k_+ \in \{0, A, B, \Gamma\}$. We index GDL by k_+ to anticipate the counterfactual decomposition in Section 6.3; each individual experiences only one value of $GDL_{st}^{k_+}$ (equal to GDL_{st}), but this does not hold in the counterfactual exercise.

The idiosyncratic error terms reflect the latent indirect utility associated with each activity, and may be correlated. Thus, e_i^A can be interpreted as motivation to work or labor force attachment, and e_i^B can be interpreted as motivation for school or expected returns to schooling. Teens with low values of both terms have a high value of leisure. The utility agents get for choosing both activities is the sum of utilities for each activity plus $\Gamma + (\gamma^\Gamma - \tilde{\gamma}^0)GDL_{st}^\Gamma$, which is positive if school and work are relative complements and negative if they are relative substitutes.

The vector z_{st} includes state-year characteristics (which provide exclusion restrictions for identification):

$$z_{st}' = \left[UR_{st}, \ln(MW_{st}), CS_{st}, GDL_{st}^B \times CS_{st} \right].$$

Here, UR_{st} is the state-level unemployment rate, $\ln(MW_{st})$ is log real minimum wage, and CS_{st} and GDL_{st} are measures of the compulsory schooling laws and graduated driver licensing laws, as in Equation 1. The $f^k(s, \xi)$ terms include correlated random effects (discussed below) and δ_t^k represent year dummies, $k \in \{A, B\}$. The model also 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. Agents choose the bundle $(y_i^A, y_i^B) \in \mathcal{C}$ that maximizes utility $V_i(y_i^A, y_i^B)$.

The model allows for a non-standard normalization that nests the standard normalization when $\tilde{\gamma}^0 = 0$. Because discrete choice models identify only *relative differences* in utility, the utility of one choice is typically normalized to zero (e.g., $V_i(0,0) = 0$). Such a normalization does not affect model fit or identification, but makes the implicit assumption that the utility of the normalized option is not affected by treatment. However, the literature relating changes in teen risky behaviors to GDL law adoption strongly suggests that the value of the neither-work-nor-school option was shifted by the implementation of GDL policies (e.g., Deza and Litwok 2016; Deza 2019; Huh and Reif 2021). Accordingly, we interact an auxiliary parameter, $\tilde{\gamma}^0$, with the policy, $V_i(0,0) = \tilde{\gamma}^0 GDL_{st}^0$, to capture the impact of GDL laws on the utility of the normalized option. We then set the utility for each other option to: $V_i(y_i^A, y_i^B) = \tilde{V}_i(y_i^A, y_i^B) - \tilde{V}_i(0,0) + \tilde{\gamma}^0 GDL_{st}^0$. The observed data are compatible with any value of the auxiliary parameter; $\tilde{\gamma}^0$ merely redistributes the impact

of the GDL laws to the outside option.

The model can be used to decompose total effects into their direct and indirect channels. **Total effects** of GDL laws on each activity capture the overall impact of increasing the minimum unrestricted driving age from 16 or less. Total effects are the model analogs to the reduced-form estimates shown in Section 4 and Section 5. **Direct effects** (denoted θ_{Dir}^k) reflect how each GDL component affects its *own activity*, e.g., the effect of GDL^A on working and of GDL^B on school enrollment. **Indirect effects** capture the consequences of the direct changes in utility of GDL laws on one activity to the *other activities*, i.e., of GDL^0, GDL^B, and GDL^{Γ} on working, or GDL^0, GDL^A and GDL^{Γ} on schooling decisions.[47] While total effects are invariant to the choice of $\tilde{\gamma}^0$, direct and indirect effects are not.

6.2 Identification and Estimation

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

Assumption 1 (Bivariate Normal Idiosyncratic Preferences). *Idiosyncratic preferences are 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). The scale of one parameter must be fixed because utility is scaleless; we set $V(e_i^A) = 1$.[48] Normality is not necessary; for example, we could instead use a finite number of discrete points to approximate any bivariate distribution at little computational cost (Train 2008). However, the parsimony of joint normality facilitates interpretation and discussion.[49]

We adapt the same policy variation used in Section 4 and 5 to identify policy parameters in the structural model. There are two challenges to address. First, Γ and ρ both

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

[48]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.

[49]As an alternative to normality, Berry, Levinsohn, and Pakes (1995) use i.i.d. logit errors with random coefficients and also flexibly capture substitution patterns. However, incorporating multiple discreteness and complementarity into such models is challenging, and interpreting coefficients can be tedious.

reflect how often teens choose work and school together and are not separately identified without an exclusion restriction.

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,*

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

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

Assumption 2 primarily serves to separately identify Γ and ρ. A large fraction of the population choosing activities A and B together 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). Additionally, while the parameters of multinomial probit models are generally identified, identification is typically weak in the absence of exclusion restrictions like those in Assumption 2 (Keane 1992).

The π must be excludable and at least one of the π should be non-zero. We include state unemployment rate and log real minimum wage in the indirect utility of employment, and exclude them from the indirect utility of schooling. There is a substantial literature suggesting that these two variables matter for teen employment outcomes.[50]

Assumption 2 requires that these two factors do not have a direct effect on schoolgoing, implying that they only have indirect effects through employment decisions. 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. Similarly, we include our measure

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[50]Aaronson, Park, and Sullivan (2006) use CPS data and state-level aggregate unemployment rates to show that teen labor force participation is pro-cyclical. For example, in the early 1990s, unemployment rates rose and teen LFP declined. Aaronson, Park, and Sullivan (2006) estimate that, had the unemployment rate remained at the natural rate, teen LFP would have instead risen by 1–2pp during this time period. As for the minimum wage, several studies suggest that increasing minimum wages drives decreases in the extensive margin of teen employment (Neumark and Wascher 1992; 1995; Zavodny 2000; Sen, Rybczynski, and Van De Waal 2011). In response to this, several papers argue that more carefully controlling for local employment conditions attenuates this effect (Allegretto, Dube, and Reich 2011; Giuliano 2013). The inclusion of state-level minimum wage is more similar to the former, as we do not observe location in fine detail.

of whether dropping out is permitted (CS_{st}) and its interaction with GDL laws only in the indirect utility of schooling equation and assume that they do not have a direct effect on work. Prior research finds significant effects of compulsory schooling on teen schooling outcomes, suggesting that this instrument is relevant (Oreopoulos 2009; Anderson 2014). This assumption requires that compulsory schooling laws impact teen employment only through their effects on schooling decisions.

