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

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Abstract

We provide evidence that graduated driver licensing (GDL) laws, originally intended to improve public safety, impact both high school completion and teen employment. Many teens use automobiles to commute both to school and to employment. Because school and work decisions are interrelated, 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 research design 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 reflects reduced access to school, work, and other activities, which reveals that limiting access to work alone cannot explain the reduction in high school dropout rates.

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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 particularly dangerous for teenagers: they 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 may impact critical human capital accumulation during formative teen years through several channels, rendering the sign of their effects 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 or leisure), which could increase school attendance and degree completion. This paper has two goals: (i) to 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 and leisure.

We combine variation in the adoption of graduated driver licensing (GDL) laws across the United States with variation in compulsory schooling laws in a difference-in-differences-in-differences design to study the effect of teen mobility restrictions on high school retention. GDL laws typically increase the minimum age at which teens can access full-privilege driver's licenses and create an intermediate licensing level that restricts night-time driving and/or restricts the number of passengers who may ride with a teen driver. Variation in the stringency of GDL laws allows us to separate the direct effect on high school dropout rates caused by reduced educational access from the indirect effects due to reduced access to alternative activities.

A leading example of alternative activities to schooling for teens is labor force partic-

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

[2]For teens aged 16-19; from the US Department of Transportation's Fatality Analysis Reporting System and the National Center for Health Statistics' National Vital Statistics System data.

ipation. However, identifying trade-offs between education and employment is complicated by several factors. First, schooling and work are not mutually exclusive activities and could act as either complements or substitutes for teens. Furthermore, teen access to employment may itself be directly impacted by the implementation of GDL laws. We therefore apply our triple-difference research design to study teen labor force participation decisions in a reduced-form analysis, and also develop and estimate a formal model of individual choice between schooling and employment. This allows us to determine whether the effect of GDL laws on high school dropout is attributable to reduced access to employment.

Using microdata from the Current Population Survey's Annual Social and Economic Supplement, we find that GDL laws decrease the probability of high school dropout for 16-year-olds by 0.8pp (a 21% reduction at the mean).[3] That restricting teen mobility *improves* educational attainment is surprising because reducing access to an activity should have a weakly negative effect on participation, ceteris paribus. However, the strictest variant of GDL law, which completely disallows unsupervised 16-year-old driving, actually *increases* the probability of high school dropout by 0.5pp (although this estimate is not statistically significant). These two results suggest that limiting teen driving can improve educational outcomes by reducing access to alternative activities (such as leisure or employment), but it can also be detrimental to educational outcomes if teen access to driving is completely removed.

Estimating the effect of GDL law implementation on teen employment outcomes, we find that GDL laws reduce 16-year-old labor force participation by 2.0pp. This could reflect a direct effect from GDL laws reducing access to employment opportunities, but may also reflect an indirect channel if teens substitute to school when faced with mobility restrictions. In our multiple discrete choice model, teens choose to participate in school, work, both activities, or neither activity and we allow GDL laws to differentially impact each of these options. Model estimates reveal that employment is not a strong substitute for high school attendance; in fact, they are weak complements. Thus, very little of the reduction in high school dropouts is a result of changes in labor market access due to GDL laws. Instead, improved high school retention likely reflects decreased access to leisure activities (including, for example, risky behaviors).

This paper offers several contributions. The first is an important insight into the de-

[3] We separately verify this result in school-district level dropout data from the National Center for Educational Statistics' Common Core of Data.

terminants of educational attainment among teenagers. Sixteen-year-olds, despite not being adults de jure, make meaningful human capital decisions that will impact their lifetime trajectories in the labor market. Policies that influence the economic environments in which these teens live can shape those decisions in unexpected ways. We show that GDL laws are one such policy.[4] By limiting teen mobility, policy-makers inadvertently impacted the monumental decision of whether or not to complete a high school degree.

This finding contributes to the literature on the determinants of high school dropout behavior, which has consistently shown that leisure activities (and especially risky behaviors) are strongly correlated with the decision to drop out (Bray et al. 2000; Koch and McGeary 2005; Crispin 2017).[5] In a comprehensive analysis of the effects of compulsory schooling laws on educational attainment, Oreopoulos (2007) concludes that, "it is very difficult to reconcile substantial returns to compulsory schooling with an investment model of school attainment. The results are more consistent with the possibility that many adolescents ignore or heavily discount future consequences when deciding to drop out of school." Related literature shows that GDL laws reduce the likelihood of criminal behavior and teen pregnancy (Deza and Litwok 2016; Deza 2019). We connect these literatures and provide evidence that GDL laws, through reduced access to leisure activities, improve educational attainment for teens.

We also provide insight into teen labor force decisions, which are both directly impacted by mobility restrictions and provide a mechanism through which driving restrictions might impact educational attainment. Our framework reveals that GDL policies directly limit access to employment for 16-year-olds, but also that it is not this reduced propensity to work that leads to increased high school retention. This finding complements existing evidence that restrictions to mobility can impact the labor supply of non-teen groups (Amuedo-Dorantes, Arenas-Arroyo, and Sevilla 2020; Black, Kolesnikova, and Taylor 2014) and adds to the literature investigating the link between education and employment for teens. Eckstein and Wolpin (1999) show that youths who drop out of high school have a comparative advantage at jobs that are typically held by non-graduates

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

[5]Anderson (2014) also finds evidence of a reversed causal link: increasing the minimum legal dropout age has a significant and negative effect on violent and property crime arrest rates for teens.

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

Our other contributions are more methodological in nature. We develop a novel triple-difference research design that interacts a second restriction on teen behavior with GDL laws: compulsory school attendance. Compulsory schooling laws make it very costly for most 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, whereas the increase in minimum school-leaving ages began largely in the early 2000s) provides a unique opportunity to observe the effects of teen mobility restrictions when students have the option to drop out of school. Moreover, this triple-difference design provides a natural placebo test. We find no evidence of an effect of GDL laws on teens for whom attendance is compulsory, which suggests that our identification strategy successfully isolates exogenous policy variation.

Finally, we develop a structural framework for policy analysis that incorporates our triple-difference identification strategy. We show that this model can distinguish between mechanisms, separating direct from indirect (substitution) effects. Few papers combine quasi-experimental identification strategies with discrete choice models for policy evaluation (an exception is Li 2018).[7] Our model retains a primary focus on identifying policy parameters while adding structure that enhances our ability to gain additional insight and interpretation. Moreover, the model provides an alternative to, and ultimately reinforces, the design-based (i.e., reduced form) approach. These two methods are complementary, and their joint use provides a fruitful path for continued research.

We briefly describe the background and context for our study and detail data sources in Section 2. In Section 3 and Section 4, we describe the triple-difference research design

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

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

and our main results on education outcomes. Section 5 then investigates both variation in GDL law intensity and teen employment outcomes to explore potential mechanisms. Section 6 unites education and employment decisions within a structural model to differentiate the various effects of GDL laws on teen activities and Section 7 offers concluding remarks.

2 Context and Data

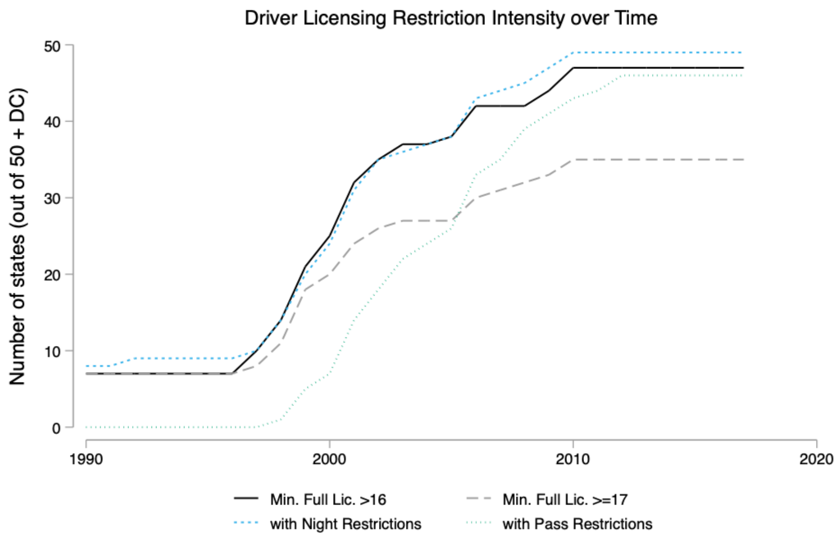
High teen driving fatality risk in the United States in the 1980s led to the implementation of a number of policies targeted at improving both car safety and limiting teen driving. One such policy that has been widely adopted starting in the mid 1990s is the graduated driver's license (GDL). GDL laws: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

We develop a database of pertinent state-level GDL laws in the 50 states and DC from several sources, including the Federal Highway Administration's (FHWA) Highway Statistics and the Insurance Institute for Highway Safety (IIHS) covering the years 1990 to 2017.[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. But by 2010, forty-seven states had placed increased restrictions on teenage driving. Much of the adoption of GDL laws occurred between 1996 and 2003.

