Driving, Dropouts, and Drive-throughs: Mobility Restrictions and Teen Outcomes*

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Abstract

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We provide evidence that graduated driver licensing (GDL) laws, originally intended to improve public safety, impact both high school completion and teen employment. Many teens use automobiles to commute to both school and to part-time employment. Thus, the effects of automobile-specific mobility restrictions are ex ante ambiguous. Combining variation in the timing of both GDL law adoption and changes in compulsory school laws into a triple-difference estimation strategy shows that restricting teen mobility significantly reduces high school dropout rates and teen employment. These findings are consistent with a model in which teens use automobiles to access educational distractions (employment or even risky behaviors). We develop a discrete choice model that decomposes these effects into a direct component generated by changing educational access and an indirect component due to decreased access to alternative activities.

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1 Introduction

The automobile is an essential tool for mobility in the United States; more than three-quarters of adults commute to work by private vehicle. This is also true for teenagers; at least 76% of high school students report some driving and roughly half use an automobile to commute to school.[1] Yet driving is a particularly dangerous activity for teenagers. Drivers aged 16-19 in the United States are three times more likely to be in a fatal car accident than adult drivers, and motor vehicle accidents are the leading cause of death for teens.[2] But because cars are so important to mobility, laws that restrict teenage driving for safety reasons may lead to unintended and long-lasting consequences if mobility is important for teen schooling and employment decisions.

Mobility restrictions on teens may impact human capital accumulation through a number of channels, rendering the sign of its effect ex ante ambiguous. On the one hand, restricting teen driving might hinder students' ability to commute to school, reducing attendance and high school completion rates (particularly in rural areas or cities with underdeveloped transportation alternatives). Conversely, mobility restrictions might limit access to alternative activities (such as employment opportunities), which could increase school attendance and degree completion. This paper has two goals: (i) to carefully identify the consequences of teen mobility restrictions on educational attainment and (ii) develop and estimate an econometric framework that distinguishes the direct effect of mobility restrictions on school access from the indirect effect on access to alternative activities, namely labor force participation.

We utilize variation in the timing and stringency of a particular type of teenage mobility restriction, the graduated driver's license (GDL), to identify the effect of teen mobility restrictions on high school retention in multiple data sources. We also leverage variation in state-level changes to compulsory schooling (CS) laws to implement a triple-difference identification strategy. Using microdata from the Current Population Survey, we find that the GDL laws decrease the probability of high school dropout for 16-year-olds by 0.8pp in states where teens are not constrained by compulsory education laws (and can therefore opt out of high school), a 21% reduction at the mean.[3]

Making an activity more difficult to access should have a weakly negative effect on

[1]See Shults, Olsen, and Williams (2015) and Voas and Kelley-Baker (2008).

[2]From the US Department of Transportation's Fatality Analysis Reporting System (`http://www.nhtsa.gov/FARS`) and NCHS/NVSS data.

[3]We also verify this effect in a separate analysis using the National Center for Educational Statistics' Common Core of Data.

participation, ceteris paribus. The surprising results that restricting mobility *improves* educational outcomes suggests that limiting teen driving also reduces access to other activities that substitute for secondary education. Accordingly, we apply our triple-difference research design to such an alternative outcome and find that GDL laws reduce teen labor force participation by 2.0pp. We also show that the most stringent GDL laws—those which completely disallow driving at age 16—do increase school-leaving by 1.2pp relative to GDL laws that impose less stringent driving restrictions.

We then turn to a structural discrete choice model to rationalize these findings. We adapt a model of multiple activity choice to policy evaluation with repeated cross-sectional data, and show that the model can be used to distinguish direct policy effects from other margins (such as substitution between activities). In the model, teens choose to participate in one, both, or neither of school and employment. School and employment can be substitutes, complements, or neither, and GDL laws differentially impact each activity. The model decomposes the total effect of GDL laws into direct effects (i.e., the schooling effect on schooling) and indirect effects (the employment effect on schooling). The estimated model reveals that very little of the schooling effect is due to changes in labor market access due to GDL laws, and instead likely reflects decreased access to activities besides work or school (leisure and/or risky behaviors).

This paper offers several contributions. A growing literature has documented the impacts of GDL law implementation. Several papers show that GDL laws change teen driving behavior and reduce traffic fatalities (Dee, Grabowski, and Morrisey 2005; Karaca-Mandic and Ridgeway 2010; Gilpin 2019; Moore and Morris 2021). Additional studies have shown the effect of GDL laws on other aspects of teen behavior: criminal participation (Deza and Litwok 2016), risky activities (Deza 2019), and youth employment (Argys, Mroz, and Pitts 2019). While adverse outcomes in each of these dimensions can be long-lived, the literature does not directly connect GDL laws to human capital formation. We provide the first evidence that GDL laws also affect educational outcomes, and thus connect to a broader literature linking changes in teenage mobility to educational outcomes (Asahi 2016; Dustan and Ngo 2018).

We also study a potentially important mechanism through which driving restrictions can impact educational attainment. Labor force participation is often very responsive to mobility and job access.[4] This second contribution adds to the existing literature investi-

[4]There is evidence that restrictions to mobility can impact the labor supply of non-teen groups. Amuedo-Dorantes, Arenas-Arroyo, and Sevilla (2018) show that undocumented women increase their labor supply in response to the availability of driver's licenses. Black, Kolesnikova, and Taylor (2014) show that long

gating the link between education and labor/leisure activities for teens. Anderson (2014) finds that increasing the minimum legal dropout age has a significant and negative effect on violent and property crime arrest rates for 16–18 year-olds. The evidence on the impact of working while in high school has been mixed: Eckstein and Wolpin (1999) show that teen employment hurts high school performance but Montmarquette, Viennot-Briot, and Dagenais (2007) provide evidence that working fewer than 15 hours per week while in school is not detrimental to academic success.[5]

Third, we develop a novel triple-difference identification strategy that interacts a second restriction on teen behavior with GDL laws: compulsory school attendance. Compulsory schooling laws make it very costly for teens to drop out of high school. We utilize differences in compulsory attendance ages to compare outcomes in states where GDL laws change for teens who are not required to stay in school with outcomes in states where GDL laws change but teens face a legal requirement to stay in school. The timing of changes to these two policies (GDL laws were adopted by many states in the late 1990s and then many states increased their minimum school-leaving ages in the early 2000s) provides a unique opportunity to observe the effects of teen mobility restrictions when students have the option to drop out of school. Moreover, we find a null effect of GDL laws on teens for whom attendance is compulsory, a placebo result that suggests the triple-difference strategy isolates exogenous policy variation.

Finally, we develop a structural framework for policy analysis wherein agents can choose multiple activities, each of which is potentially impacted by the policy.[6] The model isolates the effect of the policy on each activity; counterfactual simulations decompose their interactions. We identify the model by combining the triple-difference design with exclusion restrictions requiring certain covariates to influence either only schooling or work. Few papers combine quasi-experimental identification strategies with discrete choice models for policy evaluation (an exception is Li 2018).[7] Our results suggest that

average commuting times in large cities suppress female labor force participation in the US.

[5] Argys, Mroz, and Pitts (2019) study the effects of graduated driver licensing on employment as a potential explanation for the secular decline in US teen employment. We differ in our focus on schooling and use of a different research design, rich CPS ASEC data, and a longer sample window. We also construct and estimate a model of interactions between schooling and labor force participation.

[6] The model is a descendent of the multiple product choice model of Gentzkow (2007), adopted for policy analysis with repeated cross-sectional data.

[7] There is an extensive literature that applies structural modeling to human capital accumulation. Whereas much of that literature has at its heart dynamic considerations, our approach instead grows out of product choice models from industrial organization (e.g., Berry, Levinsohn, and Pakes 1995; Goolsbee and Petrin 2004). Often, structural approaches analyzing policy discard features of reduced-form models that aid credible identification of causal effects (e.g., difference-in-differences-type comparisons, unit fixed

this may be a fruitful path for continued research.

We briefly describe background and context for our study and detail data sources in Section 2, while Section 3 provides a "first stage" test of the effects of GDL laws on a proxy for teen driving: teen auto fatalities. Section 4 and Section 5 describe our triple-difference identification strategy and our main results on education outcomes and Section 6 investigates substitution with employment as a possible mechanism. Section 7 develops the structural model and differentiates the various effects of GDL laws on education and teen employment.

2 Context and Data

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

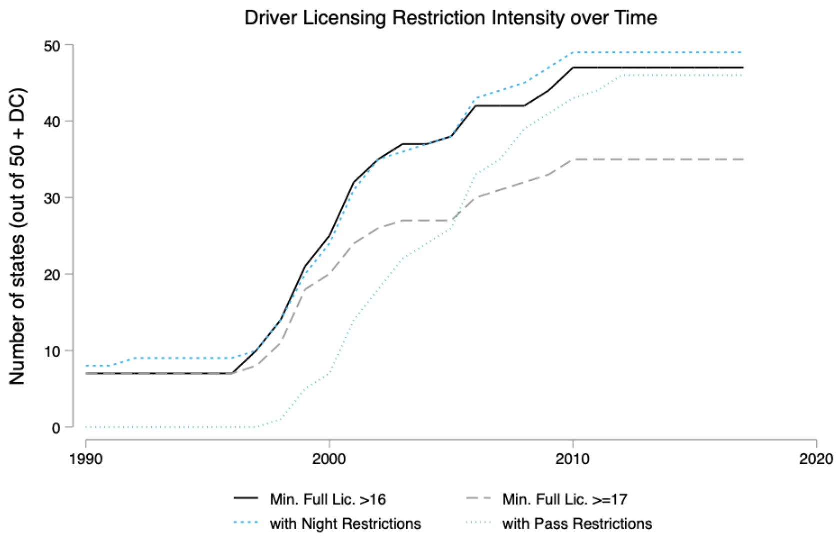
We develop a database of pertinent state-level GDL laws in the 50 states and DC from several sources, including the Federal Highway Administration's (FHWA) Highway Statistics and the Insurance Institute of Highway Safety (IIHS) covering the years 1990 to 2017.[8] Figure 1a shows counts of the number of states with various types of GDL laws over time. Prior to 1995, fewer than ten states limited full privilege licenses to those older than 16 or had nighttime driving restrictions. By 2010, forty-seven states had placed increased restrictions on teenage driving. Much of the adoption of GDL laws occurred between 1996 and 2003.

