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Graduated driver licensing and teen traffic fatalities

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Abstract

Over the last 8 years, nearly every state has introduced graduated driver licensing (GDL) for teens. These new licensing procedures require teen drivers to advance through distinct stages where they are subject to a variety of restrictions (e.g., adult supervision, daytime driving, passenger limits). In this study, we present evidence on whether these restrictions have been effective in reducing traffic fatalities among teens. These evaluations are based on state-by-year panel data from 1992 to 2002. We assess the reliability of our basic inferences in several ways including an examination of contemporaneous data for older cohorts who were not directly affected by these policies. Our results indicate that GDL regulations reduced traffic fatalities among 15–17-year-olds by at least 5.6%. We also find that the life-saving benefits of these regulations were plausibly related to their restrictiveness. And we find no evidence that these benefits were attenuated by an increase in fatality risks during the full-licensure period available to older teens.

JEL classification: I12; I18

Keywords: Teens; Traffic safety; Driver licensing

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1. Introduction

In a recent report, the Centers for Disease Control and Prevention (CDC, 1999) characterized improvements in motor vehicle safety as one of the 10 great public-health achievements of the 20th century. Over the last 3 decades, these gains have been particularly striking for young adults, the age group with the highest traffic fatality risk. More specifically, between 1975 and 1992, traffic fatality rates for 16–20-year-olds fell from 39 per 100,000 people to 28, a reduction of more than 25% (NHTSA, 2003, Table 6).

These impressive gains are due to a diverse set of factors that includes state and federal policy initiatives like minimum legal drinking ages, mandatory seat-belt laws and drunk-driving regulations (Dee and Evans, 2001). However, since 1992, the annual traffic fatality rate of young adults has been remarkably stable at approximately 29 deaths per 100,000 people (Grabowski and Morrisey, 2001). Furthermore, despite the improvements of the last 30 years, traffic fatalities are still the leading cause of death among young adults, accounting for 6277 deaths of 16–20-year-olds in 2002 alone (NHTSA, 2003, Table 54).

Over the last 8 years, most states have responded to these public-health concerns by introducing graduated driver licensing (GDL) programs for young drivers. The signature feature of these regulations is that they require new drivers to advance through restrictive beginner and immediate phases before they can achieve full licensure. The fundamental intent of these programs is to encourage new drivers to acquire critical driving skills and experience in low-risk and monitored settings. In 1996, the state of Florida implemented the first GDL program in the United States. However, within just 6 years (i.e., by 2002), 38 states had introduced similar policies (Table 1).

These new state-level licensing regulations have, arguably, become the premier policy initiative designed to improve traffic safety among young adults. However, largely because these policies are so recent, we know surprisingly little about their effects. In this study, we present new panel-based econometric evidence on the effects of GDL programs on teen traffic fatalities.

Our study makes several distinct contributions to the literature. First, we rely on the most recently available nationwide data on traffic fatalities (i.e., through 2002). Second, we validate the critical assumptions of our basic "difference-in-differences" (DD) specification by analyzing contemporaneous state-by-year data on traffic fatalities among older cohorts who were not directly affected by GDL regulations. We also synthesize these results by presenting the results of "difference-in-differences-in-differences" (DDD) specifications based on the pooled data. Third, we assess whether the effect sizes associated with GDL policies varied plausibly with the stringency of the state-specific regulations. And, fourth, we present some qualified evidence on whether the ostensible public-health benefits of these regulations were attenuated (or amplified) by changes in the traffic-safety outcomes of older teens who reached full licensure under the new policy regime.

In brief, our results suggest that GDL policies have been quite successful at reducing fatalities among teens. More specifically, our results indicate that these regulations reduced traffic fatalities among 15–17-year-olds by at least 5.6%. We find that these effects were monotonically larger in states with more stringent policies. We do not find evidence that the cohorts subject to these new regulations had higher risks upon reaching full licensure. In the concluding section, we discuss the policy implications of these results in more detail.

Table 1
Effective dates (1992–2002) and alternative characterizations of graduated driver licensing laws

State	Effective date of laws with intermediate phase[a]	IIHS characterization of driver licensing and effective dates		
		Marginal	Fair	Good
Alabama	1 October 2002		1 October 2002	
Arizona				
Arkansas	1 July 2002	29 July 1999		
California	1 July 1998			1 July 1998
Colorado	1 July 1999		1 July 1999	
Connecticut		1 January 1992		
Delaware	1 July 1999		1 July 1999	
Florida	1 July 1996		1 July 1996	
Georgia	1 July 1997		1 July 1997	1 January 2002
Idaho	1 January 2001	1 January 2001		
Illinois	1 January 1998		1 January 1998	
Indiana	1 January 1999		1 January 1999	
Iowa	1 January 1999		1 January 1999	
Kansas				
Kentucky		1 October 1996		
Louisiana	1 January 1998		1 January 1998	
Maine	11 August 2000	11 August 2000		
Maryland	1 July 1999	1 January 1992	1 July 1999	
Massachusetts	4 November 1998		1 January 1992	4 November 1998
Michigan	1 April 1997		1 April 1997	
Minnesota		1 January 1992		
Mississippi	1 July 2000	1 July 2000		
Missouri	1 January 2001		1 January 2001	
Montana				
Nebraska	1 January 1999	1 January 1999		
Nevada	1 July 2001	1 July 2001		
New Hampshire	1 January 1998		1 January 1998	
New Jersey	1 January 2001			1 January 2001
New Mexico	1 January 2000		1 January 2000	
New York			1 January 1992	
North Carolina	1 December 1997		1 December 1997	
North Dakota		1 August 1999		
Ohio	1 January 1999	1 July 1998	1 January 1999	
Oklahoma				
Oregon	1 March 2000			1 March 2000
Pennsylvania	22 December 1999		22 December 1999	
Rhode Island	1 January 1999		1 January 1999	
South Carolina	1 July 1998	1 July 1998	1 April 2002	
South Dakota	1 January 1999	1 January 1999		
Tennessee	1 July 2001			1 July 2001
Texas	1 January 2002		1 January 2002	
Utah	1 July 1999		1 July 1999	
Vermont	1 July 2000		1 July 2000	
Virginia	1 July 2001	1 July 1996	1 July 2001	
Washington	1 July 2001			1 July 2001
West Virginia	1 January 2001	1 January 2001		
Wisconsin	1 July 2000	1 July 2000		
Wyoming				

Source: IIHS (2000, 2002), Lexis-Nexis searches and conversations with state officials.

