Are Smarter Drivers Safer Drivers?*

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Abstract: This paper documents a large educational gradient in traffic fatality rates and investigates its source. Compared to individuals with a college education, those with at most a high school diploma are more than four times as likely to die in a traffic accident, a gradient exceeding that for all-cause mortality. Triple-difference estimates using state-level changes in dropout ages finds that a one year increase in the dropout age reduces fatal accident involvement among younger drivers by 5-6%. This decrease results both from improvements in driving skill or prudence and a reduction in the opportunity to drive while obtaining further education.

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1. Introduction

Are smarter drivers safer drivers? It is hard to know. While the overall effect of education on health has received much attention in the literature, the work that specifically addresses traffic safety is limited to two studies, discussed below, which document that better-educated individuals engage in more safety-related behaviors related to driving. No studies document even the raw relationship between education and traffic fatalities, much less estimate the extent to which this relationship is causal. We do both in this paper, contributing to literatures on traffic safety and the health effects of education, the latter of which has scarcely considered accidental causes of death or the mortality gradient among young adults, who are well-represented in traffic accidents.

We find that the raw educational gradient in traffic fatality rates is tremendous, greater even than the gradient in overall mortality. Compared to people with a bachelor's degree, high school dropouts are four times more likely to die in a traffic accident; among younger adults, the multiple exceeds six. To estimate the causal effects of education, we leverage state-level increases in the dropout age in the U.S. over the last four decades. We relate these increases to driver involvement in fatal accidents, using a triple-difference analysis that accounts for almost any conceivable influence on traffic fatality rates. For drivers over 25, there is no discernable effect. Younger drivers are a different story. A one year increase in the dropout age lowers fatal accident involvement among drivers 16-25 by about 5%--a sizeable effect that is comparable to that of many traffic safety policies. While it is not possible to quantify all the channels that generate this result, at least two are important: an improvement in skills or safety orientation resulting from additional education, and increased enrollment of young adults in high school and college, which diverts them from activities that are associated with more driving.

2. Education, Health, and Traffic Safety: Existing Evidence

Many studies have examined how education affects health. The survey of Lochner (2011), for example, identifies fifteen such studies within the previous five years; several others have been published since then. On the whole, this literature finds some positive effects of education on health, but studies' findings vary widely.

While these studies investigate a panoply of health behaviors and outcomes, the literature on education and mortality specifically is neither voluminous nor decisive. Lleras-Muney (2005) and Buckles et al. (2015) find that education reduces mortality, while Mazumder (2008) and Clark and Royer (2013) find a small effect in the opposite direction, and Albouy and Lequien's (2009) and Black et al.'s (2015) estimates are statistically insignificant. The recent review by Galama et al. (2018) finds that even within one country, Sweden, four recent studies conflict, with two finding little effect of education on mortality and two finding a large effect.

None of these studies address traffic fatalities specifically or even accidental causes of death generally. However, several others, also reviewed in Lochner (2011), investigate the pathways through which education might improve health. Several, such as diet, smoking, and exercise, are less relevant to traffic safety, but others are more relevant: schooling may reduce stress, improve decision-making, ameliorate prudence or self-control, or increase the resources available to purchase health-related inputs (such as safer automobiles). Three studies relate to traffic safety. Cutler and Lleras-Muney (2010) finds roles for cognitive skills and resources, and shows that education and seat belt use are positively correlated; Ross and Wu (1995) report similar findings, as does the working paper of Cox and Grant (2016). To date, these are the only studies linking education and traffic safety in any way. There is more work to be done.

In summary, despite a large literature on the effect of education on health, we know little about the relationship between education and traffic safety specifically. The literature that does exist does not provide a convincing basis to confidently assert that such a relationship should or should not exist: existing mortality studies are too few and too contradictory in their findings, while the relevant "pathways" literature is even smaller. Furthermore, traffic safety is unique in this literature in being an accidental cause of death, which affects the (relatively) young more than the elderly, and which has less to do with physiological behaviors such as diet than with psychological factors such as self-control. These reasons all justify a focused investigation on the link between education and traffic safety.

3. The Association between Education and Traffic Safety

We begin by documenting the raw association between education and traffic safety. The U.S. Vital Statistics records demographic information, including the highest level of education completed, and causes of death for every fatality in the United States. We selected all deaths within the U.S. proper (no territories) for all individuals aged 25 and over for the year 2014, which comes towards the end of a long period of relative stasis in traffic safety (Grant, 2021) and largely pre-dates the explosion of SUV's on U.S. highways and the increased pedestrian fatalities that are associated with it (Hu and Cicchino, 2022).

Educational achievement in the Vital Statistics is broken down into eight groups, ranging from individuals who never progressed beyond the 8th grade to those receiving doctorates. The U.S. Census Bureau publishes population estimates for each of these groups, broken down by the same demographics available in the Vital Statistics. Thus we can calculate the relative fatality risk by education, for the full population or any selected demographic.