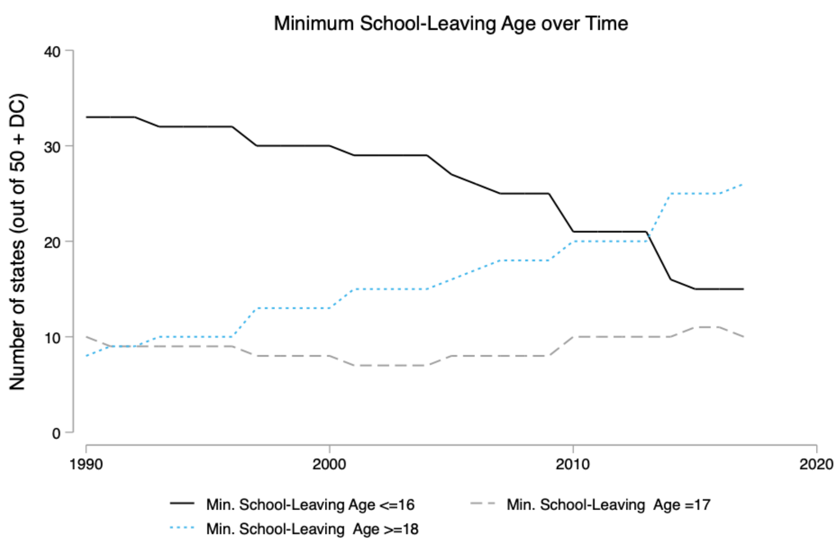
To verify that GDL laws had a binding effect on teen automobile use, we link our GDL law dataset to information from the US Department of Transportation's Fatality Analysis Reporting System (FARS). We use the rate of fatal car accidents involving a teen driver as a proxy for the prevalence of teen driving and estimate the effect of increasing the minimum full-privilege driving license age on teen accident rates. We find that the GDL

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

driving restrictions reduced the rate of fatal car accidents for drivers aged 16 by 0.08 accidents per thousand 16-year-olds in the population. This result indicates that teens are significantly less likely to be involved in a fatal car accident when they cannot access an unrestricted driver's license and that GDL laws significantly restricted teen driving. We discuss this verification exercise in detail in Appendix A.

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

We link the data on each state's GDL and CS laws to individual-level data on schooling and work decisions in the Current Population Survey (CPS) Annual Social and Economic Supplement (ASEC).[10] The CPS ASEC data is from an annual survey of U.S. households conducted in March of each year and provides person-level information on a variety of 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 chose not to enroll in college. CPS ASEC participants were also surveyed on labor force participation and employment status in the preceding week.

To construct our final estimation sample, we limit the linked GDL law, CS law, and CPS data to individuals aged 16 at the time of the ASEC survey. This serves dual purposes: (1) our measure of high school dropout, $NotInSchool_i$, should incorporate less

[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 Appendix A, 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.

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 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: Summary Statistics on Individuals Aged 16

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

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

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

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

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 D.1).

3 Empirical Strategy

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

The primary identifying assumption in this type of triple-difference set-up is much weaker than the parallel trends assumption needed for difference-in-differences.[14] 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 specification for our sample of 16-year-olds:

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

where GDL_{st} is an indicator variable that equals one if the minimum unrestricted driving age in state s in year t is > 16 (i.e., 16-year-olds experience mobility restrictions).[15] We

[14]Identification also hinges on an assumption of homogenous treatment effects. We consider deviations from static homogeneous treatment effects in Section 4.1 and Appendix C.

[15]For the purposes of this variable, we consider as restrictions: limits of the time of day that one can

capture compulsory schooling laws with CS_{st}, an indicator that equals one if the minimum school-leaving age is ≤ 16 (i.e., 16-year-olds are legally permitted to drop out of school). The vector X_i includes individual-level controls: gender, race/ethnicity indicators, mother's education, presence of father in household, and receipt of SNAP benefits. The variable Z_{st} includes controls for the state's minimum wage and the 3-month average unemployment rate. This specification also includes both state fixed effects to control for time-invariant confounding factors (such as persistent differences in school quality or returns to education across states) and year fixed effects to control for aggregate fluctuations (such as changes in national schooling laws over time).[16]

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 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 approximately 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 teen labor force participation (Deza and Litwok 2016; Deza 2019; Argys, Mroz, and Pitts 2019).[17] Because teens face a time

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

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

[17] 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,

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.

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

The coefficient β_1 in Equation 1 will therefore identify the "placebo" effect of imposing mobility restrictions on dropout behavior in states where 16-year-olds cannot legally drop out. However, if CS laws are not well-enforced (or if they incorporate exemptions for teens who are working or have parental consent), then we may still observe an impact of the GDL laws on high school dropout (ie., $\beta_1 \neq 0$). In this case, an effect should be observable only if there are enough students who experience a direct or indirect effect of the mobility restriction that is large enough to incentivize law-breaking. For example, in rural areas where school attendance zones are expansive and school buses can require long commutes (Howley, Howley, and Shamblen 2001), we might expect the direct effect of GDL laws to be large enough to cause an increase in high school dropout rates, even in states where the compulsory schooling laws are binding.

The coefficient β_2 in Equation 1 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. 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 drop out 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 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 (or if both effects are zero).

which are caused primarily by drug overdoses.

4 Results

We estimate the model in Equation 1 using a probit maximum likelihood estimator.[18] Standard errors are estimated that allow for clustering at the state level. Table 2 reports the corresponding marginal effects for each coefficient evaluated at the mean of all covariates.

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

	Not In School = 1					
	Triple-Diff				Diff-in-Diff	
	(1)	(2)	(3)	(4)	(5)	(6)
Min. Unres. Driving Age >16 (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 $\times$ School-Leaving Age ≤ 16 (β_3)	-0.0097**	-0.0088*	-0.0121*	-0.0084*		
	(0.0047)	(0.0045)	(0.0062)	(0.0050)		
Effect of GDL if School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	-0.0080**	-0.0078*	-0.0083*	-0.0073*		
	(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

In column (1), we estimate the model in Equation 1 excluding all control variables (X_i and Z_{st}). Column (2) presents our main specification, which includes all covariates. The estimates in these two columns demonstrate that our results are not sensitive to or driven by the inclusion of covariates. Estimates of 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 minimum schooling-leaving age is binding

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

(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 GDL laws on high school attendance (through increased difficulty in commuting to/from school), it is more than completely offset by the indirect effects of GDL laws through reduced access to labor and leisure activities.[19]

4.1 Robustness and Alternative Estimators

A potential confounding factor in this triple-difference model stems from 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 their minimum school-leaving age (see Figure 1b). To avoid conflating effects from changes in this policy with the effects of the GDL laws, we employ two robustness checks. First, we estimate the model in Equation 1 on the sub-sample of states that did not change their minimum school-leaving age during the time period under study. The results of this estimation strategy are shown in column (3) of Table 2.[20] This specification yields slightly larger estimates of the net

[19]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. These results are available upon request.

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

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 1 with a time-invariant measure that is fixed at each state's minimum school-leaving age in the year that the GDL law first increases the minimum unrestricted driving age to over 16. For states where the minimum unrestricted driving age is either always less than or equal to 16 or always greater than 16, we use the minimum school-leaving age from the first year of the sample, 1990.[21] The results of this specification are shown in column (4) of Table 2. Estimates are nearly identical to the estimates from our main specification in column (2) and support our previous findings.