A second challenge to identifying policy parameters in the model is that fixed effects create statistical and practical challenges for estimation in this non-linear setting. Statistically, including fixed effects induces an incidental parameters problem. Estimates of unit (i.e., state) fixed effects are inconsistent with a fixed number of time periods. Because the fixed effects are not separable in the likelihood, inconsistency of fixed effects propagates to other parameters, including γ^{k+} (Lancaster 2000). Practically, adding many fixed effects greatly increases the computational cost and can generate 'flat' areas of the likelihood that strand maximization procedures away from optima.

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.

Assumption 3 imposes a correlated random effects (CRE) structure on the model. Econometrically, CRE control for some degree of 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 for fixed effects, as in Sections 4 and 5. In fact, Mundlak (1978) shows that CRE and fixed effects models are algebraically identical in linear settings.[51] See Papke and Wooldridge (2008) for an application of CRE and Wooldridge (2019) for a recent review.

Assumptions 1–3 are sufficient to identify all model parameters except $\tilde{\gamma}_0$. This parameter cannot be identified using the data on choice probabilities alone because discrete

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[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. However, we prefer correlated random effects in the structural model because (i) the model requires twice the number of fixed effects as Equation 1 (one for each activity) and (ii) the likelihood of the structural model is no longer concave, increasing the practical risk that estimation will not find the optimum.

choice models identify only *relative differences* in utility.[52] However, we achieve set identification of $\tilde{\gamma}_0$ with an additional assumption on the sign and relative size of the direct effect of GDL laws on each activity:

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

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

Each component of Assumption 4 is independently reasonable. Direct effects are likely 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.

6.3   Model Results

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). We make use of **Lemma 1** (see Appendix E), which asserts that, under Assumption 1, the model given by Equations (7)–(10) can be estimated with a Geweke, Hajivassiliou, and Keane (GHK) simulator. Appendix E contains additional estimation details.

Table 7 shows estimates of ten key model parameters.[53] Non-policy parameters of particular interest are the correlation of idiosyncratic preferences for school and work, ρ, and the complementarity between activities, Γ. The negative estimate of ρ (-0.48) indicates negative correlation in the 'types' of teens that choose school or work. Those who receive a high (utility) value from school are more likely to receive low value from work. Conversely, those receiving the highest utility from work are less likely to find school valuable. However, the small, positive estimate of Γ indicates that school and work are

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[52]This is why the utility of one choice is typically normalized to zero (e.g., $V_i(0,0) = 0$).

[53]Table E.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.

Table 7: 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.

weak complements: decreasing access to school mildly decreases the value of work (and vice versa). This is a key piece of evidence that the decline in employment and increase in schoolgoing in response to GDL laws do not primarily reflect substitution between those two activities. 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 than it actually is.

These results both confirm and contrast previous findings. Eckstein and Wolpin (1999) and Montmarquette, Viennot-Briot, and Dagenais (2007) both find evidence of negative correlation in preferences for school and work ($\rho < 0$), although the latter paper also shows that adding 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 substitutability decreases with age. However, Montmarquette, Viennot-Briot, and Dagenais (2007) find evidence that school and work are complementary for high-achieving high-school seniors. This finding is supported by Ruhm (1997), who shows that part-time work has no negative effect on educational outcomes.[54] Relative to this literature, we separately identify 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.

The policy parameters (γ and π) are qualitatively consistent with results in Section 4 and Section 5. The utility effect of GDL laws on teen labor force participation is larger than the corresponding effect on high school enrollment, both in absolute levels and in terms of standard deviations of idiosyncratic preference ($|\gamma^A| > |\frac{\gamma^B}{\sigma}|$). 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

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

(through GDL laws) partially reverses that reduction in relative utility. Moreover, all four π are significantly different from zero. This suggests that, in conjunction with Assumption 2, they are contributing identifying variation to the likelihood. 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.

We show model-based equivalents of the design-based treatment effects estimated in Sections 4 and 5 as **total effects** in the top row of Table 8. The model predicts that adopting a GDL law when school-leaving is legal increases the probability of being enrolled in school by 1.07pp and decreases the probability of labor force participation by 0.81pp.[55] These results are roughly in line with those in prior sections, though the magnitudes differ a bit. This is to be expected, as the model incorporates additional information by modeling the entire decision space, while also imposing additional structure via the correlated preferences and exclusion restrictions. The model suggests that GDL policies reduce the likelihood of the neither-work-nor-school option by about -0.86pp, or about 36% from the mean.[56] We interpret this neither option as reflecting teen preferences for leisure activities, which encompass 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 8 also shows the decomposition of each total effect into direct and indirect channels for $\tilde{\gamma}_0$ at the upper and lower boundaries of $\mathcal{G}$ (see Assumption 4). We further decompose the indirect effects for work and school into their root causes in italics: changes in the indirect utility of neither-work-nor-school or changes in the indirect utility of the other activity and the complementarity between the two activities.