These are presented in Table 1 for three "underlying causes of death": motor vehicle accidents; “external causes,” which include motor vehicle accidents, other accidents, homicides, and suicides, in roughly equal number; and all causes of death. Reading down each column, education increases and the relative risk generally falls.

Across all three causes of death the education gradient is sizeable. The death rate of college graduates, collectively, is at most a third of high school graduates, which is in turn less than that of high school dropouts. For motor vehicle accidents specifically, the overall gradient is (with the intriguing exception of the lowest education group) larger than it is for all external causes of death and for all causes of death. High school dropouts are five times more likely to die of a motor vehicle accident than are individuals with doctorate or professional degrees.

This gradient applies across all demographic groups, but there is demographic variation. It is somewhat larger for men, who perish more frequently in these accidents, than women. It is also much larger at younger ages, no matter which cause of death is considered. Individuals between the ages of 25 and 34 with advanced education have one tenth the fatality risk of high school dropouts. In general, this difference is relatively inconsequential, because most deaths occur at higher ages—but motor vehicle accidents are a distinct exception, in which most deaths occur before age 45.

To put these differences in context, we calculated the counterfactual mortality rate that would obtain if half of the individuals in each education group had been in the next higher group. As each group generally differs from its successor by one or two years of education, this would imply an increase in the mean U.S. education level of somewhat more than half a year and a five percentage point increase in the fraction of adults who have graduated from college. The implied reduction in fatalities, 15%, would amount to saving roughly 4,000 lives.

The same pattern appears at the state level. An earlier working paper, Cox and Grant

(2016), related state-level per-mile fatality rates to the fraction of college graduates in that state in 1980, 1997, and 2014. Both within and across years, this relation was strongly negative and comparable in magnitude to that in Table 1: an increase in education levels that raised the fraction of college graduates by five percentage points lowered fatalities by 18%. This was supplemented by simple descriptive regressions that found a negative effect of high school graduation, but not college graduation, on fatality rates.

In summary, the education gradient in traffic mortality is widespread and large. Conditional on age, it exceeds that for other causes of death; furthermore, most traffic fatalities occur at younger ages, where the gradient is larger for all causes of death. However, this finding does not admit of a causal interpretation. Many personal characteristics that influence the amount of schooling obtained, such as cognitive ability, time preference, and self-control, also influence health directly. Descriptive statistics cannot disentangle the effects of all of these factors; a formal analysis is required. Nonetheless, given the magnitude of this gradient, it would be meaningful if even a small portion turns out to be causal, especially given the modest effects of many traffic safety laws, which often are 5% or less (see below).

4. **Causal Analysis**

How might education impact driving safety? Both cognitive and economic mechanisms are possible.

Education can improve individuals' skill and appreciation of the hazards of driving. A large literature tells us that risk perceptions are influenced by many factors, both subjective and objective (in traffic safety, see Machado-Leon et al., 2016, and Jing, Shan, and Zhang, 2022). Education could partly offset any tendency to underplay the risks of driving, increasing the individual's willingness to take safety precautions or drive more cautiously. At the same time, education could increase self-control, which leads to safer driving.

In addition, educated drivers have more to lose from traffic accidents. Because educated people earn more and live longer, the cost of an accident, in terms of expected future utility, is greater. This holds with even more force because educated people exhibit lower discount rates (Harrison, Lau, and Williams, 2002; Bauer and Chytilová, 2010). Altogether, education raises one's valuation of the future, which induces caution.

Finally, the additional income that education brings can purchase additional safety, in the form of improved vehicle maintenance and the purchase of cars with improved safety features. However, this income effect on safety is counteracted by a substitution effect working in the opposite direction. Educated people's time is more valuable, which may lead to increased speed or recklessness.

In summary, there are many mechanisms by which education could have a positive causal effect on traffic safety. This effect is not assured, however, because the relevant magnitudes are unknown and at least one mechanism works in the other direction.

Estimation

The link between schooling and health is filled with confounders. Cognitive ability, time preference, and self-control, which influence the amount of schooling obtained, also affect health directly. Thus, one must employ instruments that exogenously influence the amount of education received. In the existing literature, these instruments are commonly, though not exclusively, increases in the age at which youth are allowed to drop out of high school (the "dropout age"). Changes in accident involvement stemming from these increases can be considered causal.

These increases are numerous. Twenty-seven widely-dispersed states increased their dropout ages between 1981 and 2017, as shown in Table 2 (which lists our sources). And they are effective. A sizeable literature shows that these raised dropout ages have had material effects on low birthweight and pre-term birth (Noghanibehambari, Salari, and Tavassoli, 2022), juvenile crime (Anderson, 2014), education levels (Oreopoulos, 2009), and earnings (Oreopoulos, 2009). These findings, in turn, reinforce a large literature on the effects of earlier changes in dropout ages, which finds substantial effects on many social outcomes. All of these features recommend this variable for use here. Raised dropout ages provide a plausibly exogenous shock to education levels that can in turn affect traffic safety.