In order to identify the effect of the GDL laws on teen educational attainment de-

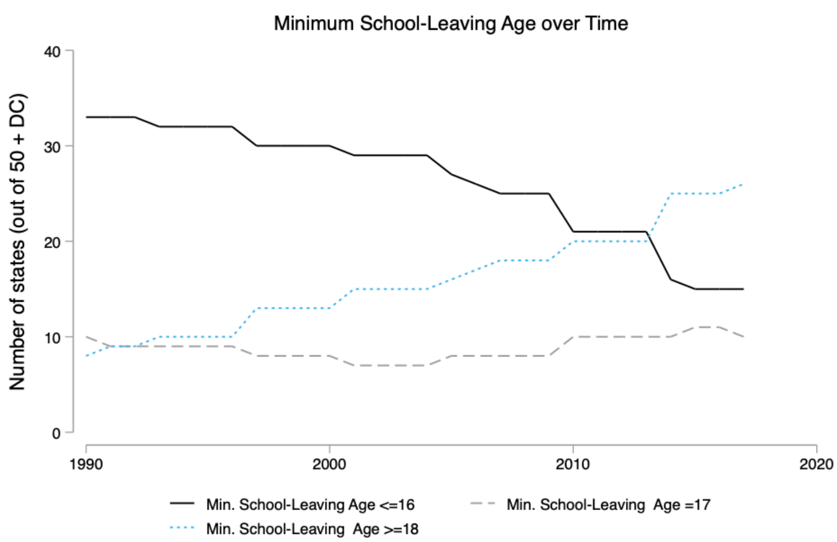
effects, etc.) to reduce computational complexity. Our model retains a primary focus on identifying policy parameters without sacrificing our ability to gain insight through additional structure.

[8]IIHS data begin coverage in 1995. We use FHWA data for the years before 1995, and rectify and conflicts between the two datasets. The GDL data is similar to that used in Severen and Benthem (2019).

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



(a) Graduate Driver Licensing Adoption



(b) Minimum Legal School-Leaving Age

cisions, we also utilize variation in the state-specific compulsory schooling (CS) laws. Specifically, we are interested in the mandated school-leaving age (the minimum age at which a teen is legally allowed to drop out of school). We extend the school-leaving age data from Anderson (2014) (which covers 1980–2008) up to 2017. For 2009–2011, 2013–2015, and 2017, we draw on the National Center for Education Statistics' (NCES) State

Education Reforms tables and fill in the intervening years for states with no changes. For states with a change in the minimum school-leaving age, we verified the timing of the change in legal databases.[9] Figure 1b shows counts of the number of states with different minimum school-leaving ages from 1990 to 2017. Over this time period 25 states changed their minimum dropout 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).[10] 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.[11] 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 will also incorporate measurement error from those teens who have already completed a high school degree and choose not to enroll in college. CPS ASEC participants were also surveyed on labor force participation and employment status in the preceding week.

To construct our final estimation sample, we limit the linked GDL law, CS law, and CPS data to individuals aged 16 at the time of the ASEC survey. This serves dual purposes: (1) our measure of high school dropout, $NotInSchool_i$, should incorporate less noise for this age group as they are very unlikely to have already completed high school; (2) this is the age that is most impacted by the implementation of the GDL laws. Between 1990–2017, 40 states switched from allowing 16-year-old teens to obtain full driver's licenses to restricting this privilege to older teens.[12]

Finally, in order to control for the local economic environment, we link in data from the Bureau of Labor Statistics (BLS) on the monthly non-seasonally adjusted unemployment rates by state and data from the Federal Reserve Economic Data (FRED) on state minimum wages. We use the BLS data to construct a state-specific unemployment rate

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

[10] When linking these datasets, we assign a GDL law to a year if that law was in effect in January of that year. In Section 3, we verify that this approach is reasonable.

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

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

in each year as the three-month average unemployment rates centered around January.[13] From FRED, we take the maximum of the state and federal minimum wage in each year and inflation-adjust to measure the binding real minimum wage in each state-year.

Table 1 reports summary statistics for the final linked estimation sample. Among surveyed 16-year-olds, 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 (see Appendix Table C.1).

Table 1: Summary Statistics on Individuals Aged 16

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

Data 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.

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

3 GDL Laws & Teen Driving

To verify that GDL laws had a binding effect on teen automobile use, we first study the effect of GDL roll-out on a proxy for driving.[14] We use the rate of fatal car accidents involving a teen driver as a proxy for the prevalence of teen driving by linking the GDL laws to data from the US Department of Transportation's Fatality Analysis Reporting System (FARS). FARS is a nationwide census of all fatal injuries suffered in motor vehicle crashes and provides data on the location and timing of the accident as well as involved drivers' birthyears.

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:

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

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 2 shows that a one year increase in the minimum age at which teens can receive an unrestricted driver's license reduces the rate of fatal car accidents for drivers aged 16 by 0.04 accidents per thousand 16-year-olds in the (state's) population. At the mean (0.227 fatal accidents per thousand population aged 16), this is equivalent to a 17% reduction. In column (2), we replace the continuous measure of unrestricted driving

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

age with an indicator variable that equals one if the minimum unrestricted driving age is strictly greater than 16 (corresponding to the solid, black line in Figure 1a). This yields an even larger negative estimate of 0.08 accidents per thousand 16-year-old population, indicating that teens are significantly less likely to be involved in a fatal car accident when they cannot access an unrestricted driver's license.

Table 2: Effect of Minimum Driving Age on Fatal Car Accidents with Age 16 Drivers

	Accidents per 1,000		
	(1)	(2)	(3)
Minimum Unrestricted Driving Age	-0.040***		
	(0.012)		
Min. Unres. Driving Age > 16 (year t+2)			-0.008
			(0.015)
Min. Unres. Driving Age > 16 (year t+1)			-0.010
			(0.016)
Min. Unres. Driving Age > 16		-0.080***	-0.036***
		(0.017)	(0.013)
Min. Unres. Driving Age > 16 (year t-1)			-0.017
			(0.015)
Min. Unres. Driving Age > 16 (year t-2)			-0.015
			(0.014)
Mean Outcome		0.227	
Obs	1,500	1,500	1,300

All specifications include state and year fixed-effects and are weighted by the total state population. Standard errors are clustered at the state-level. * p<0.10, ** p<0.05, *** p<0.01

The results in Table 2 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 either pre-trends or of a delayed impact of the policy change on teen driving. This provides a measure of confidence that we are assigning changes in GDL laws to the correct year. In Appendix Table A.1, we provide detailed estimates of the different GDL ages in half-year increments on fatal accident involvement rates for each age from 15 to 18. The largest effects as a percentage of the mean outcome are for 16-year-olds.

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.[15] When taken in conjunction with our results, it appears that we are correctly assigning GDL laws to state-years and that these laws did, in fact, restrict teen mobility.

4 Empirical Strategy

Our primary analyses investigate the relationship between GDL law adoption and teen outcomes using a difference-in-difference-in-differences identification strategy. The first difference compares teen dropout behavior before and after the implementation of a GDL law. The second difference leverages the staggered roll-out of the GDL policies and compares teens across states that restricted teen driving in different years (or not at all). All 50 states have some form of compulsory schooling law in place, which disallows teens to drop out of high school education before reaching a certain age. These age thresholds vary considerably across states and over time (see Figure 1b). We compare teens from states where the minimum school-leaving age is 16 or lower to those where the school-leaving age is 17 or higher to comprise our third difference.

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

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

$$\begin{aligned} NotInSchool_{ist} = {} & \beta_1 GDL_{st} + \beta_2 CS_{st} + \beta_3 GDL_{st} * CS_{st} \\ & + X_i'\nu + Z_{st}'\mu + D_s + D_t + \epsilon_{ist}, \end{aligned} \tag{2}$$

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

[16] We consider deviations from static homogeneous treatment effects in Section 5.1 and Appendix B.

where GDL_{st} is an indicator variable that equals one if the minimum unrestricted driving age in state s in year t is > 16 (i.e. 16-year-olds experience mobility restrictions).[17] We capture compulsory schooling laws with CS_{st}, an indicator that equals 1 of 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 legal dropout age and the 3-month average unemployment rate. The model also includes both state fixed effects to control for time invariant confounding factors (such as persistent differences in school quality or returns to education across states) and year fixed effects to control for aggregate fluctuations (such as changes in national schooling laws over time).[18]

Because the effect of mobility restrictions on teen dropout behavior is ex ante ambiguous, we first consider the multiple channels through which GDL laws might impact teen educational attainment. In the absence of GDL, laws a 16-year-old teen faces a trade-off between schooling and labor/leisure. For illustration, consider a teen with 15 hours of waking time available per day. She can: (a) attend high school full-time (which takes up, e.g., 9 hours of the day) and use the remaining 6 hours of waking time to split between labor and leisure; or (b) she can drop out of high school and split the full 15 hours of her day between labor and leisure. In a simple static model of individual labor supply, choosing option (b) increases the teen's budget and corresponding opportunity set, which unambiguously leads to a higher level of contemporaneous utility. However, this comes at the cost of giving up the utility associated with schooling, which incorporates the expected increase in lifetime earnings from achieving a high school diploma.

When a state introduces a GDL law that restricts the teen's access to driving, this may have a *direct* effect on the dropout decision if the restriction hinders the teen's ability to commute to school. In particular, for low-income households or teens in rural areas with minimal access to alternative transportation this direct effect may lead to a significant increase in high school dropout rates. However, the mobility restrictions imposed by GDL laws may also impact the teen's dropout decision *indirectly* through an effect on access to labor and leisure activities. In fact, we know from previous studies that GDL laws decrease teen participation in risky behaviors and declining teen labor force participation

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

[18] All models are estimated using CPS ASEC person-level weights.

(Deza and Litwok 2016; Deza 2019; Argys, Mroz, and Pitts 2019).[19] Because teens face a time trade-off between school and labor/leisure, this indirect effect can lead to a significant decrease in high school dropout rates. The total or net effect of GDL laws on high school dropout rates will thus be positive if the direct effect dominates, negative if the indirect effect dominates, or zero if the two effects are roughly equal in magnitude.