Panel A of Table 8 shows the decomposition assuming $\tilde{\gamma}^0 = \sup \mathcal{G} = -0.00356$. In this scenario, the direct effect of GDL laws on the utility of schoolgoing is restricted to be 0, so the total effect must come entirely from the indirect channels. The decomposition in this scenario shows that only 15% of the total schooling effect is due to changing work access or complementarity effects. The effect of GDL laws on schoolgoing is almost entirely due to a reduction in the utility of neither-work-nor-school. In contrast, the total effect of

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[55]Counterfactuals impose the interacted difference-in-differences design and estimate effects assuming teens have the option to drop out.

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

Table 8: Decomposition of GDL Law Effects by Activity

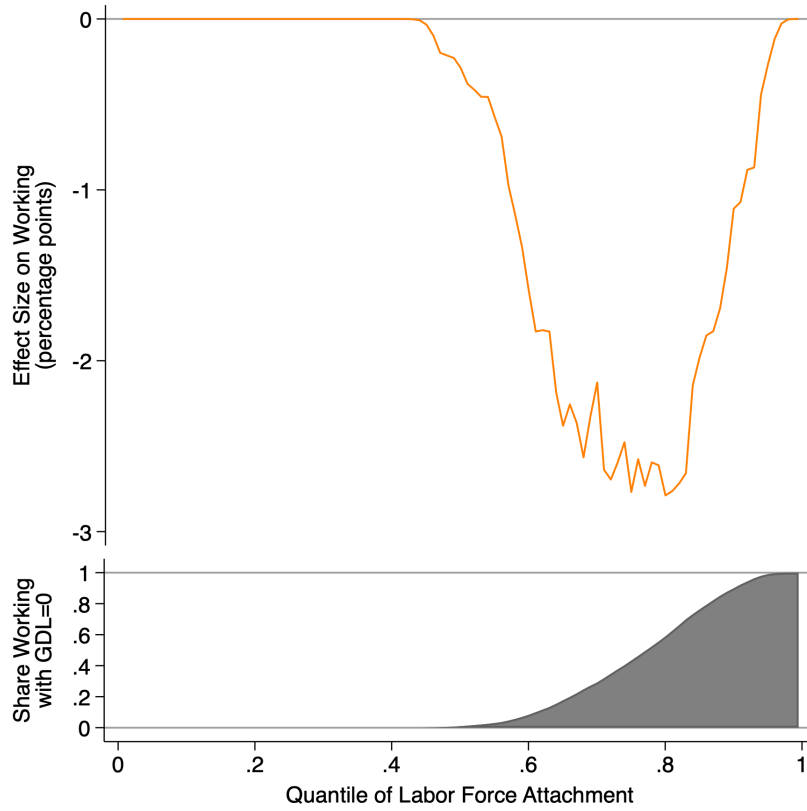| | Effect of GDL Laws on: | | | | | |
| | Neither | | Work | | School | |
	Effect	% of Total	Effect	% of Total	Effect	% of Total
Total effect	-0.86pp		-0.81pp		1.07pp	
A. Upper-bound renormalization $\tilde{\gamma}^0 = \min\{0, -\gamma^A, -(\gamma^B + \pi^B_{CS \times GDL})\}$.						
Direct	-0.91pp	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 7 averaged over 100 draws of $\mathbf{e}_i$ per person. To match the interacted difference-in-differences design, for all counterfactuals $CS_{st} = 1$ (and so $GDL^B_{st} \times CS_{st} = GDL^B_{st}$). Observations are weighted using sample weights.

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 8 instead assumes $\tilde{\gamma}^0 = \inf \mathcal{G} = -0.00554$. In this scenario, the direct effects of GDL laws on labor force participation and school enrollment are, by assumption, exactly equal. At this lower bound assumption, the impact of GDL laws on the utility of schoolgoing generates a direct effect of -0.92pp, but this is counteracted by a large indirect effect, again predominately due to the reduction in the utility of neither-work-nor-school. A much smaller portion (41%) of the total effect is due to the indirect channel stemming from reduced access to work and the declining complementarity between work and school. The decomposition of the total effect of GDL laws on teen employment is 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.

In summary, the reduced access to employment created by GDL laws can 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 $\tilde{\gamma}^0$ as being closer to the scenario

Figure 6: Heterogeneous GDL Effects on Employment

in Panel A of Table 8 than that of Panel B (which assumes negative direct effects on schooling of equivalent size to those on work). At this upper end, employment effects account for only 15% of the total impact of GDL laws on enrollment. At the less plausible lower end, employment effects account for 41% of the total impact of GDL laws on enrollment. In either scenario the majority of the total effect of the GDL policy on schoolgoing is attributable to the reduced utility from the neither-work-nor-school option.

We can also use the model to understand heterogeneity in the impacts of the policy. Figure 6 shows the effect of GDL laws on 16-year-old employment conditional on the quantile of e_i^A, which we interpret as the teen's underlying labor force attachment. There is no effect on those below the 43th percentile of e_i^A; they are inframarginal with respect to the GDL policy and employment. However, there are substantial disemployment effects of up to 2.8pp for 16-year-olds between the 60th and 80th percentile of e_i^A. Among this group, roughly half are in the labor force absent GDL laws (as indicated by the bottom panel of Figure 6). This suggests that GDL effects on employment are concentrated

44

among teens who are otherwise relatively inclined to work. Moreover, the teens whose employment decisions are most impacted by GDLs (at the 80th percentile of e^A) are, on average, at the 34th percentile of schoolgoing attachment, which is well away from the dropout threshold.[57] That is, the employment effects are concentrated on teens who are inframarginal regarding schoolgoing.

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. In contrast, policies aimed at increasing or decreasing teen employment are unlikely to have large unintended effects on educational attainment.