Columns (5)–(6) of Table 2 display the results of a simpler difference-in-differences model that excludes the interaction term, $\beta_3 GDL_{st} * CS_{st}$. These results are relatively small and insignificant. This is to be expected as these small, negative estimates 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 2020; Goodman-Bacon 2021; Sun and Abraham 2021). Bias can arise from a number of sources, but is typically due to the implicit selection by the two-way fixed effects estimator of inappropriate counterfactuals (e.g., previously treated units) or of incorrect aggregation weights (e.g., negative weights). While our research design does not fit the standard staggered adoption difference-in-differences mold, the potential for these types of bias is still present.

A number of solutions have been proposed to overcome this issue, however none have thus far been adapted to fit our setting of using repeated cross-sectional data with a placebo-style triple-difference design.[22] Moreover, this literature focuses exclusively on linear models: as our outcome of interest is binary with a mean value close to zero, a linear probability model is potentially biased and inconsistent. Because of these aspects of our research design, there is no alternative estimation strategy that we can adopt whole-

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

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

sale from the current literature to address the challenges caused by heterogeneous and dynamic treatment effects. Nevertheless, we provide several exercises to test the robustness of our results to possible deviations from static, homogeneous treatment effects. We discuss these approaches to robust estimation in detail in Appendix C.

The first approach estimates a model similar to our preferred specification but considers subsets of the time variation used in the full analysis and allows for some dynamism in treatment effects. The results (shown in Table C.1 and Table C.2) provide evidence that our main results are not being driven by long-run dynamics in the treatment effects of GDL laws. 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 C.3 and are largely unchanged from our main results, even despite the smaller sample size.

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

We find that the implementation of GDL laws leads to a 0.38pp reduction in high school dropout rates in the NCES data, which is an 11% reduction at the mean.[23] 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 2 and provide compelling evidence that imposing restrictions on teen mobility maintains high school enrollment and leads to a significant reduction in high school dropouts.

As noted in Section 3, we might expect the direct effect of GDL laws on high school attendance, which functions through restricting teens' ability to commute to school, to

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

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.

4.2 Heterogeneity Analysis

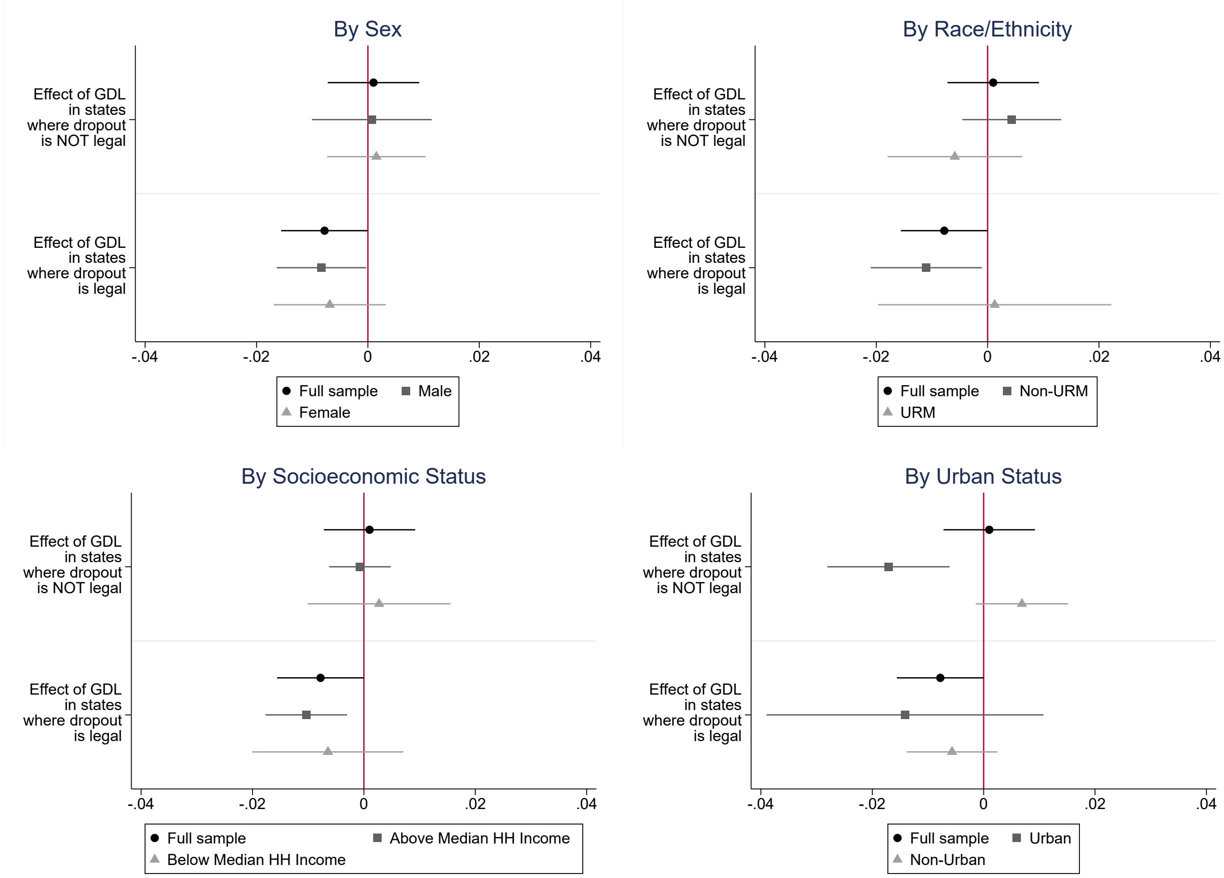
We estimate our preferred specification, given by Equation 1, separately for several subpopulations of interest. The marginal effects estimates are shown in Table 3, which also reports mean outcome values for each subgroup, and Figure 2. The top-left panel of Figure 2 shows the effects of GDL laws on 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.[24]

We next examine heterogeneity by race and household income. Heterogeneity in responses among these groups could reflect differential vehicle availability to teens, or could also reflect differential reliance on a vehicle if available. For example, a lower-income household may be less able to purchase or afford a vehicle for teen use. If vehicle take-up for teens in lower-income households is ex ante low, there will be less margin for GDL policies to shift behavior. At the same time, teens in lower-income households may not have as much access to alternatives to driving, such as parental transportation. This would suggest increased exposure to changes wrought by GDL laws and potentially larger effects.

In the top-right panel of Figure 2 (and columns (4)–(5) of Table 3) are effects of GDL laws estimated separately for underrepresented minorities (teens who identify as Black, Hispanic, or Native American) and all other race/ethnicity groups (non-URM). These estimates reveal that the negative impact of GDL laws on high school dropout is largely driven by non-URM 16-year-olds, who typically have a lower average dropout rate. The estimates for URM teens are quite noisy and very close to zero. These results may reflect greater access to vehicles related to wealth or household income, or a greater affinity for car culture among non-URM families.

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

Figure 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.

Table 3: 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	-0.0088*	-0.0090	-0.0084	-0.0154***	0.0072	-0.0096***	-0.0092	-0.0126***	0.0030
$\times$ School-Leaving Age $\leq$ 16 (β_3)	(0.0045)	(0.0056)	(0.0052)	(0.0054)	(0.0099)	(0.0037)	(0.0083)	(0.0043)	(0.0131)
Effect of GDL if	-0.0078*	-0.0083**	-0.0068	-0.0110**	0.0013	-0.0104***	-0.0065	-0.0057	-0.0141
School-Leaving Age $\leq$ 16 ($\beta_1 + \beta_3$)	(0.0040)	(0.0041)	(0.0051)	(0.0051)	(0.0107)	(0.0037)	(0.0069)	(0.0041)	(0.0127)
Mean Outcome	0.038	0.040	0.035	0.032	0.050	0.024	0.051	0.035	0.046
Obs	75,196	38,587	36,609	52,641	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

In the bottom-left panel of Figure 2 (and columns (6)–(7) of Table 3), we separate the estimation sample into two halves based on household income (as reported in the CPS). The median household income is $53,236.[25] Sixteen-year-olds in lower-income households are more than twice as likely to be observed as not in school than those in higher-income households. However, the estimated effects of GDL laws are 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. An alternative explanation is greater vehicle availability for teens in higher-income households, for whom GDL laws decrease the probability of high school dropout by 43% at the mean. Note, however, that 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 3) show the effects of GDL laws estimated separately for teens living in urban and non-urban areas. For teens in urban locations, the effects of GDL laws on high school dropout are negative and significant even when compulsory schooling laws make dropout illegal for the 16-year-olds in our sample. This suggests that enforcement of compulsory schooling laws may be lacking in urban areas.