This discussion has so far assumed that teens have the option to drop out of high school in response to changes in their mobility restrictions. This assumption will fail in states that impose compulsory schooling laws that make it illegal for younger teens to opt out of high school attendance. This creates a natural placebo test in state years where the school-leaving age is greater than the minimum age needed for an unrestricted driver's license. To the extent that the compulsory schooling laws are well enforced, these policies effectively shut down all effects of the GDL laws on dropout behavior. Thus, β_1 in Equation 2 will identify the "placebo" effect of imposing mobility restrictions on dropout behavior in states where 16-year-olds cannot legally dropout.

The coefficient β_2 in Equation 2 captures the impact of more lenient compulsory schooling laws (minimum school-leaving age is less than 17) on high school dropout behavior in the absence of GDL laws. We expect this coefficient to be large and positive. However, if CS laws are not well-enforced (or if they incorporate exemptions for teens who are working or have parental consent), then we may still observe an impact of the GDL laws on high school dropout (ie., $\beta_1 \neq 0$). In this case, an effect should only be observable if there are enough students who experience a direct or indirect effect of the mobility restriction that is large enough to incentivize law-breaking. For example, in rural areas where school buses are scarce and can require hours-long commutes, we might expect the direct effect of the GDL laws to be large enough to cause an increase in high school dropout, even in states where the compulsory schooling laws are binding.

Finally, the coefficient β_3 will capture the differential effect of increasing driving restrictions on the probability of dropping out between teens who are legally able to do so relative to teens who cannot dropout at age 16. Of particular interest is the sum of the two coefficients, $\beta_1 + \beta_3$, which captures the total effect of the changing GDL laws on those teens who are legally permitted to drop out of school. This sum identifies the total effect of GDL laws on teen dropout behavior and will be: positive if the direct effect on

[19] Huh and Reif (2021) do not study GDL laws specifically, but investigate the effect of teenage driving more generally on mortality and risky behaviors. They estimate that total mortality rises by 15% at the minimum legal driving age cutoff, driven by an increase in motor vehicle fatalities and poisoning deaths, which are caused primarily by drug overdoses.

access to school is larger than the indirect effects; negative if the indirect effect on access to labor/leisure is larger; or zero if the two effects are roughly equal in magnitude.

5 Results

We estimate the model in Equation 2 using a probit maximum likelihood estimator.[20] Standard errors are estimated allowing for clustering at the state level. Table 3 reports the corresponding marginal effects for each coefficient evaluated at the mean of all covariates.

In column (1), we estimate the model in Equation 2 excluding all control variables (X_i and Z_{st}). Column (2) presents our main specification, which includes all covariates. The estimates in these two columns demonstrate that our results are not sensitive to, or driven by the inclusion of covariates. Estimates of β_1 (our placebo test) are very small and are statistically insignificant, indicating that there is no discernible effect of GDL laws on 16-year-old dropout behavior in states where the compulsory schooling age is binding (17 or older). As expected, the estimates of β_2 are large and statistically significant, indicating that compulsory schooling laws are generally effective (i.e. the probability of a 16-year-old leaving high school is significantly larger in states where dropout is legally permitted at that age). Moreover, these estimates are quantitatively similar to those in previous studies that analyze the impacts of compulsory schooling laws (Anderson 2014; Oreopoulos 2009). As we use more recent years of data than those papers, this provides some evidence that compulsory schooling laws continue to be impactful for educational attainment.

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 (vs. those states where dropout is not legal) is negative and statistically significant. The total (or net) effect of GDL laws on teen dropout behavior is estimated by the sum of coefficients, $\beta_1 + \beta_3$. This sum reveals that increasing the minimum driving age in states where 16-year-olds can legally drop out reduces the probability that these teens are no longer in school by approximately 0.8pp, a 21% reduction from the mean. This negative estimate of the net effect indicates that, if there is any direct effect of the GDL laws on high school attendance (through increased difficulty in commuting to/from school), it is more than completely offset by the indirect

[20]Given that only 3.8% of 16-year-old teens are not in school (Table 1), a probit specification avoids the probable pitfall of predicting probabilities outside the unit interval. However, results estimated using a linear probability model are qualitatively and quantitatively similar and are shown in Appendix Table A.2.

effect of GDL laws through reduced access to labor and leisure activities.[21]

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

	Not In School = 1					
	Triple-Diff				Diff-in-Diff	
	(1)	(2)	(3)	(4)	(5)	(6)
Min. Unres. Driving Age >16 (β_1)	0.0018	0.0010	0.0038	0.0011	-0.0023	-0.0024
	(0.0044)	(0.0042)	(0.0051)	(0.0045)	(0.0036)	(0.0033)
School-Leaving Age ≤ 16 (β_2)	0.0176***	0.0162***				
	(0.0045)	(0.0044)				
Min. Unres. Driving Age >16	-0.0097**	-0.0088*	-0.0121*	-0.0084*		
$\times$ School-Leaving Age ≤ 16 (β_3)	(0.0047)	(0.0045)	(0.0062)	(0.0050)		
Effect of GDL if	-0.0080**	-0.0078*	-0.0083*	-0.0073*		
School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	(0.0041)	(0.0040)	(0.0047)	(0.0038)		
School-Leaving Age	As Observed		Never Switchers Only	Fixed in Yr. of GDL Change	-	-
Controls	-	Y	Y	Y	-	Y
Obs	75,196	75,196	46,567	75,196	75,196	75,196

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

5.1 Robustness and Alternative Estimators

A potential confounding factor in this triple-difference model is the fact that, along with GDL laws, compulsory schooling laws were also changing during this time. Between 1990 and 2017, about half of states increased the minimum legal dropout age (see Figure 1b). To avoid conflating effects from changes in this policy with the effects of the GDL laws, we employ two robustness checks. First, we estimate the model in Equation 2 on the subsample of states that did not change the minimum school-leaving age during the time

[21] We assign a GDL law to a year if that law was in effect in January of that year. The CPS ASEC survey is conducted in March of each year. Because we do not observe each teen's month of birth, this means that some teens with birth months between April and December may have turned 16 before the GDL law went into place. We account for this potential misclassification by dropping observations from the initial treatment year for each state and find that our results are largely unchanged.

period under study. The results of this estimation strategy are shown in column (3) of Table 3.[22] This specification yields slightly larger estimates of the net effect of GDL laws on teen dropout behavior and is consistent with the findings from our main specification.

Second, we replace the variable CS_{st} in Equation 2 with a time-invariant measure that is fixed at each state's minimum school-leaving age in the year that the GDL law first increases the minimum unrestricted driving age to over 16. For states where the minimum unrestricted driving age is either always less than or equal to 16 or always greater than 16, we use the minimum school-leaving age from the first year of the sample, 1990.[23] The results of this estimation strategy are shown in column (4) of Table 3. These estimates are nearly identical to the estimates from our main specification in column (2) and support our previous findings.

Columns (5)–(6) of Table 3 display the results of a simpler difference-in-differences model that excludes the interaction term, $\beta_3 GDL_{st} * CS_{st}$. These results are relatively small and insignificant. This is to be expected as these small, negative estimates represent a weighted average of the null GDL effects in states that have a high school-leaving age and the larger negative effects in states with less restrictive compulsory schooling laws. These results clearly demonstrate the advantage of estimating the more robust triple-difference model.

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

A number of solutions have been proposed to overcome this issue, however none have thus far been adapted to fit our setting of using repeated cross-sectional data with a placebo-style triple-difference design.[24] Moreover, this literature focuses exclusively on

[22] Note that in these alternate specifications, the coefficient β_2 will be absorbed by the state fixed effects.

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

[24] Chaisemartin and D'Haultfœuille (2020b) make some progress toward interacted designs, however, by studying difference-in-differences designs with multiple treatments.

linear models and as our outcome of interest is binary with a mean value close to zero, a linear probability model is potentially biased and inconsistent. Because of these aspects of our research design, there is no alternative estimation strategy that we can adopt wholesale from the current literature to address the challenges caused by heterogeneous and dynamic treatment effects. Nevertheless, we provide several exercises to test the robustness of our results to possible deviations from static, homogeneous treatment effects.

The first approach estimates a model similar to our preferred specification but considers subsets of the time variation used in the full analysis and allows for some dynamism in treatment effects. The results (shown in Table B.1 and Table B.2) provide evidence that our main results are not being driven by long-run dynamics in the treatment effects of GDL laws. The second approach recasts our research design as difference-in-differences (instead of triple-difference) and assumes a linear specification so that we can apply the imputation estimator detailed in Borusyak, Jaravel, and Spiess (2021). This estimator recovers a well-defined ATT even under arbitrary treatment-effect heterogeneity and dynamism. The results of this estimation strategy are shown in Table B.3 and are largely unchanged from our main results. We discuss both of these approaches to robust estimation in detail in Appendix B.

To further support our main findings from Table 3, we also analyze the impact of GDL laws on teen dropout decisions using school-district level data from the NCES Common Core of Data. The Common Core is a comprehensive national database of all public elementary and secondary schools and provides high school dropout rates aggregated at the school district-by-grade level. A primary advantage of this dataset is that, because it includes data by school district, we can include school district fixed effects to control for time-invariant differences between places within states; we discuss this data and analysis in detail in Appendix C.

We find that the implementation of GDL laws leads to a 0.38pp reduction in high school dropout rates in the NCES data, which is an 11% reduction at the mean. Furthermore, the effects of increasing the minimum driving age to over 16 are largest in the 10th and 11th grades (the grades in which students are most likely to be 16 years old and thus directly affected by GDL laws). These results confirm our main findings in Table 3 and provide compelling evidence that imposing restrictions on teen mobility maintains high school enrollment and leads to a significant reduction in high school dropouts.

As noted in Section 4, we might expect the direct effect of GDL laws on high school attendance, which functions through restricting teens' ability to commute to school, to differ across various subgroups. Specifically, it seems likely that teens from rural areas or from low-income backgrounds might experience larger direct effects. We investigate this possibility next.