7 Conclusion

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

A robust set of results indicate that GDL laws—which restrict teen mobility—actually decrease high school dropout by about 1.15pp (a 30% reduction from the mean), but only in settings in which school-leaving is a legal option. This potentially surprising result contrasts with evidence from large, middle-income cities that transit expansions increasing school access improve educational outcomes (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 substitu-

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[57]That is, $F^{-1}(\mathbb{E}[e^B|e^A : F^{-1}(e^A) = 0.8]) = 0.344$.

tion towards schooling. To this end, we estimate the effect of GDL laws on teen labor force participation and find that these laws led to a 1.76pp (7.5% at the mean) reduction in 16-year-old labor force participation.

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

Teen mobility restrictions offer a classic economic example of trade-offs in policy design. While the motivation for GDL laws was to increase teen safety, they had a number of other effects on teen behavior. We find an additional benefit on schoolgoing, 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 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.[58] 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:

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

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

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

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

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

Table A.1: Effect of Minimum Driving Age on Fatal Car Accidents with 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 A.1 indicate that the introduction of GDL laws significantly restricted teen driving de facto. In column (3) we also include two leads and two lags of the minimum driving age indicator variable as a test for whether we are merely picking up trends in teen driving behavior. We find no evidence of pre-trends, however, there does seem to be a 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.[59] When taken in conjunction with our results, it appears that GDL laws did, in fact, restrict teen mobility.

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[59]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.

B Additional Tables and Results

Table B.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 B.2: Summary Statistics Across Interacted Treatment Groups

	Always-Treated States $GDL_{st} = 1\,\forall t$	Early-Adopter States (before 2000)		Late-Adopter States (2000 or later)		Never-Treated States $GDL_{st} = 0\,\forall t$
		when GDL was adopted:				
		$CS_{st} = 0$	$CS_{st} = 1$	$CS_{st} = 0$	$CS_{st} = 1$	
Panel A: Not In School = 1						
1990 to 1994	0.035	0.033	0.042	0.026	0.037	0.038
1995 to 1999	0.032	0.023	0.041	0.041	0.047	0.043
2000 to 2004	0.034	0.025	0.036	0.031	0.041	0.029
2005 to 2009	0.035	0.028	0.030	0.037	0.033	0.042
2010 to 2017	0.043	0.050	0.047	0.048	0.048	0.055
Panel B: In Labor Force = 1						
1990 to 1994	0.270	0.264	0.340	0.319	0.330	0.444
1995 to 1999	0.296	0.246	0.378	0.315	0.350	0.427
2000 to 2004	0.244	0.234	0.298	0.282	0.306	0.396
2005 to 2009	0.210	0.177	0.218	0.218	0.258	0.318
2010 to 2017	0.141	0.112	0.151	0.150	0.182	0.253
States	7	4	13	15	8	4

Table B.3: The Effect of Minimum Unrestricted Driving Age on 16-Year-Old Dropout (Linear Probability Model)

	Not In School = 1			
	(1)	(2)	(3)	(4)
Min. Unres. Driving Age >16 (β_1)	0.0019	0.0009	0.0011	0.0023
	(0.0041)	(0.0038)	(0.0039)	(0.0049)
School-Leaving Age $\leq$ 16 (β_2)	0.0207***	0.0195***		
	(0.0049)	(0.0047)		
Min. Unres. Driving Age >16	-0.0119**	-0.0110**	-0.0110**	-0.0165**
$\times$ School-Leaving Age $\leq$ 16 (β_3)	(0.0045)	(0.0047)	(0.0052)	(0.0065)
Effect of GDL if -0.0173**	-0.0101**	-0.0101**	-0.0099**	-0.0142*
School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	(0.0045)	(0.0047)	(0.0049)	(0.0073)
School-Leaving Age	As Observed		Fixed in Yr. of GDL Change	Never Switchers Only
Controls	-	Y	Y	Y
Obs	75,196	75,196	75,196	46,567

Results from two-way fixed effects regression using CPS ASEC data from 1990–2017. These OLS estimates replicate the probit estimates of Equation 1 shown in Table 2. All specifications include state and year fixed effects. Controls in columns (2)–(4) 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. Column (3) also includes indicators for the state minimum legal dropout age. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

Table B.4: The Effect of Minimum Unrestricted Driving Age on 16-Year-Old Labor Force Participation (Linear Probability Model)

	In Labor Force = 1			
	(1)	(2)	(3)	(4)
Min. Unres. Driving Age >16 (β_1)	-0.0048	-0.0046	0.0014	-0.0171
	(0.0119)	(0.0127)	(0.0125)	(0.0134)
School-Leaving Age $\leq$ 16 (β_2)	0.0329**	0.0284*		
	(0.0146)	(0.0149)		
Min. Unres. Driving Age >16	-0.0174	-0.0181	-0.0316**	-0.0168
$\times$ School-Leaving Age $\leq$ 16 (β_3)	(0.0130)	(0.0134)	(0.0133)	(0.0169)
Marginal Effect of GDL if	-0.0222**	-0.0227**	-0.0302***	-0.0340**
School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	(0.0109)	(0.0108)	(0.0111)	(0.0151)
School-Leaving Age	As Observed		Fixed in Yr. of GDL Change	Never Switchers Only
Controls	-	Y	Y	Y
Obs	75,196	75,196	75,196	46,567