[a] Note that some states have an intermediate phase in their GDL program, but have been characterized by the IIHS as having a marginal program. Some states have marginal or fair programs that do not include an intermediate phase, and thus, they are not categorized as having a graduated driver licensing law.

Table 2
Insurance Institute for Highway Safety Taxonomy of Licensing Systems for Young Drivers

IIHS characterization	Definition
Good	Both of the following two conditions are required: A mandatory learner's period of at least 6 months An "optimal" restriction on the initial license that lasts until age 17 (either a night driving restriction beginning by 10 p.m. or allowing no more than one teen passenger)
Fair	Either of the following two conditions are required: An "optimal" night driving or passenger restriction lasting until age 17 without regard to the learner's period A mandatory learner's period of any length and an "optimal" night driving or passenger restriction lasting until age $16\frac{1}{2}$
Marginal	Any of the following three conditions is required: A mandatory learner's period of any length and either a night driving or passenger restriction A mandatory learner's period of at least 6 months Any night driving or passenger restriction on the initial license
Poor	A mandatory learner's period less than 6 months and no restrictions on night driving or passengers

2. Graduated driver licensing

Graduated driver licensing (GDL) regulations differ from prior state licensing procedures largely because they establish three distinct licensing stages. However, the exact requirements associated with each stage vary across states in several dimensions. Nonetheless, a common feature of the initial "learning phase" is that young drivers can only drive in the presence of a licensed driver over the age of 21. States implementing GDL regulations often increased the age at which teens could obtain these initial permits as well. Furthermore, GDL reforms typically required that teens hold these permits for at least 6 months, during which the driver must log 30–60 h of supervised driving. In the "intermediate phase" the young driver is allowed to operate a vehicle without supervision but only during daylight and early evening hours (e.g., only from 5 a.m. to 10 p.m.). In addition, they are typically allowed to have no more than one or two passengers in the car. The "full privileges phase" begins upon the successful completion of the earlier phases and at minimum ages as high as 18.

The Insurance Institute for Highway Safety (IIHS) has recently developed an explicit taxonomy for characterizing the overall restrictiveness of these multi-dimensional state-licensing regulations. Some of the evaluations we present in this study use the IIHS definitions to assess whether the effectiveness of the new licensing regulations were plausibly related to their restrictiveness. Specifically, the IIHS divides state-licensing procedures into four categories: good, fair, marginal and poor.[1] Table 2 provides the definitions used by the IIHS for each designation. Although most states introduced GDL regulations during the last decade (Table 1), only seven states met the IIHS standard for "good" procedures. Over

[1] For states and years when the IIHS ratings were not available, we applied their published criteria in assigning a score. Importantly, the IIHS assigns ratings based on the date a law was enacted and not when it was adopted. Thus, we revised the published GDL ratings to correspond to the dates of GDL adoption.

the period 1992–2002, 15 states had "marginal" programs and 27 had "fair" programs, at least at some point.[2]

The proponents of GDL regulations argue that these policies will save lives simply because they limit the amount of driving done by teen drivers, particularly in high-risk settings. Furthermore, these regulations may allow novice drivers to develop critical driving skills and experience under relatively safe and amenable conditions (e.g., daytime, with supervision). However, whether these new licensing procedures have actually been effective in reducing traffic fatalities ultimately depends on a number of factors. Most obviously, the degree of compliance and enforcement associated with these policies is likely to play an important role. In particular, some initial skepticism may be warranted because several key features of GDL regulations are clearly difficult to monitor and enforce (e.g., driver supervision, logged hours, passenger restrictions). This ambiguity implies that whether these regulations have an effect is an open, empirical question.

A somewhat more subtle concern about the overall effectiveness of these regulations involves how they might influence driving behavior over the life cycle. More specifically, the life-saving benefits of these regulations may be attenuated (or even negated entirely) if they merely shift risk-taking behavior to the full-licensure period available only to the older teens in reform states. This sort of risk shifting would be plausible if young drivers learn about the dangers of risky driving largely through their own lived experiences.[3] Alternatively, it could also be the case that GDL regulations increase the traffic safety of older teens who have reached full licensure. For example, more supervised driving with a parent under a GDL law could lead to the formation of better driving skills that are sustained into later years. We present some evidence on these issues by examining how GDL regulations influenced traffic-safety outcomes for older teens. However, we also argue that a fuller treatment of these questions will require, among other things, additional years of data on the traffic-safety outcomes of post-reform cohorts as they advance to their late teens.

The few prior studies that have examined the effects of GDL policies have largely focused on the outcomes within a particular state. For example, Ulmer et al. (1999) concluded that Florida's GDL reforms reduced the crash rates among 15–17-year-old drivers by 9%. Similarly, Shope et al. (2001) concluded that Michigan's GDL regulations reduced the crash rate for 16-year-old drivers by 25%. Foss et al. (2001) concluded that North Carolina's GDL regulations reduced the rate of fatal crashes involving 16-year-old drivers by 57%. The research designs used in these state-specific studies attempted to isolate the causal effect of these policies by implementing one of two identification strategies. One study (Ulmer et al., 1999) used the contemporaneous, within-state changes in a neighboring state that had not yet implemented GDL regulations (Alabama) as a control for unobserved, time-

[2] It should be noted that there is not an exact correspondence between new GDL (i.e., 3-stage) regulations and licensing regulations deemed stricter by the IIHS classification. For example, the state of New York met the IIHS standard for a "fair" policy, but was not categorized as having a GDL law, because its program did not have 3-stage licensing (Table 1).

[3] Dee and Evans (2001) present evidence that the life-saving benefits of the movement to minimum legal drinking ages of 21 were attenuated by increases in the traffic fatality risks for 22–24-year olds.

varying determinants. The other two studies effectively relied on the changes in traffic-safety outcomes for older cohorts within each reform state because their outcomes should have been largely unaffected by these reforms.