5.2 Heterogeneity Analysis

We estimate our preferred specification, given by Equation 2, separately for several sub-populations of interest. The marginal effects estimates are shown in Table 4 and Figure 2. The top-left panel of Figure 2 shows the effects of GDL laws on 16-year-old dropout separately for males and females. The top three estimates show the effects of GDL laws in states where dropout is not legal (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 populations. While the estimates are somewhat noisier for females than males there are no statistically significant differences in the effects of GDL laws by sex.[25]

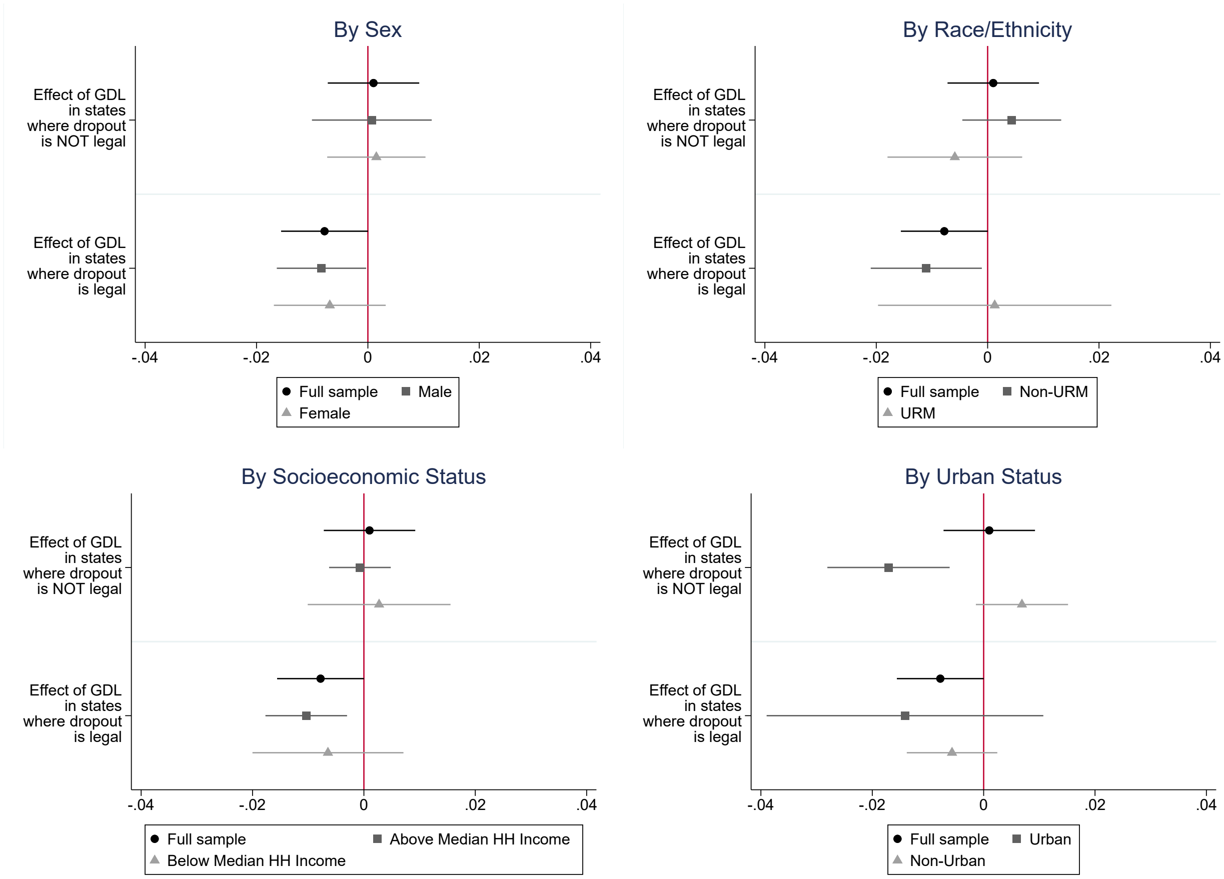
In the top-right panel of Figure 2 (and columns (4)–(5) of Table 4) are the effects of GDL laws estimated separately for underrepresented minorities (teens who identify as black, hispanic, or native american) and all other race/ethnicity groups. These estimates reveal that the negative impact of GDL laws on high school dropout is largely driven by non-URM teens. The estimates for URM teens are quite noisy and very close to zero.

In the bottom-left panel of Figure 2 (and columns (6)–(7) of Table 4), we separate the estimation sample into two halves based on household income (as reported in the CPS). The median household income is $53,236. The estimated effects of GDL laws are somewhat 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. However, the differences in the estimates across the lower-income and higher-income groups are not statistically significant.

Finally, the bottom-right panel of Figure 2 (and columns (8)–(9) of Table 4) show the effects of GDL laws estimated separately for teens living in urban and non-urban areas.

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

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



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

For teens in urban locations, the effects of GDL laws on high school dropout are negative and significant even when compulsory schooling laws make dropout illegal for the 16-year-olds in our sample. This suggests that enforcement of compulsory schooling laws may be lacking in urban areas.

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

	Not In School = 1								
	Full Sample	Men	Women	Non-URM	URM	HH Income $\geq$ Median	HH Income $<$ Median	Non-Urban	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Min. Unres. Driving Age >16 (β_1)	0.0010	0.0007	0.0015	0.0043	-0.0059	-0.0007	0.0027	0.0069	-0.0171***
	(0.0042)	(0.0055)	(0.0045)	(0.0045)	(0.0062)	(0.0028)	(0.0065)	(0.0042)	(0.0056)
School-Leaving Age $\leq$ 16 (β_2)	0.0162***	0.0190***	0.0134**	0.0174***	0.0178*	0.0131***	0.0193**	0.0152***	0.0228**
	(0.0044)	(0.0056)	(0.0056)	(0.0053)	(0.0096)	(0.0043)	(0.0094)	(0.0045)	(0.0098)
Min. Unres. Driving Age >16 $\times$ School-Leaving Age $\leq$ 16 (β_3)	-0.0088*	-0.0090	-0.0084	-0.0154***	0.0072	-0.0096***	-0.0092	-0.0126***	0.0030
	(0.0045)	(0.0056)	(0.0052)	(0.0054)	(0.0099)	(0.0037)	(0.0083)	(0.0043)	(0.0131)
Effect of GDL if School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	-0.0078*	-0.0083**	-0.0068	-0.0110**	0.0013	-0.0104***	-0.0065	-0.0057	-0.0141
	(0.0040)	(0.0041)	(0.0051)	(0.0051)	(0.0107)	(0.0037)	(0.0069)	(0.0041)	(0.0127)
Obs	75,196	38,587	36,609	52,641	22,441	37,598	37,598	59,227	15,897

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

6 Mechanism Analysis

We now turn to analyses of the mechanisms behind our main findings in Section 5, which show 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. This negative estimate of the net effect of GDL laws indicates that, if there is any direct effect of the GDL laws on commuting to high school, it is more than completely offset by the indirect effects stemming from reduced access to labor and leisure activities. We can further tease this apart using variation in the intensity of GDL laws across states and time, and by studying an alternative outcome: teen employment.

6.1 Variation in GDL Intensity

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

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

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

This specification is identical to Equation 2, except that we have replaced the single binary measure of GDL restrictions with two indicator variables corresponding to the two dif-

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

	Not In School = 1	
	(1)	(2)
GDL at 16:		
Intermediate License Only (β_1^A)	0.0039	0.0030
	(0.0045)	(0.0043)
No License (β_1^B)	0.0034	0.0038
	(0.0055)	(0.0054)
School-Leaving Age ≤ 16 (β_2)	0.0164***	0.0154***
	(0.0046)	(0.0045)
GDL at 16 $\times$ School-Leaving Age ≤ 16:		
Intermediate License Only (β_3^A)	-0.0104**	-0.0096**
	(0.0045)	(0.0044)
No License (β_3^B)	0.0035	0.0017
	(0.0057)	(0.0057)
Effect of Intermediate License Only if	-0.0066*	-0.0065*
School-Leaving Age $\leq$ 16 ($\beta_1^A + \beta_3^A$)	(0.0038)	(0.0038)
Effect of No License if	0.0069	0.0054
School-Leaving Age $\leq$ 16 ($\beta_1^B + \beta_3^B$)	(0.0073)	(0.0070)
Additional Effect of No License if	0.0135***	0.0120***
School-Leaving Age $\leq$ 16 ($\beta_1^B + \beta_3^B$)- ($\beta_1^A + \beta_3^A$)	(0.0047)	(0.0046)
Controls	-	Y
Obs	75,196	75,196

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

ferent levels of mobility restrictions. $IntLicense_{st}$ is an indicator variable that equals one if age-16 teens 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 they cannot obtain either type of drivers 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.

As with the main results in Table 3, estimates of the placebo test in our expanded model (β_1^A and β_1^B) are very 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 an intermediate license only reduces the probability of high school dropout by 0.65pp in states where compulsory schooling laws are non-binding. Because the intermediate license is unlikely to hinder access to school transportation, we can interpret this negative effect as representing only the indirect channel. In other words, the reduction in access to labor and/or leisure activities caused by limiting 16-year-old driving privileges leads to a 17% 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 positive 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 a positive 0.54pp, but is estimated with a large standard error.[26] This estimate suggests that the negative effect of the GDL law on high school dropout stemming from reduced access to alternate activities is approximately offset by a positive direct effect stemming from reduced ability to commute to school when teen access to driving is completely removed.

Also of interest here is the difference between the two total effect estimates, ($\beta_1^B + \beta_3^B$) - ($\beta_1^A + \beta_3^A$). This difference identifies the *additional* effect of going from a GDL law that restricts teens to an intermediate license only to a GDL law that full restricts teen driving (at age 16). This estimate is a 1.2pp *increase* in the probability of high school dropout. This 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

[26]Note that only 12 states ever fully restricted access to driving for 16-year-olds during the time period under study. Thus, identification of the estimates for β_1^B and β_3^B is based on a relatively small number of observations.

and indirect channels exist for this policy and rely on structural estimation to provide a more formal effect decomposition in Section 7.

6.2 GDL Laws and Teen Employment

We next turn to an investigation of whether the indirect channel is, at least in part, attributable to reduced access to labor force participation under GDL laws. We replace the dependent variable in Equation 2 with an indicator for whether the individual teen is currently in the labor force and re-estimate the model.