We can put them to use using data on fatal traffic accidents. This data comes from the Fatality Analysis Reporting System (FARS) of the National Highway Traffic Safety Administration, which reports detailed information on every fatal traffic accident in the U.S., including accident date and location and the age of each person involved. We use data for all fifty states, omitting the District of Columbia, as many of its accidents involve drivers from Maryland and Virginia.

We begin by assuming that the logarithm of the number of accident-involved drivers of age *a* in state *s* in year *t* is determined as follows:

$$\log(F_{a,s,t}) = \alpha_{a,s} + \sigma_{s,t} + \tau_{a,t} + \beta(P16_{s,t-a} + P17_{s,t-a}) + \gamma X_{a,s,t} + \varepsilon_{a,s,t}$$

where α, σ, τ are sets of dummy variables, γ is a vector of coefficients, *X* is a vector of observables, and ε is a residual. The variable P16 (P17) represents the probability a person aged *a* years in state *s* in year *t* was required to attend high school when they were 16 (17), and the coefficient β measures the percentage change in accident-involved drivers resulting from requiring another year of high school attendance. As Table 2 shows, almost all increases in the dropout age over our sample period affect sixteen year olds and/or seventeen year olds. It is not necessary to presume P16 and P17 have equivalent effects; we do so only because in practice the statistical precision needed to estimate separate effects is lacking.

Both the dummies and the probabilities in this equation need further elaboration. Because all two-way interactions of state, age, and year are included, this model is "nearly saturated." These dummies will pick up most plausible influences on traffic safety, because they operate uniformly by age within a state, uniformly across states over time, etc. Most laws, for example, apply to all ages within a state, while technological improvements in automobiles would be widely dispersed across the country. No confounders at this level need be controlled for directly, only those few that vary uniquely over time for that age in that state, found in the vector X.

The probabilities P16 and P17, in turn, are not precisely known, especially in the years surrounding an increase in the dropout age. To explicate the logic, let us temporarily ignore P17 and consider an increase in the dropout age from 16 to 17. Because accidents can happen at the beginning or the end of any year in our data, and the drivers involved could be near or far from their next birthday, a driver of age a in year T could have been 15, 16, or 17 years old at any given point in the year ($T - a + 16$). An increase in the dropout age at that time may or may not have affected this driver. Two years earlier, in year T-2, drivers of this same age would have been older than 16 at the time the dropout age was raised, and thus not subject to this requirement. On the other hand, two years later, in year T+2, drivers of age a would have been younger than 16 at the time the dropout age was raised, and thus surely (eventually) subject to this requirement. If we allow for migration into state *s* from states with other dropout ages, this latter group is "mostly treated" (P16 ≈ 1), while the preceding group is "mostly untreated" (P16 ≈ 0).

In these two years, *T*+2 and *T*-2, it would be reasonable to replace P16 with a dummy, M16, which equals one if sixteen year-olds are required to attend school in that state in that year and zero otherwise. More generally, if state *s* raised its dropout age above 16 at some point in year Y_s^{16}, $M16_{s,t-a+16}$ closely approximates P16 when $|t-a+16-Y_s^{16}| \geq 2$ but not otherwise, when P16 is an unknown fraction between zero and one. We term these latter observations "ambiguous."

Following this logic, the narrowest "clean" difference between treated and untreated drivers spans four years. Taking this difference between drivers of age *A* in state *S* yields:

$$\begin{aligned}
&\log(F_{A,S,t+2}) - \log(F_{A,S,t-2}) \\
&\qquad = \sigma_{S,t+2} - \sigma_{S,t-2} + \tau_{A,t+2} - \tau_{A,t-2} + \beta_{16}(M16_{S,t-A+18} - M16_{S,t-A+14}) \\
&\qquad + \gamma\left(X_{A,S,t+2} - X_{A,S,t-2}\right) + \xi_1 \\
&\qquad \equiv \Delta\sigma_{S,t} + \Delta\tau_{A,t} + \beta_{16}\Delta M16_{S,t-A+16} + \gamma\Delta X_{A,S,t} + \xi_1
\end{aligned}$$

where all ξ's are generic, mean zero error terms and the Δ operator represents a centered difference of four years. If all ambiguous observations are excluded for which $|t-A+16-Y_s^{16}| < 2$ is satisfied for any variable, then the difference Δ*M*16 will take the value of 1 *only* for a centered difference around the cohort immediately affected by the raised dropout age—that is, *t* – *A* + 16 = Y_s^{16}, so that $M16_{S,t-A+18}$ is one and $M16_{S,t-A+14}$ is zero. Otherwise Δ*M*16 will equal zero.