The evaluation results we present are based on econometric specifications that generalize each of these approaches and apply them to national state-by-year panel data for the 1992–2002 period. More specifically, we implement “difference-in-differences” (DD) specifications that effectively rely on the contemporaneous changes in non-reform states to control for trends unrelated to GDL regulations. However, we also present the results of “difference-in-differences-in-differences” (DDD) specifications that purge the unobserved determinants associated with each state–year cell by relying on the outcomes among the older cohorts who were not directly affected by GDL regulations.

A recent study by Eisenberg (2003), which used DD specifications, also presented evidence on the effects of GDL regulations on fatal crashes. Specifically, using state-by-year panel data for the 1982–2000 period, Eisenberg found that GDL reforms reduced total fatal crash rates by 4% and fatal crash rates involving 16–20-year-old drivers by 9.4%. However, because that otherwise compelling study focused largely on the effects of drunk-driving policies, it has some potentially important shortcomings with respect to evaluating the GDL regulations. Perhaps the most important concern is that the study’s sample period (1982–2000) implied there was relatively little within-state variation in GDL regulations.[4] By using the most recently available fatality data, our evaluations have the statistical power to assess whether the effects of GDL regulations varied in a plausibly monotonic manner with their stringency.[5] Our evaluations also discriminate more finely between those age cohorts directly affected by GDL laws (i.e., 15–17-year-olds) and the older teens (i.e., 18–20-year-olds) for whom GDL policies could have had very different consequences. Furthermore, as noted above, we use state-by-year data on the traffic-safety outcomes of older adults (i.e., 21–23- and 24–26-year-olds) to validate our basic DD results and to identify DDD specifications.

3. Data

The analyses presented here are based on a panel of annual state-level data from 1992 to 2002. The data on traffic fatalities were drawn from the Fatality Analysis Reporting System (FARS). The FARS, collected by the National Highway Traffic Administration, is a census of all motor vehicle crashes involving a fatality. To be included in this census of crashes, a crash had to involve a motor vehicle traveling on a roadway customarily open to the public and had to result in the death of a person (either an occupant of a vehicle or a non-motorist) within 30 days of the crash. From the FARS, we constructed the number of overall (i.e., driver, passenger and non-motorist) traffic fatalities for individuals aged 15–17, 18–20,

[4] More specifically, the sample mean for the GDL variable in Eisenberg (2003) was only 4.1%. This is because the first GDL was not implemented until 1996. That state (Florida) was followed by 3 states in 1997, 6 states in 1998, 11 states in 1999, 6 states in 2000, 8 states in 2001 and 3 states in 2002.

[5] Furthermore, avoiding an unusually long “pre-treatment” period combined with a short “post-treatment” period could also attenuate the uncertain biases related to unobserved, state-specific trends.

21–23 and 24–26 by state and year.[6] As in much of the prior literature, Alaska, Hawaii and the District of Columbia were excluded from the analyses, which imply a final data set with information over 11 years from 48 states ($n = 528$) for each of the four age cohorts.

We use two alternative approaches to characterize the GDL programs. First, we use a simple binary indicator for whether, in a given year, a state's licensing regulations included an intermediate phase that was required before a beginning teenaged driver could advance to full licensure (i.e., a 3-stage licensing system). This simple binary variable indicates the presence of GDL regulations. For GDL policies that became effective during a calendar year, the appropriate fractional value is used. As Table 2 indicates, some 38 states have enacted GDL programs under this definition and most have been in place since 1999. As discussed earlier, we also distinguish among GDL regulations by using the "good, fair, marginal and poor" taxonomy developed by the Insurance Institute for Highway Safety (IIHS). More specifically, we have created three dummy variables that indicate the fraction of a particular calendar year that a state had good, fair or marginal licensing regulations. States observed in years when they had poor licensing regulations are the reference category.

Our specifications also control for a variety of other potentially relevant determinants varying within states over this period. For example, because the specification we present below focuses on fatality *counts*, the natural log of the relevant age-specific population (i.e., 15–17-, 18–20- and 23–30-year-olds) is treated here as a control variable that reflects each state's exposure to risk in a given year. Other state-by-year controls include four binary indicators for state laws, related to drunk driving, which have been shown to be important predictors of teenage traffic fatalities (Dee, 2001). The variables indicate whether it is illegal per se to drive with a blood alcohol concentration (BAC) of 0.08, whether it is illegal per se to drive with a BAC of 0.10, whether the state's licensing authority is allowed to suspend driving privileges before any court action related to a charge of drunk driving ("administrative license revocation") and whether it is illegal per se to drive with a positive BAC if the driver is not of legal drinking age ("zero tolerance" laws).

Two binary indicators are also included for mandatory seat-belt laws. Seat-belt laws with primary enforcement allow the police to directly cite a motorist for not wearing a belt. Under secondary enforcement of a seat-belt law, a motorist can only be cited for a violation if they are pulled over for some other reason. Seat-belt laws have been shown to reduce motor vehicle fatality rates (e.g., Evans and Graham, 1991; Morrisey and Grabowski, in press). Two other binary indicators identify those states that have increased their rural interstate speed limit to 65 miles per hour or to 70 or more miles per hour. There is recent empirical evidence that higher rural interstate speed limits have increased the motor vehicle fatalities on these roads (e.g., Greenstone, 2002). We also control for the state unemployment rate as earlier work has recognized the importance of controlling for macroeconomic factors in analyses of state motor vehicle fatality rates (e.g., Evans and Graham, 1988). In some specifications, we also control for other unobserved determinants varying within states over time by introducing trend variables that are specific to each state. Table 3 presents the means and standard deviations of the key variables.

[6] Some prior studies focused only on driver fatalities. However, because we are concerned with the overall policy impact of these policies, we examine total traffic-related fatalities (e.g., passengers, pedestrians as well as drivers).