5 Mechanism Analysis

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

[25] In 1999 dollars.

5.1 Variation in GDL Intensity

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

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

$$\begin{aligned} NotInSchool_{ist} = {} & \beta_1^A IntLicense_{st} + \beta_1^B NoLicense_{st} + \beta_2 CS_{st} \\ & + \beta_3^A IntLicense_{st} * CS_{st} + \beta_3^B NoLicense_{st} * CS_{st} \\ & + X_i'\nu + Z_{st}'\mu + D_s + D_t + \epsilon_{ist}. \end{aligned} \tag{2}$$

This specification is identical to Equation 1, except that we have replaced the single binary measure of GDL restrictions with two indicator variables corresponding to the two different levels of mobility restrictions. $IntLicense_{st}$ is an indicator variable that equals one if 16-year-olds in state s in year t can procure an intermediate driver's license *only* (and cannot obtain a full-privilege license until they are older). $NoLicense_{st}$ is an indicator variable that equals one if 16-year-olds cannot obtain either type of driver's license (intermediate or unrestricted). The omitted category comprises state-years where 16-year-olds have access to unrestricted, full-privilege licenses. The marginal effects estimates from this expanded model are shown in Table 4.

As with the main results in Table 2, estimates of the placebo test in our expanded model (β_1^A and β_1^B) are 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

Table 4: 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 $\leq$ 16 (β_2)	0.0164***	0.0154***
	(0.0046)	(0.0045)
GDL at 16 $\times$ School-Leaving Age $\leq$ 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

negative and statistically significant. The total effect of the restriction to an intermediate license on teen dropout behavior is estimated by the sum of coefficients, $\beta_1^A + \beta_3^A$. This sum reveals that limiting teen driving access to only the intermediate license level reduces the probability of high school dropout by 0.65pp 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 offset by a positive direct effect stemming from reduced ability to commute to school when teen access to driving is completely removed.

Also of interest here is the difference between the two total effect estimates, $(\beta_1^B + \beta_3^B)$ - $(\beta_1^A + \beta_3^A)$. This difference identifies the *additional* effect of going from a GDL law that restricts teens to an intermediate license only to a GDL law that fully restricts teen driving (at age 16). This estimate 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 4 as merely an indication that both direct and indirect channels exist for this policy and rely on structural estimation to provide a more formal effect decomposition in Section 6.

5.2 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 1 with an indicator for whether the individual teen is currently in the labor force and re-estimate the model.

Columns (1)-(2) of Table 5 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 drop out is negative, but

[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.

Table 5: 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

also relatively small and statistically insignificant. Conversely, the effect of GDL laws in states where teens are legally able to drop out is much larger and statistically significant: the probability of labor force participation drops by 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 5 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

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

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 suggests that there is an indirect channel linking teens' decisions regarding schooling and work when they are faced with mobility restrictions. However, GDL laws may also restrict access to other activities besides work and school, and the estimates in Table 2 could reflect substitution away from those activities as well. We next turn to a formal discrete choice model to better understand these findings.

6 Model-Based Analysis

The effect of mobility restrictions on schooling may reflect (i) the direct channel of reducing educational access or (ii) an indirect channel through reducing access to other activities. The plausibility of an indirect channel stemming from reduced access to employment depends on whether school and work are complements or substitutes in teens' utility functions. If school and work are highly substitutable, then the negative effect of GDL laws on high school dropout rates primarily reflects a reduction in teen labor force participation. If not, then the change in dropout behavior is due to changes in access to other activities. Distinguishing between these two types of indirect channels has important implications for policy recommendations.

We develop a model that disentangles these channels. Agents choose between work, school, both work and school, or neither activity. We allow GDL laws to differentially shift the utility of each of these choices. Following Gentzkow (2007), our multiple discrete choice model allows school and work to be complements or substitutes.[29] Agents have idiosyncratic preferences for school and for work and we allow these preferences to be arbitrarily correlated. Exclusion restrictions are needed to separately identify this

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

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

correlation from the complementarity or substitutability between activities. We provide key intuition for the model and identification below; full details are in Appendix E.

6.1 Identification

Denote 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. Only differences in utility matter, so we normalize the level of utility to the neither option: $V_i(y_i^A, y_i^B) = \tilde{V}_i(y_i^A, y_i^B) - \tilde{V}_i(0,0)$. The normalized indirect utility that agent i obtains from each choice is:

$$V_i(0,0) = 0 \tag{3}$$

$$V_i(1,0) = \alpha^A + \gamma^A GDL_{st}^A + x'_{ist}\lambda^A + z'_{st}\pi^A + f^A(s,\xi) + \delta_t^A + e_i^A \tag{4}$$

$$V_i(0,1) = \alpha^B + \gamma^B GDL_{st}^B + x'_{ist}\lambda^B + z'_{st}\pi^B + f^B(s,\xi) + \delta_t^B + e_i^B \tag{5}$$

$$V_i(1,1) = V_i(1,0) + V_i(0,1) + \Gamma + \gamma^\Gamma GDL_{st}^\Gamma, \tag{6}$$

where γ^{k_+} are the parameters of interest intended to capture the utility effect of the graduated driver license policy, for $k_+ \in \{A, B, \Gamma\}$.[30] The model includes individual characteristics x_{ist}: gender, race/ethnicity indicators, mother's education, presence of father in household, and receipt of SNAP benefits. Plausibly excludable state-year characteristics are captured by z_{st}, where we define

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

where UR_{st} is the state-level unemployment rate, $\ln(MW_{st})$ is log real minimum wage, and CS_{st} and GDL_{st} are measures of the compulsory schooling laws and graduated driver licensing laws, as defined in Equation 1. Additional elements to remove confounding factors are in $f^k(s,\xi)$ and δ_t^k represent time dummies, for $k \in \{A, B\}$. Complementarity or substitutability between school and work is captured by $\Gamma + \gamma^\Gamma GDL_{st}$ (> 0 if the activities are relative complements and < 0 if relative substitutes).

Agents choose the bundle that maximizes utility: $\max_{(y_i^A, y_i^B)} V_i(y_i^A, y_i^B)$. The probability that agent i chooses bundle $c \in \mathcal{C}$ is the probability that i's utility from c is greater than that from all other choices c': $P_i^c = \Pr\left(V_i(c) \geq V_i(c'), \forall c' \in \mathcal{C}\right)$. In order to identify and

[30] We index the GDL policy variable by k_+ to aid exposition; each individual experiences only one value of $GDL_{st}^{k_+}$, namely GDL_{st}.

estimate the model parameters, we make the following assumptions:

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

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

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

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

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

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

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

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

Assumption 1 imposes a structure that is similar to a multinomial probit model (e.g., Goolsbee and Petrin 2004), although with the addition of mulitple discreteness. The exclusion restrictions (Assumption 2) allow us to separately identify Γ from ρ and strengthen identification of the model more generally. Assumption 3 imposes a correlated random effects structure onto the model, allowing for parametric correlation between unobserved state-specific factors and observable covariates (Mundlak 1978). This helps ensure that identification of the parameters γ^{k+} are not confounded by other state-specific factors that may be correlated with the implementation of GDL policies.[31]

These three assumptions are sufficient to identify all structural parameters in the model and to plausibly separate the effects of the GDL policy (γ^{k+}) from other factors. To estimate, we rely on **Lemma 1** (see proof in Appendix E), which asserts that, under As-

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

sumption 1, the model given by Equations (3)–(6) can be estimated with a Geweke, Hajivassiliou, and Keane (GHK) simulator.[32] We then estimate the model using maximum simulated likelihood.