Consider now modestly older individuals in this state, of age *B* > *A*, who were sure to have reached the age of majority by year Y_s^{16}-2 and were thus unaffected by any change in the dropout age. The same difference for this group is as follows:

$$\log(F_{B,S,t+2}) - \log(F_{B,S,t-2}) \equiv \Delta\log(F_{B,S,t}) = \Delta\sigma_{S,t} + \Delta\tau_{B,t} + \gamma\Delta X_{B,S,t} + \xi_2$$

Differencing these two differences yields the following:

$$\Delta\log(F_{A,S,t}) - \Delta\log(F_{B,S,t}) = \Delta\tau_{A,t} - \Delta\tau_{B,t} + \beta_{16}\mathit{\Delta}M16_{S,t-A+16} + \gamma(\Delta X_{A,S,t} - \Delta X_{B,S,t}) + \xi_3$$

Three last changes complete our specification. To incorporate $P17$ as well, we add a second dummy, $M17$. To accommodate a small number of age*state*year cells with zero fatalities (always in small states such as Vermont or Wyoming), $\log(1 + F_{A,S,t})$ replaces $\log(F_{A,S,t})$. And, to accommodate numerous instances where the dropout age is raised directly from 16 to 18, we amend the timing by one year, so that $\Delta\log(1 + F_{A,S,t}) \equiv \log(1 + F_{A,S,t+3}) - \log(1 + F_{A,S,t-2})$, and so on.[2] This yields the following:

$$\Delta\log(1 + F_{A,S,t}) - \Delta\log(1 + F_{B,S,t})$$
$$= (\Delta\tau_{A,t} - \Delta\tau_{B,t}) + \beta(\mathit{\Delta}M16_{S,t-A+16} + \mathit{\Delta}M17_{S,t-A+17}) + \gamma(\Delta X_{A,S,t} - \Delta X_{B,S,t}) + \xi_4$$

where ambiguous observations are dropped, as before. This equation can be applied to all states, those that raise their dropout ages over the sample period and those that don't. The τ's are not individually estimated; rather the terms $(\Delta\tau_{A,t} - \Delta\tau_{B,t})$ are directly estimated via a set of dummies. The coefficient β slightly understates the true effect of increases in the dropout age because of interstate migration, which makes $\mathit{\Delta}M16$ and $\mathit{\Delta}M17$ slightly overstate the change in the relevant probabilities.

In this differenced, "nearly-saturated" model, X need only include confounders that change when the dropout age rises and focus on the same age range. One clear candidate is graduated licensing (GDL) laws, which pertain to sixteen and seventeen year-olds, just as

[2] No state staggers increases in the dropout age ($M16$, $M17$) in close succession, so this five year difference works across the board. It is also straightforward to adapt this specification to handle Arkansas' two-year increase in the dropout age from 15 to 17.

increased dropout ages do. Dee, Grabowski, and Morrisey (2005) find these laws reduce fatalities among the teenagers immediately affected by them, but not necessarily later in their driving career—the estimates are too imprecise. The only other clear candidate is zero tolerance (ZT) laws, which apply to all drivers under twenty-one. Both of these laws were widely instituted over our sample period. Nevertheless, neither is obviously relevant here. Table 2 shows that GDL changes never coincide with changes in the dropout age, even within a couple of years. Meanwhile, the evidence supporting ZT laws is mixed at best (see Grant, 2010, 2018). Nevertheless, we place dummies for both laws in the vector X, in part because of the potential for bias in these earlier studies, neither of which accounts for changes in the dropout age.

GDL laws, ZT laws, and raised dropout ages are all staggered across time and space and affect young drivers only. Triple-difference analyses utilize all three sources of variation in order to reduce the effect of confounders, and all of the studies we have just cited fall into this category. Our approach is identical as a matter of identification but different in terms of execution. These other studies use levels specifications with fatalities as the dependent variable; the differencing is implicit through the inclusion of sets of dummies. In contrast, two of our three differences are explicit; our dependent variable is formed by subtracting fatalities across age and time. This approach is much more econometrically demanding: for each increase in the dropout age, $\Delta M16$ or $\Delta M17$ take the value of one only once. In return, numerous issues with levels specifications, including clustering, the "Goodman-Bacon problem," and variable trends, are obviated or greatly reduced.[3] Recent work by Millimet and Bellemare (2023) emphasizes the enhanced potential for bias in levels panel estimates and advocate using differences instead.

[3] As estimation is conducted on the full set of clusters (states), it is unclear whether or how one should cluster (Abadie et al., 2023). Unclustered standard errors are presented in Table 3; these *fall* mildly when clustered at the state*year level, which increases statistical significance.

To calculate the left-hand side difference above, we must know the number of drivers involved in fatal traffic accidents, by age, state, and year. This is taken directly from FARS. GDL and ZT laws are taken from the Dee, Grabowski, and Morrisey (2005) and Grant (2010), supplemented as noted in Table 2. We conduct estimation on accidents from 1994 through 2019, stopping before the Covid-19 pandemic. We also permit education to have effects that extend well beyond adolescence, conducting estimation on various age ranges up to age 35. The observations are weighted by the inverse of the state-level variance of the dependent variable; the largest states count about seven times as much as the smallest ones do.[4] To ensure there are no analogous timing issues with the control group, we make it five years older than the treated group; that is, B = A + 5.