Table 3
Descriptive statistics for state panel data: 1992–2002 ($N = 528$)

Variable	Mean	S.D.
Traffic fatalities, ages 15–17	52.88	44.70
Traffic fatalities, ages 18–20	76.99	71.61
Traffic fatalities, ages 21–23	67.10	63.84
Traffic fatalities, ages 24–26	52.31	52.01
Graduated driver licensing law	0.23	0.41
Graduated driver licensing law rated good	0.03	0.17
Graduated driver licensing law rated fair	0.20	0.39
Graduated driver licensing law rated marginal	0.13	0.33
Graduated driver licensing law rated poor	0.64	0.47
65 MPH speed limit	0.59	0.48
70+ MPH speed limit	0.35	0.47
Seatbelt law – primary enforcement	0.24	0.42
Seatbelt law – secondary enforcement	0.71	0.45
Illegal per se at 0.08 BAC	0.29	0.44
Illegal per seat 0.10 BAC	0.66	0.46
Administrative license revocation	0.75	0.43
Zero tolerance law	0.76	0.42
State unemployment rate	0.050	0.014

Alaska, Hawaii and the District of Columbia are omitted.

4. Differences-in-differences (DD) approach

4.1. Specification

Two concerns motivated our choice of a basic empirical specification. First, we wanted to exploit the nature of the available panel data by evaluating a model that accommodates the presence of both state and year fixed effects. Second, we wanted a specification that acknowledged the count nature of the underlying fatality data. Traffic-safety evaluations often focus on fatality or crash *rates*, which are denominated by population size or number of miles traveled. However, because the fatality counts we examine are constructed relatively finely by age, employing a conventional fatality rate could lead to weak statistical power by substantially reducing the signal-to-noise ratio.[7] In particular, a substantial fraction of the state–year cells in our sample have only a limited number of fatalities. For example, over 10% of our state–year observations in the 15–17-year-old age cohort have fewer than 10 fatalities and nearly a third have fewer than 25.

To accommodate both the count nature of the fatality data and the presence of fixed effects, we rely on the conditional maximum likelihood (CML) approach to negative binomial models, which was developed by Hausman et al. (1984). More specifically, we assume

[7] The measurement error associated with fatality rates would be exacerbated in this context by the fact that the population data specific to state, year and age cells are estimated for intercensal years. As an aside, we also eschew the use of fatal *crash* rates as a dependent variable because the number of deaths – not the number of crashes – is likely to be seen as more salient for policy deliberations. This distinction may be particularly relevant in this context. More specifically, because GDL regulations often included passenger restrictions, an analysis of crashes could seriously understate any policy-induced reductions in mortality.

that the number of traffic fatalities within a state and year, y_{st}, follow a negative binomial distribution with parameters $\alpha_s\lambda_{st}$ and φ_s where α_s is a state fixed effect and φ_s is a state-specific overdispersion parameter (Cameron and Trivedi, 1998). We make the conventional assumption about an exponential functional form for λ_{st} so that the mean of y_{st} is given by

$$E(y_{st}) = \alpha_s\lambda_{st} = \alpha_s \exp(X_{st}\beta + G_{st}\gamma + \nu_t) \tag{1}$$

The variable, X_{st}, contains the control variables, G_{st} the indicator for GDL policies and ν_t represents year fixed effects. The likelihood function based on this specification conditions on the total number of fatalities within each state over the sample period. This conditional approach cancels out the state fixed effects, α_s, allowing us to identify the parameter of interest, γ, without concern for "incidental parameters" bias. Given our assumption about an exponential functional form, these CML estimates can be interpreted as the proportionate change in the given traffic fatality count.

This specification identifies the effects of GDL policies conditional on any unobserved, state-specific (but time-invariant) determinants of fatality counts.[8] These determinants could include difficult-to-measure variables like the degree of law enforcement, the condition of roadways and weather patterns. The year fixed effects control unobserved determinants that vary year to year for all states (e.g., changing social more about drunk driving, improvements in automotive and medical technologies). However, the critical identifying assumption for results based on this specification is that the within-state variation in states that did not implement GDL reforms ("control" states) provides a valid control for the within-state variation in states that did ("treatment" states). We examine the validity of these conventional DD assumptions in three distinct ways. First, we present some evidence on the robustness of our key results to incrementally introducing state–year control variables. Second, we present some ad hoc evidence on whether the implementation of GDL policies led or lagged changes in traffic fatalities. And, third, we evaluate versions of this model where the dependent variable refers to fatalities among *older* cohorts who were not directly affected by the new GDL policies. If this specification were generating reliable inferences, we would expect to find that GDL regulations have little or no effect on fatalities among older cohorts.

It should also be noted that a recent study by Bertrand et al. (2004) has drawn attention to another problem that may plague DD evaluations like those presented here. Specifically, they have shown that serial correlation within states can lead to overstated precision in DD evaluations, especially when the "treatment" variable has a pre/post-structure. We suspect that this type of bias is less likely to be relevant in this context because we employ a relatively short panel of only 11 years. However, we examined the empirical relevance of this concern for our evaluations in two ways. First, a recommended method for addressing this issue is to cluster the standard errors within states (Bertrand et al., 2004). Because this procedure is less straightforward in a negative binomial model with fixed effects, we compared the results of least-squares count models with and without state-specific clustering. We found

[8] If we set $\alpha_s = \exp(\delta_s)$ in Eq. (1), this model appears to allow arbitrary intercepts for each state in the conventional, linear manner. However, Allison and Waterman (2002) criticize the Hausman et al. (1984) approach, arguing that it is not a true fixed effect method. Fortunately, they also present simulation evidence that an *unconditional* negative binomial model with dummy variables yields good results despite ignoring "incidental parameters" bias. We found that both approaches generated very similar results in this application.

that clustering actually *reduced* the standard errors by roughly 10%. Therefore, this robustness check suggests that, if anything, the negative binomial results we present understate the precision of our point estimates. Second, our “counterfactual” models based on older cohorts who were not directly affected by GDL reforms provide another useful check. More specifically, if the presence of serial correlation had led to substantially overstated precision, we would expect to find that GDL policies have a highly significant effect for this older age cohort.