6.2 Model Estimates

Table 6 shows the estimated values of ten key model parameters.[33] Structural (non-policy) parameters of particular interest are the correlation of idiosyncratic preferences for school and work, ρ, and the complementarity between activities, Γ. The model estimates $\rho = -0.47$, 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) are qualitatively consistent with results in Section 4 and Section 5. The magnitude of the effect of GDL laws on teen labor force participation is larger than the corresponding effect on high school enrollment, both in absolute levels and in terms of standard deviations of idiosyncratic preference ($|\gamma^A| > |\frac{\gamma^B}{\sigma}|$). Legalizing high school dropout (by instituting a lower minimum school-leaving age) substantially decreases the utility of attending school. However, the interaction of legalizing school-leaving and restricting mobility (through GDL laws) partially reverses that reduction in utility. Finally, GDL laws reduce the complementarity between schooling and work, as indicated by the negative value on γ^Γ.

We show model-based equivalents of the design-based treatment effects estimated in Sections 4 and 5 as **total effects** in the top row of Table 7. The model predicts that adopting a GDL law when school-leaving is legal increases the probability of being enrolled in school by 1.01pp and decreases the probability of labor force participation by 1.09pp.[34] These results are roughly in line with those in prior sections, though the magnitudes differ

[32] This result is somewhat unexpected, as the error covariance structure of the four-choice multinomial probit model implied by Equations (3)–(6) is not positive definite. For a description of the GHK simulator, see Train (2009).

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

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

Table 6: Key Model Parameters

			Work		School				
ρ	σ	Γ	α^A	γ^A	α^B	γ^B	π^B_{CS}	$\pi^B_{CS\times GDL}$	γ^Γ
-0.4664	0.0486	0.0244	-0.6289	-0.0335	0.0275	0.0012	-0.0103	0.0054	-0.0047

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.

a bit. This is to be expected, as the model-based estimates incorporate additional information by modeling the entire decision space, while also imposing additional restrictions through the correlated error structure and exclusion restrictions. The model also suggests that GDL policies reduce the likelihood of the "neither work nor school" option by about -0.85pp, or about 35% from baseline.[35] We interpret this "neither" option as reflecting teen preferences for leisure activities, which encompass both risky behaviors as well as less-risky forms of truancy. It is then unsurprising that the estimated reduction in this category is somewhat larger than previously estimated effects found in the literature on the impacts of GDL laws and teen driving on risky behaviors (Deza and Litwok 2016; Deza 2019; Huh and Reif 2021).

6.3 Decomposition into Direct and Indirect Channels

The model allows us to decompose each of these three total effects into their direct and indirect channels. The **total effects** of GDL laws on each activity (denoted θ^k_{Tot}) capture the overall impact of increasing the minimum unrestricted driving age from 16 or less. **Direct effects** (denoted θ^k_{Dir}) reflect how each GDL component affects its *own activity*, e.g., the effect of GDL^A on working and of GDL^B on school enrollment. Because GDL laws restrict mobility, we expect that they will weakly reduce the value of each activity and that all three direct effects will therefore be weakly negative. **Indirect effects** (denoted θ^k_{Ind}) 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 schooling decisions.[36]

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

[36] We provide precise definitions in Appendix E. There are several reasonable ways to define these effects; our definition preserves additivity such that $\theta^k_{\text{Tot}} = \theta^k_{\text{Dir}} + \theta^k_{\text{Ind}}$.

The decomposition exercise requires one additional assumption. This is necessary because discrete choice models are identified only up to *relative differences* in utility. Thus, the utility of one of the individual's choices is typically normalized to zero. This normalization does not affect model fit or the identification and estimation of the total effects (shown in the top row of Table 7), but it does make the strong implicit statement that the policy (GDL laws) does not affect the utility of the normalized ("neither work nor school") option. The literature on changes in risky behavior by teens in response to GDL laws suggests that the utility of the neither option was likely shifted by the implementation of GDL policies (e.g., Deza and Litwok 2016; Deza 2019; Huh and Reif 2021), so it is vital that we relax this normalization assumption.

We renormalize the model to permit the GDL policy to impact the utility of the neither option by $\tilde{\gamma}^0$:

$$V_i(0,0) = \tilde{\gamma}^0 GDL^0_{st} \tag{7}$$
$$V_i(1,0) = \alpha^A + (\gamma^A + \tilde{\gamma}^0)GDL^A_{st} + x'_{ist}\lambda^A + z'_{st}\pi^A + f^A(s,\xi) + \delta^A_t + e^A_i \tag{8}$$
$$V_i(0,1) = \alpha^B + (\gamma^B + \tilde{\gamma}^0)GDL^B_{st} + x'_{ist}\lambda^B + z'_{st}\pi^B + f^B(s,\xi) + \delta^B_t + e^B_i \tag{9}$$
$$V_i(1,1) = V_i(1,0) + V_i(0,1) + \Gamma + (\gamma^\Gamma - \tilde{\gamma}^0)GDL^\Gamma_{st}. \tag{10}$$

This renormalization is observationally equivalent to the primary model in Equations (3)–(6), but redistributes the effects of the GDL policy onto the outside option via the auxiliary parameter $\tilde{\gamma}^0$.[37] This allows us to decompose the estimated total effects into their direct and indirect channels, with the caveat that any decomposition will reflect an assumption made on the unidentified auxiliary parameter.

We consider several possible assumptions on values of the renormalizing parameter, $\tilde{\gamma}^0$:

Assumption 4A. *In the renormalized model,* $\tilde{\gamma}^0 \leq 0$.

Assumption 4B. *In the renormalized model,* $\gamma^B + \tilde{\gamma}^0 \leq 0$.

Assumption 4C. *In the renormalized model,* $\gamma^\Gamma - \tilde{\gamma}^0 \leq 0$.

Assumption 4D. *In the renormalized model,* $\tilde{\gamma}^0 : \theta^B_{Dir} \leq 0$.

If $\tilde{\gamma}^0 = 0$, then there is no direct effect of the GDL laws on the neither option; Assumption 4A requires only that this direct effect be weakly negative. The other assumptions assume

[37]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 (3)–(6).

increasingly more negative maximal values of $\tilde{\gamma}^0$. Assumption 4B requires that the GDL policy have a weakly negative effect on the utility of school if students are unable to drop out (because of compulsory schooling laws). Assumption 4C requires that the GDL policy have a weakly negative effect on the complementarity between school and work. Finally, Assumption 4D requires that the GDL policy have a weakly negative direct effect on the utility of school when students can legally choose to drop out.

Two sets of these assumptions are of particular interest. The combination of Assumptions 4B and 4C allows for set identification of $\tilde{\gamma}^0$. Specifically, it maintains that $\gamma^\Gamma \leq \tilde{\gamma}^0 \leq -\gamma^B$. This range is reasonable, as it is unlikely that GDL laws improved the utility of school-going either alone or in combination with changes to employment. Alternatively, Assumption 4D ensures that GDL laws decrease the utility of school-going regardless of compulsory schooling laws. However, Assumption 4D implicitly requires that GDL laws increase the complementarity of schooling and work.

Table 7 shows the decomposition of each total effect into direct and indirect channels under each of these possible assumptions. Each panel corresponds to one of the Assumptions 4A through 4D holding with equality. Note that each indirect effect can be further separated into a channel for each of the alternative activity choices. For example, the total effect of GDL laws on schooling decisions can be decomposed into a direct effect, an indirect effect stemming from reduced access to employment, an indirect effect stemming from reduced access to leisure (represented by the neither option), and an indirect effect stemming from changes to the complementarity between schooling and employment. These individual indirect channels are shown in each panel of Table 7 in italics.[38]

Panel A of Table 7 shows the decomposition maintaining equality in Assumption 4A ($\tilde{\gamma}^0 = 0$). Under this assumption (that GDL laws have no direct impact on the utility of the leisure-only "neither" option), the effects of GDL laws on both teen employment and schooling are driven primarily by direct effects. In fact, the direct effect of GDL laws on schooling exceeds the total effect, so that the indirect effect stemming from reduced access to work slightly decreases school-going (recall that these two activities are complements). The direct effect of GDL laws on teen employment is -0.97pp, which is only slightly amplified by the indirect channels. However, as noted above, Assumption 4A does not accord with the existing literature on GDL laws and risky teen behaviors, so we view this decomposition as highly unlikely.