Results

Estimates of β for various age ranges are presented in Table 3. They are not overly precise, as previously foreshadowed, a consequence of our differenced, "nearly saturated" estimation approach, which sacrifices precision for the sake of bias prevention. Nonetheless, at the lower end of the age range that point estimates are so large that they achieve statistical significance. The same cannot be said of the controls, which are not separately presented: of 17 estimates across all regressions in this table, only once does a control achieve statistical significance, just as chance would predict.

The significant coefficient estimates involve various sets of younger drivers, aged from 16 up to age 30. These imply that a one year increase in the dropout age in a given state lowers

[4] Because of triple-differencing, the R^2 of the regression is very low, so these variances closely approximate the variances of the residuals.

accidents involving drivers of the specified age range by 5-6%. This is a large effect as the traffic safety literature goes, all the more striking because it is biased slightly downward, per the argument in the previous section.

The estimates trend toward zero as drivers age, as predicted, with increased maturity and geographic dispersion that adulterates the degree to which $\Delta M16$ and $\Delta M17$ represent changes in the probabilities P16 and P17. However, the declines occur at different rates. Within the lower half of the ages in the sample, 16-25, the coefficient estimates diminish only slightly with age—statistical significance is lost in some age ranges only because the standard errors grow. For example, the estimate for drivers aged 21-25 is only 10% smaller than that for drivers aged 16-25, but the standard error increases by half. The estimates fall much more quickly when drivers aged 26-35 are included, and the point estimates for drivers above 25 are quite small (and insignificant). On balance, this evidence supports a sizeable effect on younger drivers that fades away in the late 20s. The diminishing of the estimates through the early 20s is mild enough that it could be wholly due to geographic dispersion, but the subsequent, far more rapid changes cannot be thus attributed. Maturation must also be a factor. (Not coincidentally, insurance premiums plummet at age 25 as well.)

The precision needed to estimate separate coefficients on $\Delta M16$ and $\Delta M17$ is lacking. Still, applying various weights to these two variables allows us to determine which the data favors: larger effects for $\Delta M16$, for $\Delta M17$, or rough equality.[5] Theoretically, $\Delta M17$ should impact traffic safety more. Increasing the dropout age from 17 to 18 effects everyone who wanted to drop out of school at age 16 along with those students who wished to leave school at

[5] That is, the value of $\Delta M17$ was halved, doubled, tripled, etc., keeping $\Delta M16$ unchanged, and the fit of these models compared.

the age of 17, while increasing the chance that 18 year olds stick around for those last few months of high school in order to graduate. The results of this exercise conform to this prediction. While far from statistical significance, the model performs best when $\Delta M17$ is attended more than thrice the effect of $\Delta M16$.

As a final check on our estimates, we conduct an event study of sorts, which "pretends" the change in the dropout age came up to five years earlier, or later, than it actually did. That is, we returned to our specification, replaced ΔM16 (and ΔM17) with its five year lead, and conducted estimation, and then repeated the process for a four year lead, three year lead, etc., up to a five year lag. This approach is not unusual for an event study; what is unusual is that estimation is conducted in differences over a period in which raised dropout ages are "phased in," that is, $0 < P16_{t-1} < P16_t < 1$. Our estimates will reflect these gradual increases in P16.

This phasing-in is slow at first, a consequence of three common practices: grandfathering in students who had already reached the previous dropout age, raising the dropout age late in the calendar year (i.e., at the beginning of the school year), and having two year increases in the dropout age (whose impact is staggered over two years, not one). As a result, the leads and lags of ΔM16 in our event study initially pick up small, then larger, changes in P16. Using Table 3's coefficient estimate for ages 16-30, the dashed line in Figure 1 shows the predicted results when all three of these "delaying mechanisms" are in effect.[6] This profile is nothing like those in standard, levels event studies in which there is no phasing-in.

The actual event study estimates, also presented in Figure 1, are to be compared to this profile. They resemble it quite well, and there is little evidence of a differential trend throughout

[6] That is, for each year y in the event study, $P16_{y+3}$ - $P16_{y-2}$ is calculated, given these delaying mechanisms and a uniform distribution of births over course of the year. Multiplying this fraction by Table 3's coefficient estimate for ages 16-30 yields the prediction in Figure 1.

the full eleven year window. On balance, this event study affirms the legitimacy of our estimates.

Altogether, we have amassed a body of evidence indicating that increases in the dropout age lower traffic fatalities: significant coefficient estimates among younger drivers that diminish with age, as anticipated; effect timing that matches expectations, as shown in our event study; and some evidence supporting the logic that an increase in the dropout age from 17 to 18 will have a larger impact than an increase from 16 to 17. Ironically, the secondary or unanticipated effects of increasing the dropout age are likely to impact traffic safety as much or more than many policies that are expressly designed for this purpose.

Discussion

Requiring one additional year of high school reduces traffic fatalities among drivers under 25 by about 5%. This is a large effect, larger than that of most drunk driving laws and similar to the effect of seat belt laws, reduced speed limits, and a 10% increase in the price of gasoline (see Grabowski and Morrisey, 2004, and the estimates and citations in Grant, 2021). The dropout age is an important traffic safety policy for younger drivers.