4.2. Results: the effect of GDL laws on teen traffic fatalities

The main evaluation results from the DD models for 15–17-year-old traffic fatalities are reported in Table 4. The first column presents the results for the sparsest specification, which includes only the state and year fixed effects, the natural log of the state population of 15–17-year-olds, and a single dummy variable representing the presence of a GDL law. The statistically significant estimate from this specification suggests that GDL laws reduced fatalities among 15–17-year-olds by 6.8%. However, the next two models introduce explicit regressors that control for the influence of other potentially important and confounding determinants of teen traffic safety. Model 2 introduces the unemployment rate, speed limit laws, and laws dealing with seatbelt enforcement. Model 3 introduces the alcohol control laws. Interestingly, the results from Model 2 do not differ significantly from Model 1. Introducing the unemployment rate, speed limit laws and seat-belt laws only reduced the estimated effect of GDL laws by 4.4% (i.e., [0.068 − 0.065]/0.068). However, Model 3 clearly indicates that the omission of alcohol control laws can lead to somewhat misleading inferences about the efficacy of GDL laws. The estimated effect was reduced by 17.6% relative to the base specification in Model 1. However, this difference is still small relative to the sampling variation. Furthermore, this preferred specification still suggests that GDL laws were effective at saving teen lives, generating a statistically significant reduction of 5.6% in traffic fatality rates among 15–17-year-olds.

Model 4 introduces a state-specific linear time trend to control for unobserved within-state variation over time in teen traffic fatalities. The estimated effect is noticeably larger, suggesting that the introduction of GDL laws reduced fatalities among 15–17-year-olds by 9.8%. This estimate is consistent with the hypothesis that there is some downward bias in the traditional fixed effects specification due to unobserved within-state variation in teen traffic fatalities. The DDD results we present below suggest this as well. However, a potential caveat with this approach is that it may re-introduce the “incidental parameters” problem addressed by Hausman et al.’s (1984) fixed effect model.

The final column in Table 4 presents estimation results from a specification that replaces the single GDL dummy variable with a set of dummy variables representing good, fair and marginal ratings of GDL laws. The poor category is the omitted reference group in the table. This model generates the plausible result that the effect of GDL is largest for those systems rated as good (i.e., those most restrictive systems), next largest for those fair systems and smallest for those marginal systems.[9] Good GDL systems generated a statistically significant 19.0% decrease in 15–17-year-old traffic fatalities. Given that relatively

[9] Furthermore, the hypothesis that these three point estimates are equivalent can be rejected (p-value = 0.0088).

Table 4
The estimated effects of graduated driver licensing on traffic fatalities among 15–17-year-olds

Variable	Model 1	Model 2	Model 3	Model 4	Model 5
Graduated driver licensing law	-0.068^{***} (0.026)	-0.065^{**} (0.026)	-0.056^{**} (0.026)	-0.098^{***} (0.032)	–
Graduated driver licensing law, good	–	–	–	–	-0.190^{***} (0.046)
Graduated driver licensing law, fair	–	–	–	–	-0.059^{**} (0.027)
Graduated driver licensing law, marginal	–	–	–	–	−0.046 (0.043)
65 MPH speed limit	–	0.021 (0.049)	0.034 (0.051)	−0.0004 (0.083)	0.022 (0.050)
70+ MPH speed limit	–	0.054 (0.068)	0.063 (0.068)	0.032 (0.101)	0.048 (0.068)
Seatbelt law – primary enforcement	–	−0.087 (0.077)	−0.084 (0.077)	−0.117 (0.104)	−0.069 (0.076)
Seatbelt law – secondary enforcement	–	−0.046 (0.067)	−0.052 (0.067)	−0.100 (0.092)	−0.041 (0.067)
Illegal per se at 0.08 BAC	–	–	−0.060 (0.068)	-0.177^{*} (0.107)	−0.074 (0.066)
Illegal per se at 0.10 BAC	–	–	−0.061 (0.058)	-0.174^{*} (0.099)	−0.065 (0.057)
Administrative license revocation	–	–	0.051 (0.034)	0.023 (0.047)	0.053 (0.034)
Zero tolerance law	–	–	0.041 (0.026)	0.033 (0.035)	0.025 (0.026)
State unemployment rate	–	1.02 (1.22)	1.33 (1.21)	−0.257 (1.61)	0.41 (1.23)
ln(population)	0.63^{***} (0.19)	0.57^{***} (0.20)	0.67^{***} (0.20)	0.71 (0.48)	0.77^{***} (0.21)
State-specific trends?	No	No	No	Yes	No

These estimates are based on negative binomial regressions that condition on state fixed effects (Hausman et al., 1984). Each model includes year fixed effects as controls. There are 528 observations in each model (48 states over 11 years). Standard errors are presented in parentheses.

* Statistically significant at the 10% level.
** Statistically significant at the 5% level.
*** Statistically significant at the 1% level.

few states implemented "good" GDL laws (i.e., only 3% of our state–year observations), this strikingly large point estimate should be interpreted cautiously. Fair systems were associated with a statistically significant 5.9% decrease in 15–17-year-old traffic fatalities. Although the estimate associated with the marginal category was not statistically significant at conventional levels, these systems were found to generate a 4.6% decrease in fatalities.

It is important to note that many of the other state laws included in the various specifications in Table 4 were not statistically significant despite the fact that earlier work (e.g., Dee, 2001; Eisenberg, 2003) found alcohol control and seat-belt laws to be important towards decreasing teen traffic fatalities. We argue that this is only an apparent inconsistency. Our study period spans the within-state variation in GDL policies but excludes much of the variation in other state policies. By comparison, Dee (2001) considers the period 1982–1998 and Eisenberg (2003) examines the period 1982–2000. Given our shorter and more recent study period (1992–2002), we have weak power for assessing the effect of these other state laws on teen traffic safety.

The fact that our evaluation results are relatively robust to the introduction of additional controls and exhibit a plausible monotonicity with respect to the policy stringency (Table 4) suggests the absence of undiagnosed specification errors. However, as an additional check on our results, we also examined how teen traffic fatalities varied with respect to the within-state timing of new GDL regulations. More specifically, we constructed 10 dummy variables that indicated whether a particular state–year observation was 1–5 years prior to GDL implementation (five variables), in the implementation year (one variable) and 1–4 or more years afterwards (four variables). We then estimated our basic DD model with these variables as the key independent regressors.[10] Collectively, the point estimates on these 10 variables map out how teen traffic fatalities varied within each reform state in the years prior to and after GDL implementation. The reference category for these point estimates is the state–year observation that is 6 or more years prior to GDL implementation.