Columns (1)-(2) of Table 6 show the marginal effects estimates resulting from the triple-difference model. The effect on labor force participation of increasing the minimum driving age in states where 16-year-olds cannot legally dropout is negative, but also relatively small and statistically insignificant. Conversely, the effect of GDL laws in states where teens are legally able to dropout is much larger and statistically significant: the probability of labor force participation drops by 2.0pp. At the mean, this is an 8.7% reduction in 16-year-old labor force participation (about one quarter of 16-year-olds work in this sample; see Table 1).[27]

Columns (3)-(4) of Table 6 show the marginal effects estimates resulting from the difference-in-differences model that excludes the interaction term, $\beta_3 GDL_{st} * CS_{st}$. These results indicate that increasing the unrestricted driving age to greater than 16 weakly reduces labor force participation for 16-year-olds by approximately 1.1pp (this effect is imprecisely measured with a p-value of 0.135). This estimate is, again, an average of the effects for teens that can legally dropout and those that cannot. Moreover, these results are qualitatively similar to those in Argys, Mroz, and Pitts (2019), though somewhat smaller in magnitude.[28]

These results indicate that, when teens are required to stay in school, the impact of GDL restrictions is, at most, a weak reduction in teen labor force participation. However, when teens are at liberty to drop out of school, they significantly reduce labor force participation in response to the GDL laws. This strongly suggests that there is an indirect channel linking teens' decisions regarding schooling and work when they are faced with mobility restrictions. However, GDL laws may also restrict access to other activi-

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

[28]The analysis in Argys, Mroz, and Pitts (2019) differs from ours in several respects. Argys, Mroz, and Pitts: (1) probabilistically assign treatment (GDL laws) to monthly employment data; (2) only consider data starting 1995; and (3) do not control for compulsory schooling laws.

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

	In Labor Force = 1			
	Triple-Diff		Diff-in-Diff	
	(1)	(2)	(3)	(4)
Min. Unres. Driving Age >16 (β_1)	-0.0071	-0.0057	-0.0122	-0.0106
	(0.0102)	(0.0107)	(0.0076)	(0.0073)
School-Leaving Age $\leq$ 16 (β_2)	0.0235	0.0177		
	(0.0153)	(0.0161)		
Min. Unres. Driving Age >16 $\times$ School-Leaving Age $\leq$ 16 (β_3)	-0.0131	-0.0145		
	(0.0121)	(0.0132)		
Effect of GDL if School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	-0.0201**	-0.0202**		
	(0.0085)	(0.0086)		
Controls	-	Y	-	Y
Obs	75,196	75,196	75,196	75,196

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

ties besides work and school, and the estimates in Table 3 could reflect substitution away from those activities as well. We next turn to a formal discrete choice model to better understand these findings.

7 Model

Section 5 and 6 show that teen schooling and work decisions are interrelated. The effect of mobility restrictions on schooling reflects both an indirect channel through the decision of whether to work (or participate in other activities) as well as a direct effect of reducing educational access. We develop a model that disentangles these channels. Agents choose between work, school, both, or neither, and GDL laws can differentially shift the value of these activities. Following Gentzkow (2007), our model allows school and work to function as complements or substitutes—choosing an outcome can have a direct impact on choosing the other outcome.[29] Agents may have idiosyncratic preferences for school and work; we allow these to be arbitrarily correlated. Exclusion restrictions separately identify this correlation from the complementarity or substitutability of activities.

Denote two activities, work and school, as A and B, respectively. Each agent i chooses to partake in one, both, or neither; their choice set is $(y_i^A, y_i^B) \in \{0,1\}^2 = \mathcal{C}$. Agents receive utility $\tilde{V}_i(y_i^A, y_i^B)$ from each bundle and choose the bundle that maximizes utility: $\max_{(y_i^A, y_i^B)} \tilde{V}_i(y_i^A, y_i^B)$. Only differences in utility matter, so we normalize the level of utility to $\tilde{V}_i(0,0)$ and define $V_i(y_i^A, y_i^B) = \tilde{V}_i(y_i^A, y_i^B) - \tilde{V}_i(0,0)$. **Assumption 1** formalizes this assumption on the form of indirect utility for each choice:

$$V_i(0,0) = 0 \tag{4}$$

$$V_i(1,0) = \alpha^A + \gamma^A GDL_{st}^A + x_{ist}'\lambda^A + z_{st}^{A'}\pi^A + f^A(s,t;\phi) + e_i^A \tag{5}$$

$$V_i(0,1) = \alpha^B + \gamma^B GDL_{st}^B + x_{ist}'\lambda^B + z_{st}^{A'}\pi^B + f^B(s,t;\phi) + e_i^B \tag{6}$$

$$V_i(1,1) = V_i(1,0) + V_i(0,1) + \Gamma + \gamma^\Gamma GDL_{st}^\Gamma, \tag{7}$$

where γ^{k_+} are the parameters of interest intended to capture the utility effect of the graduated driver license policy, for $k_+ \in \{A, B, \Gamma\}$. The model also includes individual characteristics x_{ist}, excludable state-year characteristics z_{st}^k, and additional controls to remove confounding factors in $f^k(s,t;\phi)$ for $k \in \{A, B\}$ (discussed below).[30] Complementarity or substitutability between school and work is captured by $\Gamma + \gamma^\Gamma GDL_{st}$ (> 0 if the activities are complements, and < 0 if substitutes).[31]

Agents have idiosyncratic preferences (e_i^k) for each activity. **Assumption 2** states that these preferences are distributed bivariate normal: $\mathbf{e}_i = [e_i^A \;\; e_i^B]' \sim N(0, \Omega)$, where

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

and the scale of the idiosyncratic preference is normalized to activity A (work). Given the model above, the probability that agent i chooses bundle $c \in \mathcal{C}$ is the probability that i's

[29]An alternative model is bivariate probit with both outcomes endogenous. However, Lewbel (2007) shows that such a model is generally incoherent and incomplete. Recent work explores new combinatorial discrete choice methods (Arkolakis and Eckert 2017).

[30]Specifically, individual characteristics include gender, race/ethnicity, mother's education, presence of father in household, and receipt of SNAP benefits, while state chracteristics include drop out laws and real minimum wage.

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

utility from c is greater than that from the other choices c':

$$P_i^c = \Pr\left(V_i(c) \geq V_i(c'), \forall c' \in \mathcal{C}\right).$$

7.1 Model Identification and Estimation

Model identification relies on the bivariate normal assumption on idiosyncratic preferences and the utility structure presented above. However, three facets of identification warrant additional discussion. First, both Γ and ρ reflect how likely both activities are to be chosen and are not separately identified without further assumptions. However, activity-specific utility shifters provide variation that disentangles these two parameters. A shift in the utility of one activity only increases (decreases) the likelihood of choosing the other activity if both activities are complements (substitutes). Thus comparing outcomes across different values of the shifter identifies Γ, while ρ then reflects how correlated idiosyncratic tastes are for the two activities.

Second, while the parameters of multinomial probit models are asymptotically identified from choice data, Keane (1992) shows that this identification is weak even in datasets with reasonable numbers of observations. Stronger identification can be obtained with activity-specific characteristics, and many applications of multinomial probit exploit such exclusions (e.g., Goolsbee and Petrin 2004).

To jointly resolve these issues, z^k includes factors that may shift the utility of one activity but are plausibly excludable from the other activity. Specifically:

$$z_{st}^A = \begin{bmatrix} UR_{st}, \\ \ln(MW_{st}) \end{bmatrix} \qquad \begin{matrix} \text{(unemployment rate)} \\ \text{(real minimum wage)} \end{matrix} \tag{8}$$

$$z_{st}^B = \begin{bmatrix} CS_{st}, \\ GDL_{st}^B \times CS_{st} \end{bmatrix} \begin{matrix} \text{(dropping out permitted)} \\ \text{(16yo dropping out permitted} \times \text{GDL)} \end{matrix} \tag{9}$$

Assumption 3 requires that (i) at least one element of π^A or π^B be non-zero and (ii) z_{st}^A does not directly influence $V_i(0,1)$ and z_{st}^B does not directly influence $V_i(1,0)$. While 3(i) is testable, excludability 3(ii) is not. However, we believe that 3(ii) is reasonable given the selected z^k. The exclusion restrictions require that unemployment rate and minimum wage only impact schooling decisions of 16-year-olds indirectly, i.e., by influencing their likelihood to work. Similarly, whether 16-year-old teenagers are permitted to drop out and the interaction of CS laws and GDL laws can only indirectly influence working by shifting the likelihood of remaining in school.

The third identification challenge is to ensure that γ^{k_+} reflect the effects of GDL policies and not other factors that may be correlated with GDL policies. In linear settings, state and year fixed effects would control for many of these potentially confounding factors. In non-linear settings, the inclusion of fixed effects can create estimation challenges (if there are large number of effects) and induce bias in parameter estimates (the "incidental parameter problem"). Correlated random effects share many of the benefits of fixed effects but are more amenable to non-linear settings. In fact, fixed effects and correlated random effects are numerically equivalent in linear models (Mundlak 1978). We therefore include correlated random effects by assuming including in $f^k(s,t;\phi)$ a vector with the average value of each x and z for each state as well as a vector of time dummies (omitting the first sample year to avoid collinearity).[32]

Because the cdf of the truncated bivariate normal presented above is non-standard, we estimate the model using maximum simulated likelihood (SML). We use Halton draws for each observation and use a multi-step minimization procedure to aid convergence of the optimization algorithm.

7.2 Model Estimates and Fit

Table 7 shows the estimated values of ten key model parameters. Non-policy parameters of particular interest are the correlation of idiosyncratic preferences for school and work, ρ, and the complementarity between activity, Γ. The model estimates $\rho = -0.64$, indicating that unobserved individual factors that shift the utility of school and work are quite negatively correlated. Despite this, school and work are moderate complements, suggesting that participating in one activity increases the utility of participating in the other.