Results from two-way fixed effects regression using CPS ASEC data from 1990–2017. These OLS estimates replicate the probit estimates shown in Table 6. All specifications include state and year fixed effects. Controls in columns (2)–(4) 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. Column (3) also includes indicators for the state minimum legal dropout age. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01
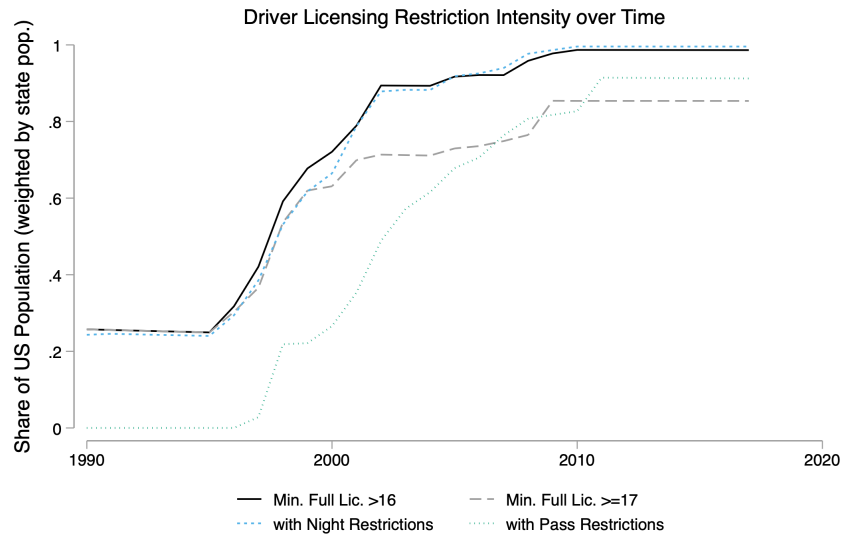
Table B.5: The Effect of Minimum Unrestricted Driving Age on Full-Time and Part-Time Employment

| | Usually & Seeking | | Actual | | |
	Full Time (1)	Part Time (2)	Full Time (3)	Part Time (4)	Other (5)
Min. Unres. Driving Age > 16 (β_1)	-0.0051**	0.0014	-0.0038*	0.0077	-0.0077
	(0.0023)	(0.0091)	(0.0018)	(0.0070)	(0.0061)
School-Leaving Age $\leq$ 16 (β_2)	0.0123***	0.0108	0.0073***	0.0170	-0.0034
	(0.0033)	(0.0148)	(0.0026)	(0.0127)	(0.0101)
Min. Unres. Driving Age > 16	-0.085***	-0.0056	-0.0053**	-0.0091	0.0015
$\times$ School-Leaving Age $\leq$ 16 (β_3)	(0.0032)	(0.0124)	(0.0027)	(0.0106)	0.0075
Effect of GDL if	-0.0136***	-0.0041	-0.0091***	-0.0013	-0.0062
School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	(0.0037)	(0.0092)	(0.0031)	(0.0088)	(0.0062)
Mean of Outcome	1.6%	21.7%	1.0%	16.6%	5.7%
Obs	75,196	75,196	75,196	75,196	75,196

Average marginal effects from probit regression using CPS ASEC data from 1990–2017 corresponding to estimates of Equation 6 with indicators of types of employment as defined in the CPS in place of LFP_{ist}. All specifications include state and year fixed effects. "Usually & Seeking" refers to usual work and, if unemployed, desired work. "Actual" refers to actual time worked (and "Other" includes unemployed). Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

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

(a) Graduated Driver Licensing Adoption

Driver Licensing Restriction Intensity over Time

Min. Full Lic. >16   Min. Full Lic. >=17
with Night Restrictions   with Pass Restrictions

(b) Minimum Legal School-Leaving Age

Minimum School-Leaving Age over Time

Min. School-Leaving Age <=16   Min. School-Leaving Age =17
Min. School-Leaving Age >=18
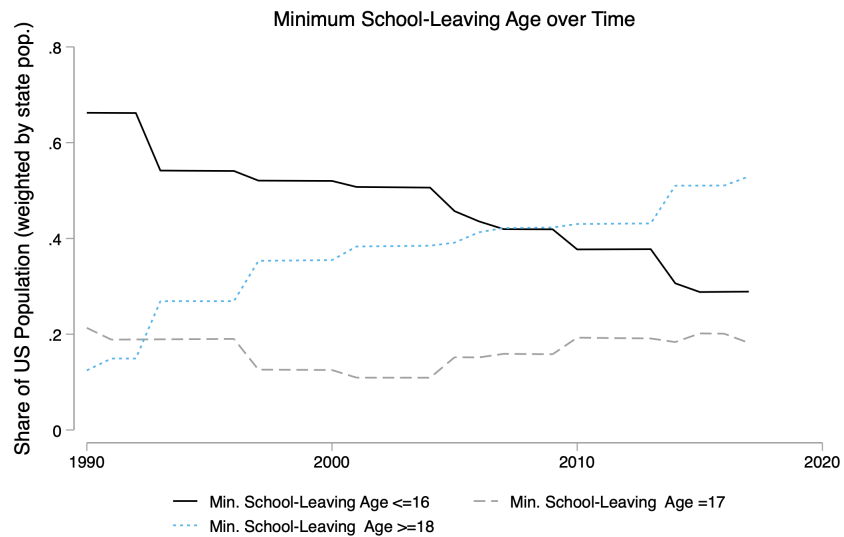
C   Robustness Analyses

C.1   Difference-in-Differences Analyses

We employ a difference-in-differences approach and estimate a model similar to Equation 2 that aggregates pre- and post-treatment years. This strategy provides more power to detect average effects. We estimate the following separately for the "Dropout Always Legal" and "Placebo/Dropout Never Legal" state sub-samples:

$$NotInSchool_{ist} = \beta GDL_{st} + X_i'\nu + Z_{st}'\mu + D_s + D_t + \epsilon_{ist}, \tag{C.1}$$

where all variables are defined as in Equation 1.