Researchers will sometimes use this sort of dynamic evidence to assess whether there was any "policy endogeneity". More specifically, a distinct trend in teen traffic fatalities in the years *prior* to the adoption of GDL laws would suggest that their within-state timing was not independent of the contemporaneous changes in teen traffic safety. In contrast, seemingly random variation in teen traffic fatalities in the years prior to reform and a distinct trend break in the post-reform period would suggest that these policies actually had an independent effect. An additional benefit of this analysis is that the pattern to the post-reform point estimates can suggest whether the new policies appears to have had sustained effects.

We present the 10-point estimates from this regression model graphically in Fig. 1. The results of this regression analysis are qualified by the fact that the standard errors associated with these point estimates are generally quite large. However, the pattern suggested by these results is still broadly consistent with this study's maintained assumption that the within-state timing of GDL reforms was independent of state trends in teen traffic safety. Specifically, in the pre-reform period, teen traffic fatalities varied both positively and negatively. However, they are lower in all four time periods after GDL implementation.

[10] We find similar results for this exercise when we use the DDD specification we introduce below.

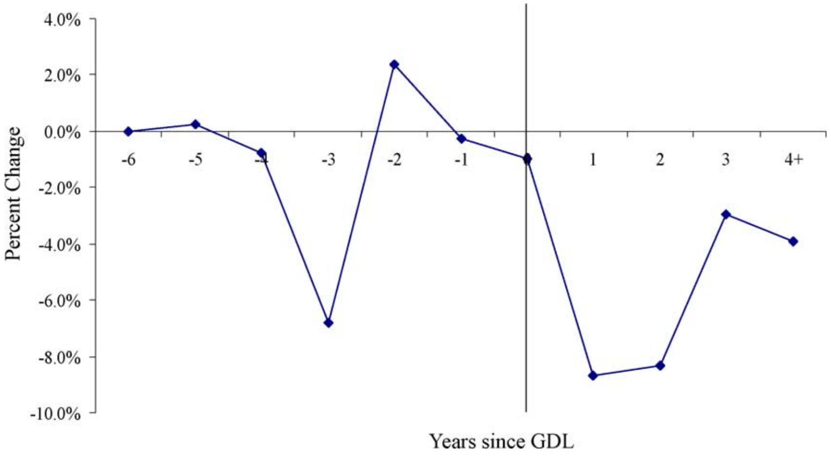

Fig. 1. Percent change in teen traffic fatalities relative to years since GDL.

Interestingly, the results in Fig. 1 also suggest that the life-saving benefits of GDL regulations were noticeably smaller after 3 or more years. This could indicate that GDL restrictions received a great deal of publicity when first enacted, but were perhaps enforced less vigorously over time. However, because relatively few states in our sample have 3 or more years of experience with GDL regulations, we conclude that the long-term effects of GDL laws should be considered an open empirical issue that can be addressed as additional FARS data become available.

The results in Table 4 and Fig. 1 consistently suggest that GDL regulations led to rather large and statistically significant reductions in teen traffic fatalities. However, these key inferences could still be biased by a variety of undiagnosed specification errors. For example, there are undoubtedly other attitudinal, institutional and economic variables that influenced the within-state variation in teen traffic fatalities but are inherently difficult to measure and have necessarily been omitted from our analyses. Fortunately, a fairly compelling and general robustness check can be conducted by exploiting the panel data on traffic fatalities among 18–20-, 21–23- and 24–26-year-olds.

The logic of these "counterfactual" evaluations is straightforward. Because the first birth cohort affected by GDL were 16-year-olds in Florida in 1996, cohorts age 23 or older by 2002 should be largely unaffected by GDL.[11] If the DD models presented here are generating reliable inferences about the true relationship between GDL changes and teen traffic fatalities, then we should also observe, in similarly specified models of traffic fatalities among 24–26-year-olds, small and statistically insignificant effects associated with GDL. In contrast, if there were significant links between the GDL changes and traffic fatalities among 24–26-year-olds, it would suggest the existence of important and overlooked specification errors (e.g., omitted determinants of traffic fatalities that are confounded with GDL

[11] These regulations could conceivably generate traffic safety benefits for older cohorts who may be in fewer accidents caused by younger drivers. However, this sort of negative effect should be relatively small, if not empirically negligible.

Table 5
The estimated effect of graduated driver licensing on traffic fatalities by age

Specification	Ages 15–17 (1)	Ages 18–20 (2)	Ages 21–23 (3)	Ages 24–26 (4)
Model 1	−0.068*** (0.026)	−0.030 (0.027)	0.010 (0.027)	0.011 (0.026)
Model 2	−0.065** (0.026)	−0.023 (0.027)	0.014 (0.026)	0.035 (0.027)
Model 3	−0.056** (0.026)	−0.020 (0.027)	0.012 (0.026)	0.015 (0.027)
Model 4	−0.098*** (0.032)	−0.016 (0.033)	0.004 (0.030)	0.013 (0.033)

See Table 4 for a description of each model. There are 528 observations in each model (48 states over 11 years). Standard errors are presented in parentheses.
** Statistically significant at the 5% level.
*** Statistically significant at the 1% level.

variation). We also report the results of DD specifications based on data for 18–20- and 21–23-year-olds, for whom we would also expect contemporaneous GDL policies to have relatively small effects.

Table 5 presents the key results from these comparative evaluations. For ease of comparison, the GDL estimates from Table 4 are reproduced in column 1 for Models 1–4. Columns 2, 3 and 4 present the GDL estimates across the same four models for 18–20-, 21–23- and 24–26-year-olds, respectively. In all four models and for all three older age cohorts, we observe small and statistically insignificant effects associated with GDL. We interpret these results as providing strong support for the validity of the DD inferences for 15–17-year-olds.

4.3. Results: risk-shifting hypotheses

The DD results presented in Table 4 provided direct insight into the immediate effects of GDL on fatalities among 15–17-year-olds. However, those estimates did not explicitly address whether the life-saving benefits of GDL programs were attenuated (or amplified) by the traffic-safety outcomes among older teens when they reached full licensure under these new policy regimes.[12] As we noted earlier, GDL regulations could merely shift risky driving to older teens if experiential learning is an important component of maturing through these behaviors. Alternatively, by fostering the development of critical driving skills and experience, GDL regulations could generate traffic-safety improvements that are sustained into the period of full licensure.