This conclusion is all the more surprising given the modest fraction of students who are compelled to complete more schooling via an increase in the dropout age. Most students never drop out and never plan to. While on the high side, however, it is not implausible.

In 2014, 7% of young drivers (like 7% of the population) were high school dropouts; these accounted for 16% of all traffic fatalities.[7] Among this group—people who drop out of

[7] Population estimates by education level are available for ages 18-24, but include 18 and 19 year olds still finishing high school and do not record the ultimate level of schooling completed for

high school, or would have absent the raised dropout age—a fatality decline of one-third is necessary to match the effects estimated in Table 3. To determine whether this is plausible, we rely on evidence from Anderson (2014) and Oreopoulos (2009) on the effects of raised dropout ages on school attendance.

Anderson (2014) finds that a one-year increase in the dropout age reduces the number of high school dropouts by 2.2 percentage points. Oreopoulos (2009) obtains a similar number,[8] and further shows that effect of raised dropout ages goes well beyond high school completion. There are large increases in school enrollment among 16-18 year olds (his Fig. 3.3), increases in grade attainment even for high school dropouts (his Fig. 3.4), and sizeable increases in the fraction of 16 year olds ultimately obtaining some college (his Table 3.3).

The most obvious channel involves an improvement in driving skills, prudence, and/or safety orientation that results from further education. This is important but cannot account for the full effect. Anderson's (2014) estimates imply that one-third of all would-be dropouts go on to complete high school when the dropout age rises by one year; Oreopoulous (2009) shows that the share of "some college" attainers increases by a similar amount (that is, about two percentage points). Even if this increase in educational attainment reduced crash risk by half—consistent with the relative risks of high school dropouts and the "some college" group in Table 1—total fatalities among young drivers would fall by only one-sixth. This is half of what is needed.

However, this effect is supplemented by a "diversion effect" resulting from greater school enrollment. This could be quite large. Oreopoulos (2009) finds that higher dropout ages increase school attendance by one percentage point for sixteen year olds, four percentage points

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many people attending college. As a result, they cannot be used, and "young drivers" refers to 25-34 year olds by necessity.

[8] His Table 3.3, final estimate on the left panel. Oreopoulos appears to have miscoded a few dropout ages; the resulting errors-in-variables bias may account for Anderson's larger effects.

for seventeen year olds, and two percentage points for eighteen year olds (who are no longer directly affected by the law);[9] the educational attendance of nineteen and (perhaps) twenty year olds in college also increases by about two percentage points. On balance, about a third of would-be dropouts are affected. Students cannot drive while physically in school, and scholastic activities are likely to supplant jobs and leisure activities, both of which would involve more driving. (Bostwick and Severen, 2022, provide evidence in support of both channels.) A diversion effect that also reduced risk by half would be sufficient to close the gap.

In addition to these primary channels, there are four additional possibilities. One is an income effect associated with greater schooling; however, at young ages this would be quite small. Second, a fraction of eventual dropouts nonetheless complete more grades of high school. Based on Oreopoulos' (2009) estimates, this channel is also likely to be small. Third, raised dropout ages may cause some would-be GED-earners to get the regular diploma instead. Heckman (2008) has shown that the two groups have similar cognitive skills but the latter has superior non-cognitive skills, which probably pertain to driving. Finally, the distinction between *fatalities* and *fatal crash involvement* (as a driver) is likely to matter. The latter is the proper subject of study and forms the dependent variable in our estimations. It picks up the "signal" from drivers who cause fatal crashes but do not die in them, while avoiding the "noise" resulting from the "incidental" fatalities of passengers. The relative risks in Table 1 do neither of these things, and thus likely understate the raw difference in fatal accident involvement that we would like to observe. Had we the data to re-do Table 1 in terms of crash involvement, the relative risks would be more spread out (that is, further from one in both directions), suggesting a greater benefit from schooling and magnifying the plausible size of the "driving skills" channel in our

[9] The greater attendance effect for seventeen year olds supports the larger effects of $\Delta M17$, relative to $\Delta M16$, on crash involvement mentioned previously.

calculations above.

Overall, then, we lack the information needed to fully trace out the mechanisms by which increases in the dropout age affect traffic safety, and their magnitudes. Nonetheless, there appears to be a sizeable causal effect of education on drivers' skill and safety-orientation, which is complemented by a "diversion effect" whereby students in school have fewer opportunities to drive and get in accidents while doing so. On balance, the 5-6% reduction in fatal crash involvement estimated in our data is plausible, but may be on the high end of the reasonable range.

5. Conclusion

The educational gradient in traffic safety is substantial: a fourfold difference between bachelor's degree holders and high school dropouts, similar to that for all external causes of death and greater than that for all-cause mortality. Some, potentially much, of this gradient is causal (with the rest presumably due to selection). In a triple-difference analysis, a one year increase in the dropout age among reduces fatal accident involvement among drivers 25 and under by about 5%. This sizeable effect is comparable to that of many frequently-studied influences on traffic safety, including drunk driving legislation, seat belt laws, and speed limits.