Table C.1: The Effect of Minimum Unrestricted Driving Age on 16-Year-Old Dropout, Treatment and Placebo Difference-in-Differences

	Not In School = 1							
	Treatment States				Placebo States			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Min. Unres. Driving Age >16 (β)	-0.0175***	-0.0192**	-0.0179***	-0.0160	0.0087	-0.0016	0.0082	-0.0007
	(0.0059)	(0.0078)	(0.0069)	(0.0101)	(0.0074)	(0.0055)	(0.0064)	(0.0065)
Estimator	Probit	Imputation	Probit	Imputation	Probit	Imputation	Probit	Imputation
Controls	-	-	Y	Y	-	-	Y	Y
School-Leaving Age	Always ≤16	Always ≤16	Always ≤16	Always ≤16	Always >16	Always >16	Always >16	Always >16
Obs	22,269	15,035	22,269	15,035	24,298	13,325	24,298	13,325

Estimates using CPS ASEC data from 1990–2017 limited to states that never changed school-leaving age. All specifications include state and year fixed effects. Controls in columns (3)–(4) and (7)–(8) include: 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)–(4) further restrict the sample to states where the school-leaving age is always ≤16, while columns (5)–(8) include only states where the school-leaving age is always >16. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

Table C.1 reports the difference-in-differences estimates corresponding to Equation C.1. Odd-numbered columns show the average marginal effects from a probit estimator, while the even-numbered columns show estimates using the BJS imputation method. In columns (1)–(4), estimates of β from Equation C.1 point to a negative effect of GDL laws on dropout behavior for 16-year-olds in states where dropout is legally permitted. In columns (1)–(2), we estimate the model excluding control variables (X_i and Z_{st}). The probit estimate from the model with all covariates included (column 3) reveals that increasing the minimum driving age in states where 16-year-olds can legally drop out reduces the probability that these teens are no longer in school by approximately 1.79pp, a 45.6% reduction from the mean. This is consistent with the average magnitude of the estimates shown in Figure 2.

The imputation point estimates are very similar in magnitude, but use a smaller sample and are thus somewhat noisier. In the model with all covariates, this estimate (-1.60pp) is therefore not statistically significant at traditional thresholds (a p-value of 0.112). Samples sizes are smaller in columns (2) and (4) because the imputation method drops "always-treated" states (those that adopted a GDL law before 1990). Note that the samples in all columns of Table C.1 are also limited to those 33 states that did not change their minimum school-leaving age between 1990 and 2017.

Estimates in columns (5)–(8) show the results of the placebo test. These estimates are quantitatively small and statistically insignificant in all four specifications, indicating that there is no discernible effect of GDL laws on 16-year-old dropout behavior in states where the minimum school-leaving age is binding.

C.2   Robustness of Interacted Difference-in-Differences

In this section, we address in more detail the literature that has identified biases in two-way fixed effects estimation of staggered adoption difference-in-differences research designs (e.g., de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021). While several alternative estimators exist for the standard difference-in-differences model with staggered adoption treatment, none (thus far) fits our setting of repeated cross-sectional data with a placebo-style interacted difference-in-differences design where policy interactions can "turn on" and then "turn off" again. We therefore provide a battery of tests to show that our main estimates do not suffer from the biases identified in the new difference-in-differences literature.

One source of such bias highlighted by Goodman-Bacon (2021) is that the two-way fixed effects estimator for a difference-in-differences identification strategy implicitly uses previously treated cohorts to estimate counterfactual outcomes for later-treated cohorts. This can be problematic if treatment effects are changing over time. Given that our data covers 28 years and that there are changes in GDL laws over several years, we are able to consider subsets of the full study window in order to probe whether our estimated treatment effects are dynamic or static.

Specifically, we introduce two sample restrictions into the estimation of the probit model in Equation 1. First, we remove states that are "always treated" in our study window (i.e., adopted a GDL law restricting full-privilege licenses to teens older than 16 prior to 1997).[60] This precludes long-run dynamic effects from early-adopter states from con-

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

taminating estimated effects. Second, we cut off the sample at earlier and earlier years, targeting the 1997–2002 window when most states adopted GDL laws.

Table C.2: The Effect of Min. Unrestricted Driving Age on Dropout for a Limited Panel

| | Not In School = 1 | | | | |
| | | Drop always-treated states & Limit sample to years: | | | |
	Full Sample (1)	1990-2017 (2)	1990-2012 (3)	1990-2007 (4)	1990-2002 (5)
Min. Unres. Driving Age >16 (β_1)	0.0014	-0.0009	0.0010	0.0026	0.0031
	(0.0040)	(0.0041)	(0.0032)	(0.0036)	(0.0070)
School-Leaving Age $\leq$ 16 (β_2)	0.0191***	0.0212***	0.0244***	0.0298***	0.0280***
	(0.0049)	(0.0054)	(0.0062)	(0.0068)	(0.0109)
Min. Unres. Driving Age >16	-0.0129**	-0.0142**	-0.0139***	-0.0154***	-0.0151*
$\times$ School-Leaving Age $\leq$ 16 (β_3)	(0.0052)	(0.0056)	(0.0051)	(0.0056)	(0.0086)
Effect of GDL	-0.0115**	-0.0151**	-0.0129**	-0.0128**	-0.0120*
if School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	(0.0052)	(0.0065)	(0.0059)	(0.0061)	(0.0070)
Exclude Always Treated	-	Y	Y	Y	Y
Obs	75,196	60,864	49,038	35,755	21,603

Average marginal effects from probit regression using CPS ASEC data. All specifications include state and year fixed effects. Controls include: gender; race/ethnicity indicators; mother's education; presence of father in household; receipt of SNAP benefits; state unemployment rate; 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

Table C.2 shows the results of these exercises. Column (1) replicates our preferred specification (column (2) of Table 2) to aid comparison. Columns (2)–(5) drop any states that are always treated during our study window (about 20% of observations). While column (2) uses data over the full study window, columns (3)–(5) respectively omit the five, ten, and fifteen most recent years of data. Results for all model estimates are relatively constant across specifications, though they become less precise as more data are omitted. The placebo effect (β_1) remains close to zero, whereas the interaction effect (β_3) and total effect of GDL laws where dropouts are legal ($\beta_1 + \beta_3$) both vary within relatively narrow bands. There is a bit more variation in the CS effect (β_2), but these estimates all suggest that, if anything, our primary estimates are conservative relative to other sample windows. The results in Table C.2 suggest that our main findings are not being driven by long-run dynamics in the effects of GDL laws.