[38]Differences between the sum of these italicized components and the indirect effect are due to non-additivity and are discussed in Appendix E.

Table 7: Decomposition of GDL Law Effects by Activity

	Effect of GDL Laws on:					
	Neither		Work		School	
	Effect	% of Total	Effect	% of Total	Effect	% of Total
Total effect	-0.852pp		-1.085pp		1.014pp	
A. Decomposition maintaining $\tilde{\gamma}^0 = 0$.						
Direct	0pp^+	0.0%	-0.967pp	89.2%	1.343pp	132.5%
Indirect	-0.852pp	100.0%	-0.118pp	10.8%	-0.329pp	-32.5%
via Neither	-		0pp^+		0pp^+	
via Work	0.101pp		-		-0.053pp	
via School	-0.937pp		0.011pp		-	
via Change in Complementarity	0.007pp		-0.129pp		-0.277pp	
B. Decomposition assuming $\tilde{\gamma}^0 = -\gamma^B$.						
Direct	-0.194pp	22.8%	-1.004pp	92.6%	1.110pp	109.5%
Indirect	-0.658pp	77.2%	-0.080pp	7.4%	-0.096pp	9.5%
via Neither	-		0.004pp		0.162pp	
via Work	0.100pp		-		-0.057pp	
via School	-0.745pp		0.009pp		-	
via Change in Complementarity	0.005pp		-0.093pp		-0.205pp	
C. Decomposition assuming $\tilde{\gamma}^0 = \gamma^\Gamma$.						
Direct	-0.682pp	79.9%	-1.101pp	101.5%	0.433pp	42.8%
Indirect	-0.171pp	20.1%	0.016pp	-1.5%	0.580pp	57.2%
via Neither	-		0.014pp		0.633pp	
via Work	0.096pp		-		-0.071pp	
via School	-0.260pp		0.004pp		-	
via Change in Complementarity	0pp^+		0pp^+		0pp^+	
D. Decomposition assuming $\tilde{\gamma}^0 : \theta^B_{\text{Dir}} = 0$.						
Direct	-0.943pp	110.6%	-1.156pp	106.6%	0pp^+	0.0%
Indirect	0.091pp	-10.6%	0.071pp	-6.6%	1.014pp	100.0%
via Neither	-		0.018pp		0.936pp	
via Work	0.093pp		-		-0.080pp	
via School	0pp^+		0pp^+		-	
via Change in Complementarity	-0.002pp		0.053pp		0.129pp	

Shows the simulated effects of policy counterfactuals and their decomposition using parameters shown in Table 6 averaged over 100 draws of errors from a bivariate normal with a standard generator. To match the triple-difference design in Sections 3–5, 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.

In panels B and C of Table 7 we decompose each total effect maintaining Assumptions 4B and 4C ($\gamma^\Gamma \leq \tilde{\gamma}^0 \leq -\gamma^B$), respectively. In both of these panels, the direct effects of GDL laws on both schooling and employment are quite substantial, but the indirect channels also play an important role in the determination of high school dropout. Of the total effect of GDL laws on high school enrollment, 43%–110% can be attributable to the direct channel of reducing access to school commuting options. However, very little (-0.06pp – -0.07pp) of the indirect effect of GDL laws on schooling is coming through the employment channel. Instead, the indirect effect of GDL laws on schooling is caused by a reduction in the utility of the "neither" activity.

In panel D of Table 7, we make the strongest statement about the $\tilde{\gamma}^0$ parameter; that it is so negative that $\tilde{\gamma} + (\gamma^B + \pi^B_{CS\times GDL}) = 0$ (the upper edge of Assumption 4D). Intuitively, this forces the direct effect of GDL laws on school-going to be zero. Under this assumption, nearly all of the increase in schooling can be attributed to the indirect effect stemming from decreased utility for the neither school nor work option. The effect of GDL laws on teen employment is driven almost entirely by the direct effect of reduced access to commuting options. Note that this finding is consistent across all panels of Table 7.

The decompositions in Table 7 consistently show that at most a small component of the total effect of GDL laws on schooling is due to substitution away from employment. This holds across all considered values of $\tilde{\gamma}^0$. Instead, the total effect on schooling is driven by some combination of the direct channel and the indirect effect through the "neither work nor school" channel. Similarly, only a small component of the total effect of GDL laws on employment is due to changes in schooling decisions. In sum, work and school are not the margins of adjustment to mobility restrictions that affect each other; rather, participating in neither activity is the channel of primary adjustment.

This conclusion is crucial for interpreting the effects presented earlier in this paper and for future policy design. It suggests that the estimated reduction in high school dropout due to the implementation of GDL laws is not caused by teens substituting away from employment, but instead is caused by reduced utility associated with leisure and/or risky behaviors. On the other hand, the estimated reduction in teen employment due to GDL laws seems to have been driven primarily by a reduction in teens' ability to commute to jobs. Future policies that specifically target access to non-work, non-school activities would therefore likely be able to preserve the negative dropout effect without inducing a corresponding negative effect on teen employment.

7 Conclusion

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

A robust set of results indicate that GDL laws—which restrict teen mobility—actually decrease high school dropout by about 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 towards schooling. To this end, we estimate the effect of GDL laws on teen labor force participation and find that these laws led to a 2.0pp (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 channels (through substitution or complementarity effects). Under reasonable assumptions, we find that the indirect impacts of GDL laws on schooling are not due to decreased access to work, but likely reflect decreased access to activities that are neither work nor school. This accords with the literature on GDL laws and risky behaviors.

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

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Appendix

A GDL Laws and Teen Driving

To verify that GDL laws had a binding effect on teen automobile use, we estimate the effect of GDL roll-out on a proxy for driving.[39] We use the rate of fatal car accidents involving a teen driver as a proxy for the prevalence of teen driving by linking the GDL laws to data from the US Department of Transportation's Fatality Analysis Reporting System (FARS). FARS is a nationwide census of all fatal injuries suffered in motor vehicle crashes and provides data on the location and timing of the accident as well as the involved drivers' birth-years.

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

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

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

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

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

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

drivers aged 16 by 0.04 accidents per thousand 16-year-olds in the (state's) population. At the mean (0.214 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 age with an indicator variable that equals one if the minimum unrestricted driving age is strictly greater than 16 (corresponding to the solid, black line in Figure 1a). This yields an even larger negative estimate of 0.07 accidents per thousand 16-year-old population, indicating that teens are significantly less likely to be involved in a fatal car accident when they cannot access an unrestricted driver's license.

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

	Accidents per 1,000		
	(1)	(2)	(3)
Minimum Unrestricted Driving Age	-0.036***		
	(0.011)		
Min. Unres. Driving Age > 16 (year t+2)			-0.007
			(0.016)
Min. Unres. Driving Age > 16 (year t+1)			-0.010
			(0.016)
Min. Unres. Driving Age > 16		-0.073***	-0.035***
		(0.016)	(0.013)
Min. Unres. Driving Age > 16 (year t-1)			-0.016
			(0.015)
Min. Unres. Driving Age > 16 (year t-2)			-0.014
			(0.014)
Mean Outcome		0.214	
Obs	1,350	1,350	1,150

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 A.1 indicate that the introduction of GDL laws significantly restricted teen driving de facto. In column (3) we also include two leads and two lags of the minimum driving age indicator variable as a test for whether we are merely picking up trends in teen driving behavior. We find no evidence of either pre-trends or 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 Table A.2, 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.