We first examined this issue by assessing whether the adoption of GDL regulations influenced the prevalence of traffic fatalities among 18–20- and 21–23-year-olds. The results in Table 5 (columns 2 and 3) indicate that GDL regulations had a small, negative (but statistically insignificant) effect on fatalities among 18–20-year-olds and a small positive (but statistically insignificant) effect among 21–23-year-olds. We chose these age cohorts because, even in the most restrictive states, drivers could have unrestricted licenses by age 18. This result suggests that, to the extent GDL regulations had any implications for traffic safety at full licensure, the effects were beneficial among 18–20-year-olds. However, it should be noted that this approach is somewhat crude (and may have weak power) for at

[12] Actually, because, in some GDL states, full licensure was available to 16- and 17-year-olds, our basic DD results would have reflected these responses somewhat.

least two reasons. One is that the states with weaker GDL policies allowed teens to have unrestricted licenses at the age of 16 or 17. A second reason is that the 18–20-year-olds in states that implemented GDL recently would have actually been licensed under the prior regime.

As a more refined check, we also examined traffic fatalities among 18-year-olds more closely. First, we evaluated models that matched 18–20-year-old fatalities to the state GDL law in place 3 years prior when the individuals were 15–17-year-olds. The point estimates from these models were consistently negative, suggesting that the life-saving benefits of GDL policies continued as these cohorts advanced to full licensure. However, these effects were not statistically distinguishable from zero. Second, we matched state–year fatality data for 18-year-olds to an indicator for whether their state birth cohort could only be fully licensed at age 18. The results of DD specifications based on these data also indicate that GDL regulations had small and statistically insignificant effects on the traffic safety of older teens who have reached full licensure. Based on this evidence, our overall conclusion is that there is nothing yet to indicate that GDL regulations had any traffic-safety implications – positive or negative – for teens that have reached full licensure. However, these conclusions are qualified by the fact that, because most GDL programs were adopted recently, relatively few GDL-constrained cohorts have advanced to full licensure. This implies that these issues should be revisited in future research as additional years of FARS data become available.

5. Differences-in-differences-in-differences (DDD) approach

5.1. Specification

An alternative evaluation strategy is to rely explicitly on traffic fatality data among older individuals as possibly better controls for unobserved state- and year-specific traffic fatality shocks. Rather than simply examining state–year observations for 15–17-year-olds, the model also exploits the contemporaneous variation in state–year observations for 18–20-, 21–23- and 24–26-year-olds. More specifically, in the conditional maximum likelihood version of these negative binomial regressions, the mean for the fatality counts with age group i, state s in year t (i.e., y_{ist}) takes the following form:

$$E(y_{ist}) = \alpha_{st}\lambda_{ist} = \alpha_{st}\exp(X_{ist}\beta + (G_{st} \times \mathrm{AGE1517}_i)\gamma + (\omega_i \times \nu_t) + (\omega_i \times \mu_s)) \tag{2}$$

where α_{st} represents fixed effects specific to each state-by-year cell. The terms $(\omega_i \times \nu_t)$ and $(\omega_i \times \mu_s)$ represent unrestrictive interactions between fixed effects for each age group (i.e., ω_i) and the state and year fixed effects (i.e., μ_s and ν_t, respectively). The term $\mathrm{AGE1517}_i$ represents a binary indicator equal to 1 only for observations from the age 15–17-year-old cohort. Because these models condition on state-by-year fixed effects (i.e., α_{st}), the variable, X_{ist}, only includes age-specific state–year variables: the natural log of the state-by-year-by-age population estimates and binary indicators for zero tolerance laws and GDL regulations.

Identification in this "differences-in-differences-in-differences" (DDD) framework relies effectively on comparing the change in the gap between teen and young adult traffic fatality rates in states that did and did not adopt a GDL system.[13] The interactions between the GDL variables, G_{st}, and the age fixed effects, AGE1517$_i$, equals zero for the three older cohorts for whom these laws were presumably irrelevant. This implies that the contemporaneous traffic fatalities among the older age cohorts serve as controls for unobserved factors specific to each state–year cell but unrelated to GDL adoption. The results in Table 5 suggest that this maintained assumption is a valid one. However, it is the case that some 18–20-year-olds (and, to a lesser extent, 21–23-year-olds) were exposed to GDL as 15–17-year-olds. And the results in column 2 of Table 5 suggest that this exposure may have reduced traffic fatalities among 18–20-year-olds. This concern suggests that the DDD results based only on the 15–17- and 24–26-year-old cohorts may be preferable to those including the 18–20- or 21–23-year-old cohorts. However, a related concern about this identification strategy is that GDL policies could have also reduced fatalities among 24–26-year-olds simply because they might have been in fewer accidents caused by novice drivers. Although we cannot dismiss this possibility, the results in Table 5 suggest that this source of bias is empirically negligible. Furthermore, the direction of the bias implied by this concern suggests that our DDD estimates merely understate the true fatality reductions implied by GDL regulations.

As noted above, the DDD approach implicitly assumes that teenagers and young adults share the same traffic fatality shocks in a given state and year that are unrelated to GDL policies. The DD approach, which instead used as controls the within-state traffic fatality shocks among teens in other states, may actually be preferable. Because there is little basis for distinguishing these approaches ex ante, these models are probably best viewed as complementary approaches for exploring the validity of this study's key results. However, one practical problem with the negative binomial version of this DDD model is that it has poor convergence properties when saturated with a full set of interactions between the 48 state fixed effects and 4 age fixed effects. The estimates we present in Table 6 were generated by limiting the number of model iterations to 30. However, as a specification check, we confirmed that the results we report here are similar to those generated by a model that excluded the state-by-age fixed effects to facilitate convergence.[14]

5.2. *Results: the effect of GDL laws on teen traffic fatalities*

Table 6 presents results from five model specifications using the three different older age cohorts as controls for unobserved state and year-specific traffic fatality shocks. Model 1, which includes the 18–20-year-old cohort, resulted in a negative (but statistically insignificant) effect of GDL on teen traffic fatalities. However, when 21–23-year-olds (Model 2) were used as controls, the model indicated a 7.7% statistically significant decrease in

[13] This identification strategy has been used in a variety of other policy contexts including evaluations of the labor market effects of mandated maternity benefits (Gruber, 1994).