1997.

In a second test of the dynamism of GDL law treatment effects, we estimate a model that includes indicators for bins of years in post-treatment time: 0–4, 5–9, 10–14, and 15+ years after GDL adoption. As before, we also drop always-treated units to avoid contamination from long-run effects. Table C.3 reports the results of this "grouped" triple-difference design. Estimates of β_1 are stable and close to zero, providing further placebo evidence that our research design and implementation identifies the effect of interest and is not overly subject to dynamic contamination. Moreover, the total effects of GDL laws in states without binding CS laws ($\beta_1 + \beta_3$) are fairly constant over time as well, further suggesting that our estimates are not biased by treatment effect dynamism.

In our final robustness check, we recast our research design into a more compatible framework to fit the imputation estimator of Borusyak, Jaravel, and Spiess (2021). We make three major changes from our preferred specification. First, we disallow an independent effect of GDL laws on schoolgoing when teens are not permitted to drop out (when school-leaving age is >16). That is, we recast our interacted difference-in-differences design as a more standard difference-in-differences design where the treatment is the interaction of restricted driving laws and non-binding compulsory schooling laws. Given the small, insignificant, and relatively precise estimates of β_1 throughout our analyses, we view this as a reasonable restriction on the estimation model.

Second, we assume a linear probability model. This is potentially consequential because our binary outcome variable has a mean that is very close to zero (only 3.8% of 16-year-olds drop out in our sample), a setting in which a linear probability model will usually generate biased and inconsistent estimates. However, comparing the linear probability model estimates in Table B.3 with the probit results in Table 2 suggests that this is reasonable.

Third, the newly developed estimators that account for treatment effect dynamics in a difference-in-differences model do not permit treatment to "turn on" and then "turn off" again. Therefore, we must omit some data from our sample to account for the fact that our interacted treatment ($GDL_{st} * CS_{st}$) both turns on and turns off over time. Specifically, in states for which the interacted treatment ever equals one (turns on), we drop all years of data after treatment then turns off. Figure 1a reveals that states are gradually adopting GDL laws, and Figure 1b shows that they are also gradually restricting the ability of 16-year-olds to drop out. This implies that the interaction of restricted GDL laws and un-restricted dropout legality typically comes into effect (turns on) for a period of time before being blocked (turns off) by more restrictive compulsory schooling laws. To illustrate, the

Table C.3: The Effect of Minimum Unrestricted Driving Age on 16-Year-Old Dropout Over Time

	Not In School = 1	
	Main Specification (1)	Effect Over Time (2)
Min. Unres. Driving Age >16 (β_1)	-0.0009 (0.0041)	
0-4 Yrs Post		0.0007 (0.0044)
5-9 Yrs Post		-0.0013 (0.0052)
10-14 Yrs Post		-0.0021 (0.0079)
15+ Yrs Post		0.0044 (0.0102)
School-Leaving Age $\leq$ 16 (β_2)	0.0212*** (0.0054)	0.0254*** (0.0068)
Min. Unres. Driving Age >16 $\times$ School-Leaving Age $\leq$ 16 (β_3)	-0.0142** (0.0056)	
0-4 Yrs Post		-0.0157** (0.0065)
5-9 Yrs Post		-0.0068 (0.0046)
10-14 Yrs Post		-0.0126** (0.0059)
15+ Yrs Post		-0.0181** (0.0077)
Effect of GDL if School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	-0.0151** (0.0065)	
0-4 Yrs Post		-0.0150** (0.0061)
5-9 Yrs Post		-0.0081 (0.0061)
10-14 Yrs Post		-0.0147** (0.0058)
15+ Yrs Post		-0.0138 (0.0091)
Obs	60,864	60,864

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

Figure C.1: Prevalence of the "Interacted" Treatment over Time
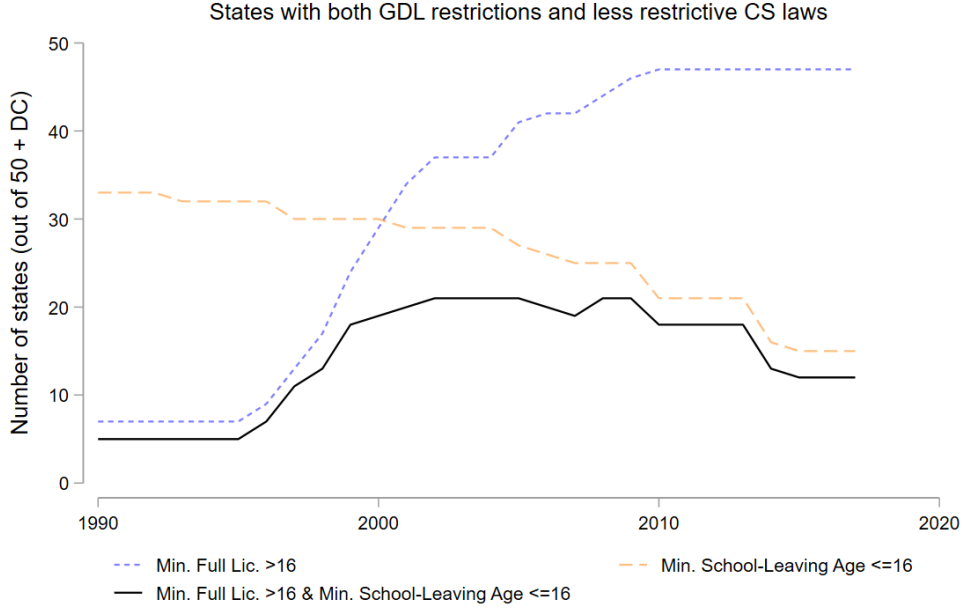
solid black line in Figure C.1 plots the number of states for which the interacted treatment is equal to one over time. Many states adopt GDL laws without restricting dropping out between 1995 and 2001, but the number of states with this interacted treatment begins to decline slowly through 2010 and more abruptly in 2013 and 2014.