The policy parameters (γ and π) appear to be qualitatively consistent with results in Section 5 and Section 6, suggesting a reduction in employment and increase in schooling in response to GDL adoption. The absolute magnitude of the work effect is larger than that of the schooling effect, but smaller in terms of standard deviations of idiosyncratic preference ($|\gamma^A| < |\frac{\gamma^B}{\sigma}|$). Legalizing school-leaving substantially decreases the utility of

[32] Note that we use correlated random effects, not the quite dissimilar correlated random coefficients model. Our model has 104 parameters to estimate instead of the 182 required with a fixed effects specification, saving computational time, improving the likelihood of convergence, and reducing concerns about incidental parameters.

Table 7: Key Model Parameters

			Work		School				
ρ	σ	Γ	α^A	γ^A	α^B	γ^B	π^B_{MLDA}	$\pi^B_{MLDA\times GDL}$	γ^Γ
-0.6366	0.0807	0.0611	-0.4996	-0.0258	0.0427	0.0059	-0.0103	0.0059	-0.0040

Point estimates of key model parameters estimated via maximum simulated likelihood using a discrete accept-reject simulator and Nelder-Mead optimization algorithm, where the errors are simulated using 100 bivariate Halton draws per observation. Observations are weighted using sample weights.

attending school, and the interaction of legalizing school-leaving with restricting GDL laws partially reverses that effect. Finally, GDL laws appear to slightly reduce the complementarity between schooling and work, as indicated by the negative value on γ^Γ.

Table 8 assesses how well the model explains the data by showing how often a simulated choice matches the observed choice (averaged over 100 draws of e). The model slightly overestimates neither work nor school (0,0) and school only (0,1), while slightly underestimates work only (1,0) and both work and school (1,1). Overall, summing the diagonal components of Table 8, the model correctly classifies those in the sample 62.3% of the time. Given the large number of individual characteristics that we do not observe, we believe this to be reasonable.

Table 8: 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%	0.080%	0.038%	1.931%	0.423%
Model $\mathcal{P}^{(1,0)}$	1.241%	0.033%	0.019%	0.893%	0.295%
Model $\mathcal{P}^{(0,1)}$	74.413%	1.888%	0.978%	56.246%	15.301%
Model $\mathcal{P}^{(1,1)}$	21.874%	0.453%	0.294%	15.200%	5.927%

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.

7.3 Counterfactuals: Decompositions and Invariance

We use the model to estimate treatment effects, decompose them, and provide additional interpretation of the effects in Section 5 and 6. While Assumptions 1–3 are sufficient to identify model parameters and total treatment effects, one additional assumption is required to identify decompositions. **Assumption 4** specifies a value or range of values for $\tilde{\gamma}^0$ in the following:

$$V_i(0,0) = \tilde{\gamma}^0 GDL^0_{st} \tag{10}$$
$$V_i(1,0) = \alpha^A + (\gamma^A + \tilde{\gamma}^0)GDL^A_{st} + x'_{ist}\lambda^A + \pi^A z^A_{st} + f^A(s,t;\phi) + e^A_i \tag{11}$$
$$V_i(0,1) = \alpha^B + (\gamma^B + \tilde{\gamma}^0)GDL^B_{st} + x'_{ist}\lambda^B + \pi^B z^B_{st} + f^B(s,t;\phi) + e^B_i \tag{12}$$
$$V_i(1,1) = V_i(1,0) + V_i(0,1) + \Gamma + (\gamma^\Gamma - \tilde{\gamma}^0)GDL^\Gamma_{st}. \tag{13}$$

This renormalization augments and is observationally equivalent to the primary model in Equations (4)–(7).[33] Because each individual only faces one value of GDL across activities, observing individual choice does not distinguish the absolute level of GDL's effect on each activity. But the model is informative about the relative effects on each activity up to the linear scalar $\tilde{\gamma}^0$. The augmented model formalizes this by redistributing the estimated effects of the GDL policy onto the outside option via the auxiliary parameter $\tilde{\gamma}^0$.[34] While any decomposition will reflect this unidentified auxiliary parameter, it allows recovering direct and indirect effects in a manner disciplined by the model. Furthermore, we use additional information to specify reasonable values of $\tilde{\gamma}^0$.

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

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

where n is the total number of observations. The right hand side captures the average probability of a bundle being chosen in the sample given the GDL terms and $\tilde{\gamma}^0$. In a

[33]This follows immediately upon renormalizing to set $V_i(0,0) = 0$. In fact, if $GDL^0_{st} = GDL^{k_+}_{st}, \forall k_+, s, t$, the renormalization is exactly equivalent to that shown in Equations (4)–(7).

[34]Retaining $\tilde{\gamma}^0 = 0$ implies that mobility restrictions do not impact the value of neither work nor school (0,0), which is unlikely as we discuss below.

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 with $GDL^k_{st} = 1$ and $GDL^k_{st} = 0, \forall k, s, t$:

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

The total effect is invariant to the value $\tilde{\gamma}^0$, so $\theta^k_{\text{Tot}} = \theta^k_{\text{Tot}}(\tilde{\gamma}^0), \forall \tilde{\gamma}^0$, though this will not be generally true for the decompositions.[35] We define these counterfactuals to reflect the triple-difference design.[36]

Model estimates of the total effect are presented at the top of Table 9. These effects are not targeted by estimation, so we can compare them with the results in Section 5 and 6. The model predicts that adopting a GDL law when school leaving is legal increases the probability of being in school by 1.29pp and decreases the probability of labor force participation by 0.82pp. These results are roughly in line with those in prior sections, though the magnitudes differ a bit. The model uses more information than the separate analyses in Section 5 and Section 6 by modeling the entire decision space. However, it also requires more restrictions, such as the correlation structure on latent utility (Assumptions 1 and 2) and the exclusion restrictions (Assumption 3ii), and relies on correlated random effects rather than fixed effects. The model also suggests that GDL policies reduce the likelihood of neither work nor school by about -0.97pp, or just over 40% from baseline.[37] This is bit larger than effects found in the literature on the impacts of GDL laws and driving on risky behaviors (Deza and Litwok 2016; Deza 2019; Huh and Reif 2021), but this is to be expected as our "neither nor work school" category is a catch-all that captures participation in risky behaviors as well as simple dislike of school and work.

We next use the model to decompose the total effect into its direct and indirect chan-

[35]Invariance of the θ^k_{Tot} is because the incidence of $\tilde{\gamma}^0$ is identical across all choices.

[36]Counterfactuals assume $CS = 1$ (and thus $CS \times GDL = GDL$) to replicate the triple-difference design.

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

nels. 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 school. We define these effects in a consistent manner that additively decomposes the total effect into the two channels.[38] Specifically:

Neither activity effects

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

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

Employment effects

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

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

Schooling effects

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

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

These decompositions are not invariant to the auxiliary parameter $\tilde{\gamma}^0$; the incidence of $\tilde{\gamma}^0$ across choices varies with the decomposition. Thus, one could select any value of $\tilde{\gamma}^0$ to perform the decomposition, and the decomposition will still be consistent with the estimated model and hence observed choices. For example, we expect the direct effect of GDL laws to be weakly negative because mobility restrictions weakly decrease school access for teens, at least in states where 16-year-olds are not legally allowed to drop out. Enforcing this sign restriction set identifies the decompositions.[39]

We use variations on this type of restriction to decompose the effect of GDL laws under several different variants of Assumption 4:

4a) The GDL law has no direct effect on the outside option. This is equivalent to $\tilde{\gamma}^0 = 0$, and disallows restricted mobility decreasing the utility of the outside option.

4b) The GDL has no direct effect on the utility of school if students cannot drop out. This is equivalent to $\tilde{\gamma}^0 = -\gamma^B$. This implicitly makes it so that the decrease in

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

[39] Set identification of the decompositions requires weak monotonicity of θ^k with respect to $\tilde{\gamma}^0$, and can be strengthened if $\lim_{\tilde{\gamma}^0 \to \pm\infty} \theta^k_{\cdot}(\tilde{\gamma}^0) = \bar{\theta}^k_{\cdot}$.

utility to the outside option from GDL restrictions in states where students cannot drop out is about as big as the increase in utility to school from GDL restrictions in states where students can drop out.

4c) The GDL has no direct effect on the utility of school if students can drop out. This is equivalent to $\tilde{\gamma}^0 = -(\gamma^B + \pi^B_{CS\times GDL})$ or equivalently to setting $\tilde{\gamma}^0$ such that $\theta^B_{\text{Dir}} = 0$.

4d) The direct effect of the GDL is as negative on the utility of the outside option as it is on the utility of work. This is equivalent to setting $\tilde{\gamma}^0$ such that $\theta^{\varnothing}_{\text{Dir}} = \theta^A_{\text{Dir}}$.

We prefer Assumption 4c, as it requires that the direct effect of the GDL restriction on school is weakly negative ($\tilde{\gamma}^0 \leq -(\gamma^B + \pi^B_{CS\times GDL})$, or equivalently $\tilde{\gamma}^0 : \theta^B_{\text{Dir}} \leq 0$). In large part, this is because we find it unlikely that restricting mobility directly increases access to schooling. Moreover, it implies a substantial direct effect of restricted mobility on the neither-activity option, which is consistent with the literature on how teen risky behavior responds to changes in mobility (Deza and Litwok 2016; Deza 2019; Huh and Reif 2021). Finally, this assumption leads to all direct effects being weakly negative. While we prefer this assumption, we discuss all estimates below.

Table 9 shows the decomposition.[40] Under the assumption that the value of the outside (neither-activity) option is unaffected by GDL laws in Panel A, the direct effects dominate for both work and school. In fact, the direct effect on schooling exceeds the total effect, so by necessity the indirect effect from reduced access to work slightly decreases schooling (recall that school and work are complements). The direct effect on work is -0.74pp, which is slightly amplified by indirect effects (though the effect of schooling is positive). The indirect effect from schooling is especially important for the reduction in neither activity.

Under the assumption that $\tilde{\gamma}^0 = -\gamma^B$ in Panel B, the direct effect continues to play an important role, but the indirect effects play a more important role than under assumption 4a. Roughly 38% of the increase in schooling comes from the GDL reducing the value of the other activities, especially doing neither. This assumption also implicitly assumes that the direct effect on work is more severe than in the baseline (as is also true under the two assumptions that follow). The impact on the neither-activity option is composed of a substantial direct effect of -0.57pp as well as an important but smaller indirect effect.