While we lack the information to fully reveal how greater schooling improves traffic safety, on balance we find support for two mechanisms: an improvement in driving quality and a "diversion effect" whereby students in school have fewer opportunities to drive. Further elucidation of the relevant mechanisms awaits data suitable to the purpose.

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Table 1. Relative Fatality Rates, Standardized to Those of High School Graduates, 2014 U.S. Vital Statistics, Ages 25+.

(a) Motor Vehicle Accidents

Highest Level of Education Completed	*All*	*Male*	*Female*	*Age 25-34*	*Age 35-54*	*Age 55+*	*Non-Hispanic White*	*Black*	*Hispanic (of any race)*
≤ 8th Grade	1.03	1.00	1.08	0.84	0.93	1.21	1.28	0.85	1.24
H.S. Dropout	1.24	1.23	1.22	1.25	1.39	1.06	1.50	1.15	1.00
GED, H.S. diploma	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Some College	0.63	0.63	0.70	0.59	0.61	0.66	0.62	0.58	0.73
Associates	0.50	0.49	0.61	0.41	0.44	0.63	0.50	0.46	0.47
Bachelor’s	0.31	0.28	0.41	0.18	0.23	0.54	0.31	0.32	0.38
Master’s	0.27	0.24	0.37	0.13	0.16	0.47	0.28	0.21	0.19
Doctorate	0.25	0.21	0.30	0.13	0.15	0.40	0.25	0.25	0.37

(b) All External Causes

Highest Level of Education Completed	All	Male	Female	Age 25-34	Age 35-54	Age 55+	Non-Hispanic White	Black	Hispanic (of any race)
≤ 8th grade	0.95	0.91	1.01	0.68	0.68	1.14	1.60	0.95	1.06
H.S. Dropout	1.24	1.26	1.15	1.34	1.41	1.07	1.51	1.37	1.07
GED, H.S. Diploma	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Some College	0.61	0.63	0.61	0.60	0.60	0.64	0.60	0.54	0.68
Associates	0.48	0.48	0.53	0.38	0.42	0.61	0.47	0.42	0.49
Bachelor's	0.34	0.35	0.34	0.18	0.25	0.56	0.34	0.26	0.35
Master's	0.28	0.30	0.28	0.11	0.19	0.46	0.28	0.22	0.28
Doctorate	0.33	0.33	0.24	0.11	0.19	0.50	0.32	0.23	0.41

(c) All Causes

Highest Level of Education Completed	*All*	*Male*	*Female*	*Age 25-34*	*Age 35-54*	*Age 55+*	*Non-Hispanic White*	*Black*	*Hispanic (of any race)*
≤ 8th Grade	1.59	1.60	1.58	0.83	0.82	1.45	2.76	2.61	1.95
H.S. Dropout	1.08	1.13	1.03	1.29	1.36	1.12	1.37	1.29	0.82
GED, H.S. Diploma	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Some College	0.51	0.60	0.43	0.60	0.59	0.56	0.49	0.49	0.59
Associates	0.42	0.46	0.38	0.40	0.44	0.53	0.39	0.49	0.53
Bachelor's	0.35	0.43	0.27	0.20	0.27	0.48	0.32	0.34	0.47
Master's	0.33	0.44	0.24	0.14	0.20	0.39	0.30	0.38	0.45
Doctorate	0.36	0.49	0.18	0.12	0.18	0.38	0.33	0.39	0.79

Table 2. Raised Dropout Ages, 1981-2017, and Related Controls.

State	*Affected Ages*	*Year Changed Dropout Age*	*Zero Tolerance Law*	*Graduated Licensing*
Arkansas	15, 16	1982	1993	2002
	17	2014		(see note)
South Carolina	16	1982	1998	1998
Wisconsin	16, 17	1982	1997	2000
New Mexico	17	1983	1994	2000
Louisiana	16	1988	1997	1998
	17	2001		
Texas	16	1989	1997	2002
	17	1997		
Virginia	17	1991	1994	2001
California	16, 17	1993	1994	1998
Mississippi	14, 15	1994	1998	2000
	16	1997		
Kansas	16, 17	1997	1997	none
Connecticut	16, 17	2001	1995	2005
Nebraska	16, 17	2005	1994	1999
Illinois	16	2005	1995	1998
Indiana	16, 17	2005	1997	1999
Colorado	16	2007	1997	1999
Nevada	17	2007	1997	2001
Alabama	16	2009	1996	2002
Missouri	16	2009	1996	2001
New Hampshire	16, 17	2009	1993	1998
South Dakota	16, 17	2009	1998	1999
Michigan	16, 17	2010	1994	1997
Rhode Island	16, 17	2011	1998	1999
West Virginia	16	2011	1994	2001
Maryland	16	2014	1989	1999
	17	2016		
Minnesota	16	2014	1993	2008
Tennessee	17	2014	1993	2001
Kentucky	16, 17	2015	1996	2006