[14] Given the "incidental parameters" problem (Hausman et al., 1984), the model excluding the state–age fixed effects may also be preferable on theoretical grounds. We also found that an unconditional negative binomial model led to similar results and had good convergence properties when fully saturated with the fixed effects and their interactions.

Table 6
The effects of graduated driver licensing (GDL) laws on traffic fatalities among 15–17-year-olds, DDD specifications

Variable	Model 1	Model 2	Model 3	Model 4	Model 5
GDL × age 15–17	−0.033	−0.077**	−0.086**	−0.074**	−0.057**
	(0.034)	(0.034)	(0.037)	(0.031)	(0.028)
Sample size	1056	1056	1056	1584	2112
Cohorts included					
15–17-year-olds	X	X	X	X	X
18–20-year-olds	X				X
21–23-year-olds		X		X	X
24–26-year-olds			X	X	X

These estimates are based on negative binomial regressions that condition on fixed effects specific to each state–year cell (Hausman et al., 1984). Each model includes a binary indicator for a zero tolerance law, the natural log of the age-specific population, year-by-age fixed effects and state-by-age fixed effects. Standard errors are presented in parentheses.

** Statistically significant at the 5% level.

15–17-year-old traffic fatalities. This effect was even larger (8.6%) in Model 3 in which 24–26-year-olds were included as controls. When the 21–23 and the 24–26 age cohorts were combined as controls (Model 4), GDL decreased teen traffic fatalities by 7.4%.

The final column (Model 5) includes all three of the older age cohorts as controls. This specification indicates that GDL decreased teen traffic fatalities by 5.7%. This statistically significant effect is almost identical to the estimate (5.6%) from the preferred DD specification (Table 4, Model 4). Although the DD and DDD models rely on different identifying assumptions, the comparative results are quite complementary. Both approaches suggest that GDL regulations had large negative effects on teen traffic fatalities.

6. Conclusions

Graduated drivers licensing (GDL) programs attempt to promote traffic safety by providing new teenaged drivers with driving experience in progressively more independent situations. These driving regulations have been widely adopted by states over the last several years. This study evaluated the effects of these programs on teen traffic fatalities. Three major findings emerged from this analysis. First, GDL programs have been quite effective in reducing traffic fatalities among 15–17-year-olds. Our analysis indicates that the average GDL program led to a reduction in fatality counts of at least 5.6%. Second, our results also suggest that there are substantive differences in the effectiveness of alternative GDL programs. In particular, more restrictive policies (i.e., those characterized as “good” by the Insurance Institute for Highway Safety) appear to have reduced motor vehicle fatalities among 15–17-year-olds by 19%. The hallmarks of these particular GDL regulations are minimum time limits in the learner’s stage, hours and passenger limits in the intermediate stage, and a minimum age at which one could have a full license. In contrast, “fair” programs, which lack some of these features, appear to have reduced teen traffic fatalities by only 6%. Some caution must be exercised in accepting the results for the “good” programs because only seven states implemented such policies, most of them recently. Nonetheless, the clear implication of these findings is that more stringent GDL programs are markedly more effective in reducing teenage motor vehicle fatalities. If teenage motor vehicle fatalities are to be reduced further, more stringent GDL programs appear to be one of the few successful tools available to policymakers. And, third, we investigated whether GDL regulations had any traffic-safety implications for teens when they reached full licensure. Our results provided preliminary evidence that they did not.

Rough calculations based on our estimates suggest that GDL regulations saved an appreciable number of young lives. More specifically, in 2002, there were 2215 traffic fatalities among 15–17-year-olds in the 38 states that had implemented GDL. A 5.6% effect size implies that there would have been 131 additional teen fatalities in these states annually if they had not adopted these new licensing regulations. Furthermore, the 10 states in our sample that did not introduce GDL by the end of 2002 had 409 traffic fatalities among 15–17-year-olds. Our results imply that implementing GDL regulations would prevent at least 23 of these deaths annually.

From a policy perspective, these estimates can be used to conduct a “back-of-the-envelope” welfare analysis of the hypothetical adoption of GDL in the 10 states without such regulations by the end of 2002. A recent meta-analysis (Viscusi and Aldy, 2003) suggests that the value of a statistical life for prime-aged workers has a median value of about $7.3 million (in 2002 dollars) in the United States, implying that 23 young lives saved in 2002 would, at a minimum, be valued at $167.9 million. On the cost side, the administrative burden associated with GDL is fairly trivial in that these policies typically require (at most) one additional visit to the license examiner and only minimal additional law enforcement activities.

However, restricted driving during the late evening hours and with other teen passengers is probably the most significant cost of GDL, generating disutility among constrained teens and their parents. The magnitude of these costs is difficult to quantify. However, a crude but useful point of reference is to compare the dollar value of the lives in these 10 states ($167.9 million) to the number of 15–17-year-olds (i.e., 1.9 million individuals), who would be subjected to regulations in these states. These numbers imply that the dollar benefit, in terms of lives saved, per constrained teen is roughly $88. Many teens might be willing to pay this amount for the privilege of full licensure. However, though this rough calculation suggests that GDL policies may generate costs in excess of their benefits, at least two caveats are worth bearing in mind. First, this exercise ignored the benefits from a reduction in injuries sustained in both fatal and non-fatal crashes.[15] And, second, many policymakers and citizens are likely to reject such cost–benefit appraisals in favor of an unapologetically paternalistic view of the desirability of these licensing policies.

These cost–benefit questions are one of several that merit further scrutiny. Another question that should be revisited as additional data become available involves the relative effects of the most restrictive GDL regulations. Additional years of FARS data will also make it possible to identify more precisely whether GDL policies influenced traffic safety after full licensure and whether states can sustain the traffic-safety benefits of GDL policies

[15] Most studies analyzing the value of a statistical injury have estimates in the range of $20,000–70,000 per injury (Viscusi and Aldy, 2003).

beyond their initial implementation. Nonetheless, our results suggest that, despite these caveats, GDL regulations have been highly effective at limiting the leading cause of fatalities among young adults.

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