The next two scenarios are quite similar to each other; each assume that the direct effect of GDL policies is weakly negative if school-leaving is legal. Panel C of Table 9

[40] Table 9 includes in italics additional, non-additive terms that focus on specific indirect channels to aid interpretation. Differences between the sum of these components and the indirect effect are due to both non-additivity and the changes in complementarity.

Table 9: Effect Decomposition

	Neither Effects		Work Effects		Schooling Effects	
	Effect	% of Total	Effect	% of Total	Effect	% of Total
Total effect	-0.972pp		-0.821pp		1.293pp	
A. Decomposition maintaining $\tilde{\gamma}^0 = 0$.						
Direct	0pp$^+$	0.0%	-0.743pp	90.5%	1.508pp	116.6%
Indirect	-0.972pp	100.0%	-0.077pp	-9.5%	-0.215pp	-16.6%
via Neither	-		0pp$^+$		0pp$^+$	
via Work	0.105pp		-		-0.066pp	
via School	-1.060pp		0.035pp		-	
B. Decomposition assuming $\tilde{\gamma}^0 = -\gamma^B$.						
Direct	-0.573pp	58.9%	-0.915pp	111.3%	0.802pp	62.0%
Indirect	-0.399pp	41.1%	0.093pp	-11.3%	0.492pp	38.0%
via Neither	-		0.023pp		0.495pp	
via Work	0.111pp		-		-0.094pp	
via School	-0.490pp		0.019pp		-	
C. Decomposition assuming $\theta^B_{\text{Dir}} = 0$.						
Direct	-1.067pp	109.7%	-1.079pp	131.4%	0pp$^+$	0.0%
Indirect	0.095pp	-9.7%	0.258pp	-31.4%	1.293pp	100.0%
via Neither	-		0.042pp		1.058pp	
via Work	0.113pp		-		-0.129pp	
via School	0pp$^+$		0pp$^+$		-	
D. Decomposition assuming $\theta^{\varnothing}_{\text{Dir}} = \theta^A_{\text{Dir}}$.						
Direct	-1.086pp	111.7%	-1.086pp	132.2%	-0.039pp	-3.0%
Indirect	0.114pp	-11.7%	0.265pp	-32.2%	1.332pp	103.0%
via Neither	-		0.043pp		1.086pp	
via Work	0.113pp		-		-0.130pp	
via School	0.019pp		-0.001pp		-	

Shows the simulated effects of policy counterfactuals and their decomposition using parameters shown in Table 7 averaged over 100 draws of errors from a bivariate normal with a standard generator. To match the triple-difference design in Sections 4–**??**, for all counterfactuals we set $CS_{st} = 1$ (and so $GDL^B_{st} \times CS_{st} = GDL^B_{st}$). Observations are weighted using sample weights. $^+$ indicates that the value was assumed.

reflects our preferred bound, $\tilde{\gamma}^0 : \theta^B_{\text{Dir}} \leq 0$, and Panel D lies within that bound. Panels C and D show that all of the increase in schooling is due to the indirect effect of decreased utility for being out of school and work. This is by assumption. More interestingly, this effect on schooling is driven primarily by the decrease in utility from the neither-activity option. Thus, a reasonable conclusion is that decreases in access to work (and thus its utility) due to GDL laws have at most *a small effect* on schooling; this is consistent across all rotations.

The model also indicates that the effect of GDL laws on labor force participation are mostly direct; these laws substantially inhibit 16-year-old work. This is not surprising, given the substantial reductions in mobility that accompany GDL restrictions, at least for a period of time after receiving the intermediate license. The countervailing effects from decreased utility of the neither-activity option are relatively small.

8 Conclusion

We interact graduated drivers licensing and compulsory schooling laws to study the effects of mobility restrictions on schooling and employment outcomes for 16-year-old teenagers in the United States. GDL laws were adopted by many states in the late 1990s, before the gradual ratcheting up minimum legal dropout ages in the 2000s. This provided a window during which teen automobility was restricted but during which they 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 0.8pp (a 21% reduction from the mean), but only in settings in which school-leaving is a legal option. This potentially surprising result suggests that access to other activities may have decreased even more than access to school, leading to substitution toward school. To this end, we estimate the effect of GDL laws on teen labor force participation and find that they lead to a 2.0pp (8.7% at the mean) reduction in 16-year-old labor force participation.

We turn to a structural model of multiple activity choice to help interpret these results. The model has its own set of identification and interpretation challenges, and our discussion of these may be useful for others combining policy analysis with structural modeling. The model separates the direct effects of the policy from indirect effects (through substitution or complementarity effects). While the model can be identified using a similar empirical strategy as the rest of the paper, the decomposition requires an additional assumption on the relative incidence of the GDL laws on each activity in the model. 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. In this case, there was an additional benefit to schoolgoing, contributing to educational attainment. However, there was also a reduction in teen work, which may itself have additional positive or negative consequences in the long run.

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Appendix

A Additional Results

Table A.1: The Effect of Mininum Driving Age on Fatal Car Accidents by Driver Age, Precise Measures

	Accidents per 1,000			
	Age 15 (1)	Age 16 (2)	Age 17 (3)	Age 18 (4)
Min. Unres. Driving Age $\in [16.5, 17)$	-0.016*	-0.075***	-0.033**	-0.028*
	(0.009)	(0.019)	(0.014)	(0.015)
Min. Unres. Driving Age $\in [17, 17.5)$	-0.002	-0.077***	-0.040***	-0.046***
	(0.007)	(0.023)	(0.012)	(0.012)
Min. Unres. Driving Age $\in [17.5, 18)$	0.012	-0.171***	-0.093***	-0.100***
	(0.008)	(0.040)	(0.030)	(0.027)
Min. Unres. Driving Age $= 18$	-0.000	-0.081***	-0.054***	-0.019
	(0.010)	(0.021)	(0.014)	(0.015)
Mean Outcome	0.044	0.227	0.309	0.400
Obs	1,500	1,500	1,500	1,500

All regressions include state and year fixed-effects. Standard errors are clustered at the state-level.
* p<0.10, ** p<0.05, *** p<0.01

Table A.2: The Effect of Minimum Unrestricted Driving Age on Teen Dropout Decision (Linear)

	Not In School = 1					
	Triple-Diff				Diff-in-Diff	
	(1)	(2)	(3)	(4)	(5)	(6)
Min. Unres. Driving Age >16 (β_1)	0.0014 (0.0043)	0.0005 (0.0042)	0.0033 (0.0051)	0.0008 (0.0044)	-0.0022 (0.0035)	-0.0024 (0.0033)
School-Leaving Age $\leq$ 16 (β_2)	0.0186*** (0.0046)	0.0175*** (0.0044)				
Min. Unres. Driving Age >16 $\times$ School-Leaving Age $\leq$ 16 (β_3)	-0.0089* (0.0045)	-0.0076* (0.0045)	-0.0104 (0.0062)			
Effect of GDL if **School-Leaving Age $\leq$ 16** ($\beta_1 + \beta_3$)	-0.0075** (0.0037)	-0.0071* (0.0038)	-0.0071 (0.0046)	-0.0069* (0.0038)		
School-Leaving Age	As Observed		Never Switchers Only	Fixed in Yr. of GDL Change	-	-
Controls	-	Y	Y	Y	-	Y
Obs	75,196	75,196	46,567	75,196	75,196	75,196

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

B Robustness Analyses

A potential issue 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 proves problematic if treatment effects are changing over time. Since our data cover 28 years and there are changes in GDL laws in several years, we consider subsets of the full study window to probe whether treatment effects are dynamic or static.

Specifically, we estimate a probit model similar to Equation 2 and introduce 2 sample restrictions. First, we remove states that are "always-treated" in our study window (adopted a GDL law prior to 1995). This precludes long-run dynamic effects from states that adopted GDL laws long ago from contaminating estimated effects. Second, we cut off the sample at earlier and earlier years, targeting the 1995–2002 window when most states adopted GDL laws.

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

	Not In School = 1				
		Drop always-treated states & Limit sample to years:			
	Full Sample (1)	1990-2017 (2)	1990-2012 (3)	1990-2007 (4)	1990-2002 (5)
Min. Unres. Driving Age >16 (β_1)	0.0010	-0.0004	0.0012	0.0019	-0.0008
	(0.0042)	(0.0044)	(0.0038)	(0.0042)	(0.0072)
School-Leaving Age $\leq$ 16 (β_2)	0.0162***	0.0181***	0.0208***	0.0258***	0.0235**
	(0.0044)	(0.0047)	(0.0054)	(0.0062)	(0.0093)
Min. Unres. Driving Age >16 $\times$ School-Leaving Age $\leq$ 16 (β_3)	-0.0088*	-0.0098**	-0.0092**	-0.0106**	-0.0070
	(0.0045)	(.0047)	(0.0038)	(0.0044)	(0.0079)
Effect of GDL if School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	-0.0078*	-0.0102**	-0.0080**	-0.0087**	-0.0078
	(0.0040)	(0.0046)	(0.0040)	(0.0040)	(0.0050)
Obs	75,196	60,864	49,038	35,755	21,603

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

Table B.1 shows the results of these exercises. Column (1) replicates our preferred specification (column (2) of Table 3) to aid comparison. Columns (2)–(5) drop any states that are always-treated during our study window (about 20% of observations). While column (2) uses data over the full study window, columns (3)–(5) respectively omit the five, ten, and fifteen most recent years of data. Results for all model estimates are relatively constant across specifications, though they become less precise as more data is omitted. The placebo effect (β_1) remains close to zero, whereas the interaction effect (β_3) and marginal effect of GDL if drop outs 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 downward bias in considering the full sample window. We consider the results in Table B.1 as signifying that our findings are not being driven by long-run dynamics in the effects of GDL laws.

Much of the recent literature is concerned with consistently estimating the path of treatment effects in treatment time. However, we lack sufficient power to simultaneously estimate dynamic treatment effects paths for all components of our interacted design. Instead, we include indicators than bin groups 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 B.2 reports the results of this "grouped" triple-difference design. Estimates of β_1 stay 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 marginal effects of GDL laws without drop out restrictions ($\beta_1 + \beta_3$) are relatively constant over time as well, further suggesting that our estimates represent a reasonable approximation of the average treatment effect.