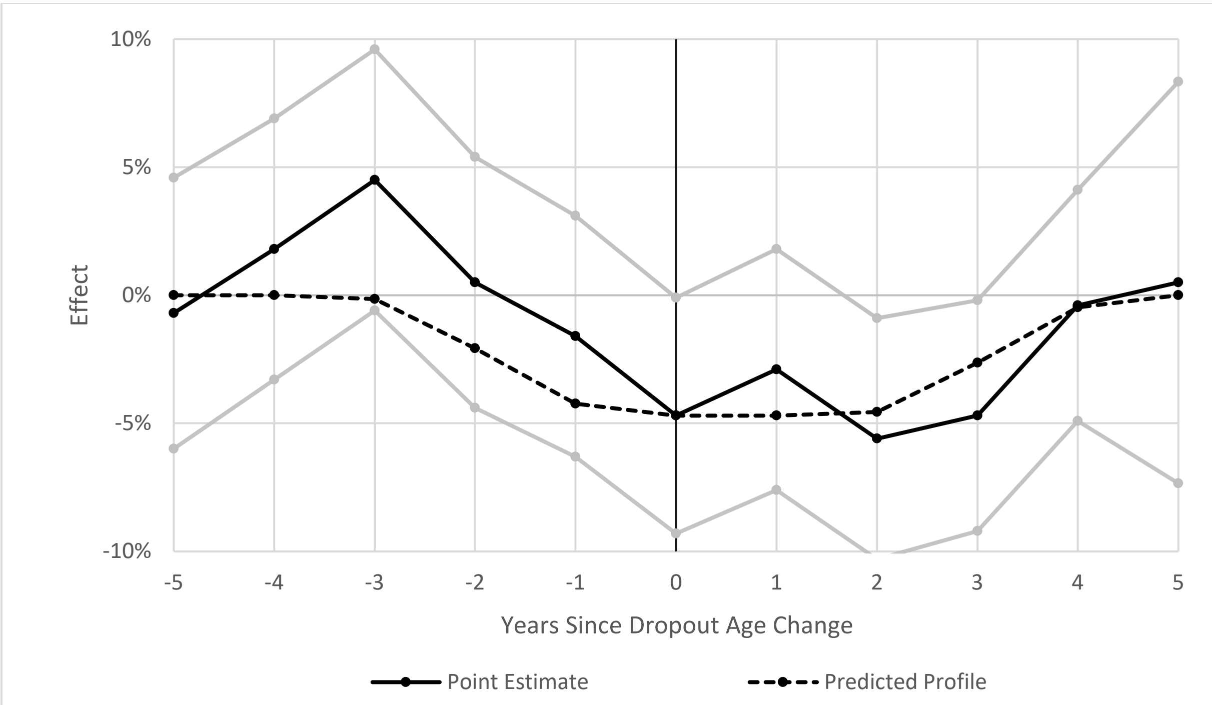
Note: Dropout ages through 2009 rely on Anderson (2014), updates through 2013 taken from Mackey and Duncan (2013), subsequent updates from the *Digest of Education Statistics*. Zero Tolerance laws taken from Grant (2010), and Graduated Licensing Laws from Dee, Grabowski, and Morrisey (2005) and the Insurance Institute for Highway Safety (IIHS, 2007). In contrast to Dee, Grabowski, and Morrisey, the IIHS records Arkansas as having no intermediate stage graduated licensing. Of the states listed here, the graduated licensing programs of California, Kentucky, Maryland, and Tennessee are rated by the IIHS as “good”; the others are “fair” or “marginal.”

Table 3. Estimation Results (estimated effect of raising the dropout age one year on the number of drivers involved in fatal accidents, in percent, with standard errors in parentheses).

Beginning of Age Range	*End of Age Range* 25	30	35
16	-0.062** (0.027)	-0.047** (0.024)	-0.034 (0.022)
18	-0.058* (0.032)	-0.041 (0.026)	-0.027 (0.024)
21	-0.055 (0.043)	-0.033 (0.031)	-0.016 (0.028)
25	----	-0.010 (0.045)	0.010 (0.037)

Note: Accidents from years 1994-2019 were studied. $R^2 \approx 0.05$ in all regressions. There were two controls: dummies for the presence of a zero tolerance law (when drivers are under 21) and the presence of an intermediate-level graduated licensing program at the time the driver was sixteen years old. Across all of the regressions in this table, only once did the coefficient estimate on either of these dummies achieve significance at the 5% level, in line with random chance. Estimates without controls were very similar. The ages in the table apply to the target group; the control group is five years older. * = $p < 0.10$, ** = $p < .05$ in two-sided tests.

Figure 1. Event Study (predicted and estimated effects with moving event window, ages 16-30, with 95% confidence intervals included).



Note: The predicted values assume a two-year increase in the dropout age from 16 to 18, implemented two-thirds of the way through the calendar year, grandfathering in students who have already turned 17. The Table 3, ages 16-30, coefficient estimate of -0.047 is treated as the true effect of a one-year increase in the dropout age.