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

$$NotInSchool_{ist} = \beta_2 CS_{st} + \sum_k \beta_{sk} 1[t - E_s = k]$$
$$+ X_i'\nu + Z_{st}'\mu + D_s + D_t + \epsilon_{ist}, \qquad \text{(C.2)}$$

wherein E_s is the first year that $GDL_{st} * CS_{st} = 1$ in state s and the β_{sk} are potentially heterogeneous and dynamic treatment effects that, when aggregated, correspond to β_3 in Equation 1.[61] If, as previously estimated, the true value of β_1 is zero, then estimates from Equation 1 and Equation C.2 should be very similar.

We estimate Equation C.2 using the BJS imputation estimator, which recovers a well-defined ATT even under arbitrary treatment-effect heterogeneity and dynamism. Columns

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[61]They also correspond to $\beta_1 + \beta_3$ when aggregated because β_1 here is assumed to be zero.

(5)–(7) of Table 2 in Section 4 show the results using the imputation estimator. The model in column (5) omits all controls except CS_{st}. Column (6) includes all control variables (X_i and Z_{st}). Column (7) omits never-treated units (all three columns omit always-treated units) to test whether our results hinge on comparisons to states that are subject to different trends than those that eventually adopt GDL laws. These estimates are nearly identical to the probit results in Table 2. The standard errors, which are conservative under treatment effect heterogeneity but exact if treatment effects are homogenous, are actually slightly smaller.[62] These results imply that our main results are robust to arbitrary treatment effect heterogeneity and dynamics.

D District-Level Dropout Analyses

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

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

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

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

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

Table D.1: Summary Statistics on School Districts

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

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

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 D.1 as a linear model and estimate standard errors clustered at the state level.

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

	Dropout Rate Grades 9-12	Dropout Rate Grades 9-12	Dropout Rate Grades 9-12	Dropout Rate Grade 9	Dropout Rate Grade 10	Dropout Rate Grade 11	Dropout Rate Grade 12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Min. Unres. Driving Age	-0.0043***						
	(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. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Column (1) of Table D.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 (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%.

E 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.

E.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.[63] 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 7–10) 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

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[63]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):

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

Straightforward substitution of data and coefficients for V_1, V_2, and Γ show equivalence to the primary model (Equations 7–10). 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:

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

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

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

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

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:

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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.

E.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 $\sigma, \rho, \Gamma, \gamma^{k+}$, 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 7 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.

E.3 Model Fit

Table E.1 assesses how well our estimated model explains the data by showing how often a simulated choice matches the observed choice (averaged over 100 draws of (e_i^A, e_i^B) for each individual). The model slightly overestimates the probabilities of choosing neither work nor school (0,0) and school only (0,1), while it slightly underestimates the probabilities for work only (1,0) and the both work and school choice (1,1). Overall, summing the diagonal components of Table E.1, the model correctly classifies those in the sample 62.23% of the time. Given the large number of individual characteristics that we do not observe, we believe this to be a reasonable approximation.

Table E.1: Model Fit

		True $\mathcal{P}^{(0,0)}$	True $\mathcal{P}^{(1,0)}$	True $\mathcal{P}^{(0,1)}$	True $\mathcal{P}^{(1,1)}$
	Totals	2.454%	1.329%	74.271%	21.946%
Model $\mathcal{P}^{(0,0)}$	2.472%	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%

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

E.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.[64] Specifically:

> *Neither activity effects*
> $\theta_{\text{Dir}}^{\varnothing} = \mathcal{Q}^{\varnothing}(1,0,0,0,\tilde{\gamma}^0) - \mathcal{Q}^{\varnothing}(0,0,0,0,\tilde{\gamma}^0)$ Direct effect on "neither" activity
> $\theta_{\text{Ind}}^{\varnothing} = \mathcal{Q}^{\varnothing}(1,1,1,1,\tilde{\gamma}^0) - \mathcal{Q}^{\varnothing}(1,0,0,0,\tilde{\gamma}^0)$ Indirect effect on "neither" activity
>
> *Employment effects*
> $\theta_{\text{Dir}}^{A} = \mathcal{Q}^{A}(0,1,0,0,\tilde{\gamma}^0) - \mathcal{Q}^{A}(0,0,0,0,\tilde{\gamma}^0)$ Direct effect on employment
> $\theta_{\text{Ind}}^{A} = \mathcal{Q}^{A}(1,1,1,1,\tilde{\gamma}^0) - \mathcal{Q}^{A}(0,1,0,0,\tilde{\gamma}^0)$ Indirect effect on employment
>
> *Schooling effects*
> $\theta_{\text{Dir}}^{B} = \mathcal{Q}^{B}(0,0,1,0,\tilde{\gamma}^0) - \mathcal{Q}^{B}(0,0,0,0,\tilde{\gamma}^0)$ Direct effect on schoolgoing
> $\theta_{\text{Ind}}^{B} = \mathcal{Q}^{B}(1,1,1,1,\tilde{\gamma}^0) - \mathcal{Q}^{B}(0,0,1,0,\tilde{\gamma}^0)$ Indirect effect on schoolgoing

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

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