We also recast our research design into a framework that is more compatible with the recent literature on robust difference-in-difference estimation. To do this, we make three major changes from our preferred specification. First, we disallow an independent effect of GDL laws on school-going when teens are not permitted to drop out (when school-leaving age is >16). That is, we recast our triple-difference design as a more standard difference-in-differences design where the treatment is the interaction of restricted driving laws and unrestricted drop out laws. Given the small, insignificant, and relatively precise estimates of β_1, we view this as a reasonable shift to probe robustness to the concerns raised in the econometric literature.

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

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

	Not In School = 1	
	Main Specification (1)	Effect Over Time (2)
Min. Unres. Driving Age >16 (β_1)	-0.0004	
	(0.0044)	
0-4 Yrs Post		-0.0003
		(0.0051)
5-9 Yrs Post		-0.0021
		(0.0046)
10-14 Yrs Post		-0.0016
		(0.0073)
15+ Yrs Post		0.0019
		(0.0090)
School-Leaving Age ≤ 16 (β_2)	0.0181***	0.0214***
	(0.0047)	(0.0053)
Min. Unres. Driving Age >16	-0.0098**	
$\times$ School-Leaving Age ≤ 16 (β_3)	(0.0047)	
0-4 Yrs Post		-0.0093
		(0.0063)
5-9 Yrs Post		-0.0052
		(0.0043)
10-14 Yrs Post		-0.0080
		(0.0053)
15+ Yrs Post		-0.0155*
		(0.0084)
Effect of GDL if	-0.0102**	
School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	(0.0046)	
0-4 Yrs Post		-0.0097**
		(0.0042)
5-9 Yrs Post		-0.0074
		(0.0046)
10-14 Yrs Post		-0.0095*
		(0.0058)
15+ Yrs Post		-0.0136**
		(0.0062)
Obs	60,864	60,864

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

usually generate biased and inconsistent estimates. However, comparing the linear probability model estimates in Table A.2 with the probit results in Table 3 suggests that this is reasonable.

Finally, the newly developed estimators that account for treatment effect dynamics in a difference-in-differences model do not permit treatment to "turn on" and then "turn off" again. Therefore, we must omit some data from our sample to account for the fact that our interacted treatment ($GDL_{st} * CS_{st}$) both turns on and turns off over time. Specifically, in states for which the interacted treatment ever equals one (turns on), we drop all years of data after treatment then turns off.

Figure 1a reveals that states are gradually adopting GDL laws, but Figure 1b shows that they are also gradually restricting the ability of 16-year-olds to drop out. This implies that the interaction of restricted GDL laws and unrestricted dropout legality typically comes into effect (turns on) for a period of time before being blocked (turns off) by restricted compulsory schooling laws. To illustrate, the solid black line in Figure B.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 then 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 2 that excludes the non-interacted GDL_{st} term:

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

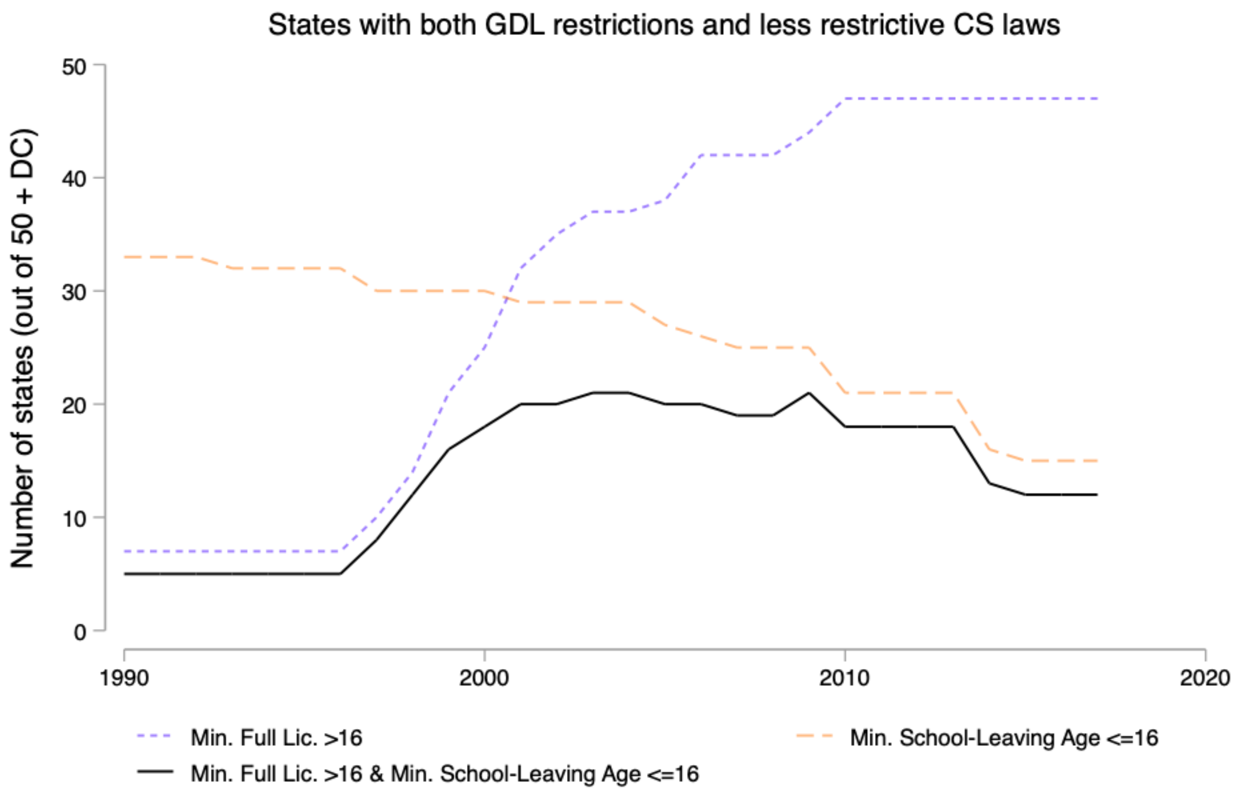
wherein E_s is the first year that $GDL_{st} * CS_{st} = 1$ and the β_{sk} are potentially heterogeneous and dynamic estimates that, when aggregated, correspond to β_3 in Equation 2.[41] If, as previously estimated, the true value of β_1 is zero, then estimates from Equation 2 and Equation B.1 should be very similar.

We apply the imputation estimator of Borusyak, Jaravel, and Spiess (2021), which is the most efficient linear unbiased estimator of any pre-specified weighted sum of treatment effects under the assumptions of parallel trends and homoskedasticity, and has attractive efficiency properties under heteroskedasticity.[42] This estimator recovers a well-

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

[42]In our experience, this imputation estimator is more readily implementable and computationally robust

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



defined ATT even under arbitrary treatment-effect heterogeneity and dynamism. We define the initial year of treatment as the first year in which both $GDL_{st} = 1$ and $CS_{st} = 1$ for a state. If either $GDL_{st} = 0$ or $CS_{st} = 0$ for all years in our sample for a state, it is considered never-treated. If both $GDL_{st} = 1$ and $CS_{st} = 1$ in 1990 at the beginning of our sample, the state is considered always-treated and it is omitted.

Table B.3 shows the results using the imputation estimator of Borusyak, Jaravel, and Spiess (2021). Specifically, column (1) omits all controls except CS_{st}. Column (2) includes our standard battery of controls. Point estimates are only slightly smaller in magnitude than the corresponding linear probability estimates in Table A.2. The standard errors,

than estimators that individually estimate and aggregate all possible 2x2 difference-in-differences design (Callaway and Sant'Anna 2020). In our setting, many individual 2x2 designs feature a small number of observations, and individual estimates from these designs are extremely noisy. The imputation approach uses more information to estimates st-specific treatments (under a maintained assumption of parallel trends), and so is more efficient.

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

	Not In School = 1	
	(1)	(2)
Effect of GDL if	-0.0066*	-0.0067*
School-Leaving Age $\leq$ 16	(0.0040)	(0.0038)
Controls	-	Y
Obs	50,729	50,729

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

which are conservative under treatment effect heterogeneity but exact if treatment effects are homogenous, are only slightly larger.[43] These results suggest that our main results are robust to arbitrary treatment effect heterogeneity and dynamics.

C District-Level Dropout Analyses

To support the findings on teen education outcomes shown in Section 5, we collect school-district level data on high school dropouts from the National Center for Education Statistics' (NCES) Common Core of Data (CCD). This data covers school-years from 1994 to 2009 and includes the combined dropout rate for grades 9-12 as well as several time-varying measures of district-level student demographics and other characteristics. For a smaller set of years (1994-2001) dropout rates are also reported separately for each grade 9 through 12. Due to reporting inconsistencies, the data comprises an unbalanced panel of 12,325 school-districts over 16 school-years.

Because this data is aggregated up to the district-by-grade level, we are not able to

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

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

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

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

Table C.1: Summary Statistics on School Districts

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

Data 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 and the 3-month average unemployment rate. The model also includes both district and year fixed effects. District fixed effects control for time-invariant characteristics of a school, such as location and district membership. Because schools typically stay relatively fixed in the income distribution of attendee families in the short and medium term, these also control to some degree for socioeconomic differences in student populations. We estimate Equation C.1 as a linear model and estimate standard errors clustered at the state level.

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

	Dropout Rate Grades 9-12	Dropout Rate Grades 9-12	Dropout Rate Grades 9-12	Dropout Rate Grade 9	Dropout Rate Grade 10	Dropout Rate Grade 11	Dropout Rate Grade 12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Min. Unres. Driving Age	-0.0038***						
	(0.0010)						
Min. Unres. Driving Age >16		-0.0030*	-0.0046**	-0.0031	-0.0049**	-0.0057**	-0.0046
		(0.0016)	(0.0020)	(0.0024)	(0.0019)	(0.0025)	(0.0032)
Years in Sample	1994-2009	1994-2009	1994-2001	1994-2001	1994-2001	1994-2001	1994-2001
Obs	114,043	114,043	44,735	44,166	44,246	44,366	44,623

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

Column (1) of Table C.2 shows that a one year increase in the minimum unrestricted driving age leads to a 0.38pp reduction in high school dropout rates. This is equivalent to an 11% 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 0.30pp reduction in the high school dropout rate (a 9% reduction from the mean).

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