Table A.2: The Effect of Minimum 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*	-0.069***	-0.029*	-0.025
	(0.009)	(0.016)	(0.015)	(0.015)
Min. Unres. Driving Age $\in [17, 17.5)$	-0.001	-0.070***	-0.035**	-0.041***
	(0.007)	(0.023)	(0.013)	(0.012)
Min. Unres. Driving Age $\in [17.5, 18)$	0.011	-0.165***	-0.109***	-0.092**
	(0.007)	(0.038)	(0.031)	(0.045)
Min. Unres. Driving Age $= 18$	0.004	-0.071***	-0.043***	-0.019
	(0.008)	(0.019)	(0.015)	(0.017)
Mean Outcome	0.042	0.212	0.292	0.380
Obs	1,350	1,350	1,350	1,350

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

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.[40] 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.

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

B Additional Results

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

	Not In School = 1					
	Triple-Diff				Diff-in-Diff	
	(1)	(2)	(3)	(4)	(5)	(6)
Min. Unres. Driving Age >16 (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

C Robustness Analyses

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

Specifically, we introduce two sample restrictions into the estimation of the probit model in Equation 1. First, we remove states that are "always-treated" in our study window (i.e., adopted a GDL law restricting full privilege licenses to teens older than 16 prior to 1997).[41] This precludes long-run dynamic effects from early-adopter states from contaminating estimated effects. Second, we cut off the sample at earlier and earlier years, targeting the 1997–2002 window when most states adopted GDL laws.

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

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

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

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

	Not In School = 1				
		Drop always-treated states & Limit sample to years:			
	Full Sample (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

tamination from long-run effects. Table C.2 reports the results of this "grouped" triple-difference design. Estimates of β_1 are stable and consistently 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 in states without binding dropout restrictions ($\beta_1 + \beta_3$) are fairly constant over time as well, further suggesting that our estimates are not biased by treatment effect dynamism.

Finally, in order to implement the solutions provided in the recent literature on robust difference-in-difference estimation, we recast our research design into a more compatible framework for those proposed robust estimators. 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 dropout laws. Given the small, insignificant, and relatively precise estimates

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

	Not In School = 1	
	Main Specification (1)	Effect Over Time (2)
Min. Unres. Driving Age >16 (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 $\leq$ 16 (β_2)	0.0181***	0.0214***
	(0.0047)	(0.0053)
Min. Unres. Driving Age >16	-0.0098**	
$\times$ School-Leaving Age $\leq$ 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 are omitted. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01

of β_1 throughout our analyses, we view this as a reasonable restriction on the estimation model.

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

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

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

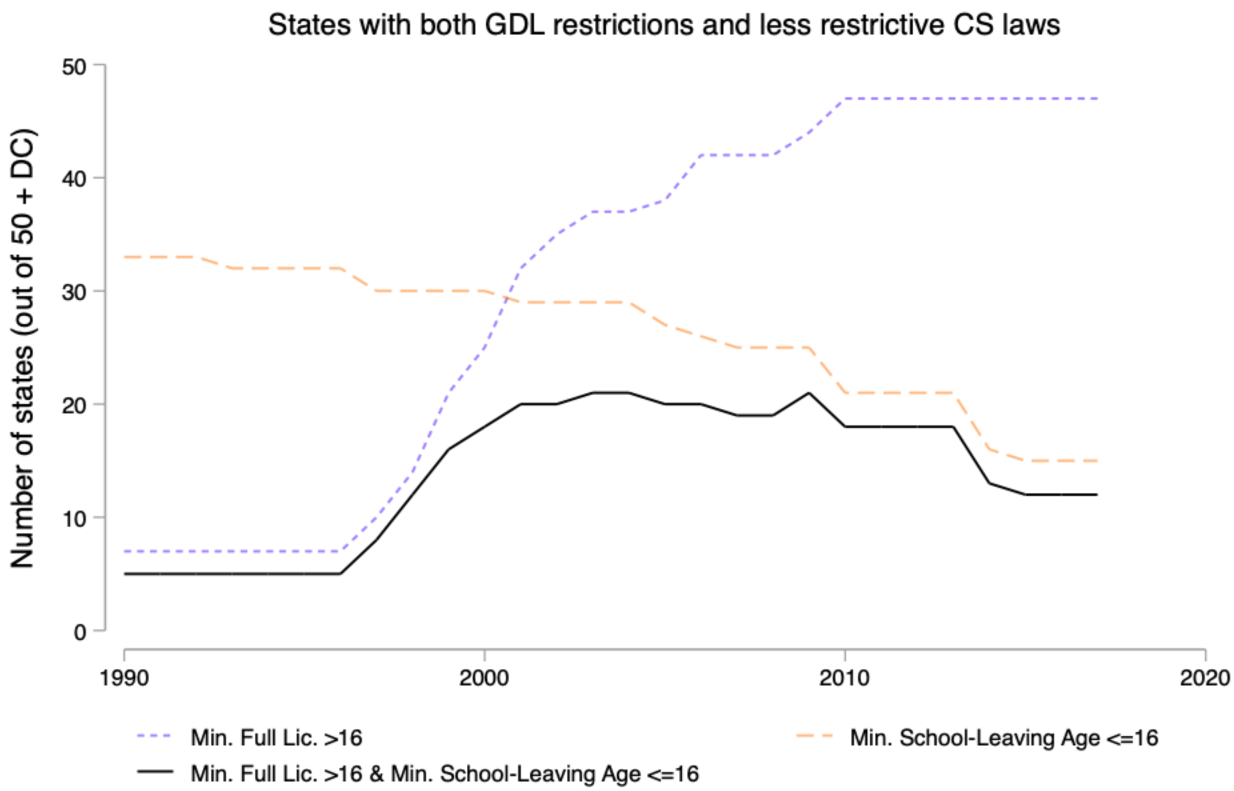
$$\begin{aligned} NotInSchool_{ist} = \beta_2 CS_{st} + \sum_k \beta_{sk} 1[t - E_s = k] \\ + X_i' \nu + Z_{st}' \mu + D_s + D_t + \epsilon_{ist}, \end{aligned} \tag{C.1}$$

wherein E_s is the first year that $GDL_{st} * CS_{st} = 1$ in state s and the β_{sk} are potentially heterogeneous and dynamic treatment effects that, when aggregated, correspond to β_3 in Equation 1.[42] If, as previously estimated, the true value of β_1 is zero, then estimates from Equation 1 and Equation C.1 should be very similar.

We apply the imputation estimator of Borusyak, Jaravel, and Spiess (2021), which is

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

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



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.[43] This estimator recovers a well-defined ATT even under arbitrary treatment-effect heterogeneity and dynamism.

Table C.3 shows the results using the imputation estimator. The model in column (1) omits all controls except CS_{st}. Column (2) includes all control variables (X_i and Z_{st}). These estimates are only slightly smaller in magnitude than the corresponding linear probability estimates in Table B.1. The standard errors, which are conservative under

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

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

	Not In School = 1	
	(1)	(2)
Effect of GDL if School-Leaving Age $\leq$ 16	-0.0066*	-0.0067*
	(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 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. * p<0.10, ** p<0.05, *** p<0.01

treatment effect heterogeneity but exact if treatment effects are homogenous, are only slightly larger.[44] These results suggest that our main results are robust to arbitrary treatment effect heterogeneity and dynamics.

D District-Level Dropout Analyses

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

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

[44] 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:

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

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

Table D.1: Summary Statistics on School Districts

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

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

The primary variable of interest is GDL_{st}, which measures the minimum age at which teens can obtain a full (unrestricted) driver's license. The vector X_{dt} includes time-varying school-district level controls: percent of students eligible for free lunch; percent of students white; number of full-time equivalent teachers; log of total expenditures per student; and urbanization indicators. The variable Z_{st} includes the state's minimum school-leaving age, log minimum wage, and 3-month average unemployment rate. The model also includes both district and year fixed effects. District fixed effects control for time-invariant characteristics of a school, such as location and district membership. Because schools typically stay relatively fixed in the income distribution of attendee families in the short and medium term, these also control to some degree for socioeconomic differences in student populations. We estimate Equation D.1 as a linear model and estimate standard errors clustered at the state level.

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

	Dropout Rate Grades 9-12	Dropout Rate Grades 9-12	Dropout Rate Grades 9-12	Dropout Rate Grade 9	Dropout Rate Grade 10	Dropout Rate Grade 11	Dropout Rate Grade 12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Min. Unres. Driving Age	-0.0038***						
	(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 D.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 includes only years up to 2001 in the sample. The effect of raising the minimum driving age to greater than 16 on overall high school dropouts is somewhat larger in magnitude in this sub-sample, reducing dropouts by 0.46pp. Columns (4)-(7) show that the effects of increasing the minimum driving age to over 16 are largest for 10th- and 11th-grade dropout rates (a 14% reduction from the mean in both grades). It is during these 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%.

E Details of the Model-Based Analysis

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

E.1 Identification

Model identification takes advantage of Assumption 1 (idiosyncratic preferences for work and school are distributed bivariate normal). However, three facets of identification warrant additional discussion. First, both 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.[45] 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 theoretically identified from choice data, Keane (1992) shows that this identification is weak even in datasets with reasonable numbers of observations. Stronger identification can be obtained with activity-specific characteristics, and many applications of multinomial probit exploit such

[45] Gentzkow (2007) mentions this possibility although instead uses repeated observations per individual to separate Γ and ρ.

exclusion restrictions (e.g., Goolsbee and Petrin 2004). Assumption 2 assigns restrictions on the parameters that serve as coefficients for z_{st}. These become activity-specific utility shifters, in that two components of z_{st} can only shift the utility of work and the other two components of z_{st} can only shift the utility of school. As with instrumental variables, the exclusion restriction alone may not aid estimation. It is also useful for at least one of the activity-specific utility shifters to have a non-zero effect on the activities they shift. This relevance condition is likely satisfied, given the π coefficients in Table 6.

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 parameters problem"). Correlated random effects models share many of the benefits of fixed effects models but are more amenable to non-linear settings. In fact, estimators using fixed effects and correlated random effects are numerically equivalent in linear models (Mundlak 1978). We therefore assume correlated random effects (Assumption 3) and include in $f^k(s, \xi)$ a vector with the average value of each x and z for each state. The model also includes a vector of time dummies (omitting the first sample year to avoid collinearity).[46]

E.2 GHK Simulator

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

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

multinomial probit, we would have:

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

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

The positive definiteness of the covariance matrix of idiosyncratic preferences has important implications for estimation. Lemma 1 shows that even though the implicit covariance matrix is Ω^{Extended}, we can instead rely just on Ω and thus the model can be estimated using a GHK (Geweke, Hajivassiliou, and Keane) simulator.[47] This simulator is advantageous because it is both fast and reasonably easy to implement, and results in much smoother likelihood functions than accept-reject simulators. These properties are computationally useful and also help ensure convergence.

Lemma 1. *Under Assumption 1, the model (Equations 3–6) can be estimated with a GHK simulator.*

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

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

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

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

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

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

where $c = \rho\sigma$ and $d = \sqrt{\sigma^2(1-\rho^2)}$. Thus, $(e^A, e^B) \stackrel{d}{=} (\eta_1, c\eta_1 + d\eta_2)$, where η_1 and η_2 represent i.i.d. standard normal variables. Rewrite the model in light of this equivalence in distribution (suppressing notation denoting individual i):

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

Straightforward substitution of data and coefficients for V_1, V_2, and Γ show equivalence to the primary model (Equations 3–6). Specifically, if $V_1 = \alpha^A + \gamma^A GDL^A_{st} + x'_{ist}\lambda^A + z'_{st}\pi^A + f^A(s,\xi) + \delta^A_t$, $V_2 = \alpha^B + \gamma^B GDL^B_{st} + x'_{ist}\lambda^B + z'_{st}\pi^B + f^B(s,\xi) + \delta^B_t$, and $\Gamma_{12} = \Gamma + \gamma^\Gamma GDL^\Gamma_{st}$, then the models are equivalent.

Step 2: We now show that the choice probabilities from this i.i.d. normal model can be derived in order to take advantage of the i.i.d. nature of the η_1 and η_2 variables. We show this sequentially for each choice in the choice set. First, the probability of choosing neither activity is:

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

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

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

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

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

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

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

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

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

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

E.3 Estimation Details

To simulate η_2, we use Halton draws. We then estimate the model by maximum simulated likelihood. Specifically, we code the model in Julia using the L-BFGS optimization routine in the Optim package (Mogensen and Riseth 2018). Because ρ and σ cannot take on all real values, we transform them as:

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

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

Our optimization procedure consists of several steps:

1. Maximize the simulated likelihood of a variant of the model where δ_t^A and δ_t^B are replaced with linear time trends with 100 draws of η_2 for each observation, using starting values of $\tilde{\rho} = -0.2$ and $\tilde{\sigma} = -0.5$ and all other coefficient at zero (experimentation showed that these starting values improved convergence).

2. Maximize the simulated likelihood of a variant of the model where δ_t^A and δ_t^B are replaced with linear time trends with 250 draws of η_2 for each observation, using the optimum from Step 1 for starting values.

3. Maximize the simulated likelihood of the full model with 100 draws of η_2 for each observation, using the optimum from Step 2 for starting values and extrapolating the linear time trends to create starting values for individual year dummies.

4. Maximize the simulated likelihood of the full model with 250 draws of η_2 for each observation, using the optimum of Step 3 for starting values.

E.4 Model Fit

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

Table E.1: Model Fit

		True $\mathcal{P}^{(0,0)}$	True $\mathcal{P}^{(1,0)}$	True $\mathcal{P}^{(0,1)}$	True $\mathcal{P}^{(1,1)}$
	Totals	2.454%	1.329%	74.271%	21.946%
Model $\mathcal{P}^{(0,0)}$	2.462%	0.083%	0.040%	1.901%	0.438%
Model $\mathcal{P}^{(1,0)}$	1.313%	0.036%	0.021%	0.941%	0.315%
Model $\mathcal{P}^{(0,1)}$	74.273%	1.880%	0.973%	56.168%	15.252%
Model $\mathcal{P}^{(1,1)}$	21.952%	0.455%	0.296%	15.261%	5.940%

Shows the shares of each observed and simulated outcome of the model using parameters shown in Table 6 averaged over 100 draws of errors from a bivariate normal with a standard generator. The top row shows the observed share of the population choosing each outcome, whereas the right column shows the average simulated shares that choose each outcome. The other cells show the average shares of the population for each observed and simulated outcome combination. Observations are weighted using sample weights.

E.5 Counterfactuals: Decompositions and Invariance

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

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

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

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

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

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

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

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

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

We define these effects in a consistent manner that additively decomposes the total effects into the two types of channels.[48] Specifically:

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

Neither activity effects

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

Employment effects

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

Schooling effects

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

Table 7 includes in italics additional, non-additive terms that focus on specific indirect channels to aid interpretation. For example, the indirect effect of GDL laws on schooling decisions consists of a component stemming from reduced access to employment, a component stemming from reduced access to leisure (represented by the neither option), and a component stemming from changes to the complementarity between schooling and employment. Differences between the sum of these components and the indirect effect are due to both non-additivity and the changes in complementarity. The *via Neither*, *via Work*, and *via School* sub-channels are all defined relative to the single-effect probability, i.e.:

$$\begin{aligned}
\theta^{\varnothing}_{\text{Ind, } \textit{via } k} &= \mathcal{Q}^{A}(1,k,k,0,\tilde{\gamma}^0) - \mathcal{Q}^{A}(1,0,0,0,\tilde{\gamma}^0)\\
\theta^{A}_{\text{Ind, } \textit{via } k} &= \mathcal{Q}^{A}(k,1,k,0,\tilde{\gamma}^0) - \mathcal{Q}^{A}(0,1,0,0,\tilde{\gamma}^0)\\
\theta^{B}_{\text{Ind, } \textit{via } k} &= \mathcal{Q}^{A}(k,k,1,0,\tilde{\gamma}^0) - \mathcal{Q}^{A}(0,0,1,0,\tilde{\gamma}^0).
\end{aligned}$$

where k determines which sub-channel is being investigated. Instead, the *via Change in Complementarity* is evaluated relative to the presence of all the other effects, as:

$$\theta^{k}_{\text{Ind, } \textit{via Change in Complementarity}} = \mathcal{Q}^{k}(1,1,1,1,\tilde{\gamma}^0) - \mathcal{Q}^{k}(1,1,1,0,\tilde{\gamma}^0).$$