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AGE AND SUICIDE IMPULSIVITY:
EVIDENCE FROM HANDGUN PURCHASE DELAY LAWS

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John Donohue has at various times served as an expert witness in litigation involving firearm regulation. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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ABSTRACT

We provide the first quasi-experimental estimates of variation in suicide impulsivity by age by examining the impact of firearm purchase delay laws by age. Prior studies of firearm purchase delay laws use traditional two-way-fixed-effects estimation, but we demonstrate that bias due to heterogenous treatment effects may have inflated previous estimates relative to our stacked-regression approach. We also develop a triple-difference stacked-regression estimator to confirm the robustness of our results. We find that purchase delay laws reduce firearm suicide for the overall adult population, but this effect is largely driven by a 6.1 percent reduction in firearm suicides for young adults ages 21-34. We demonstrate that the relationship between purchase delay laws and firearm suicide reduction weakens with age and is not driven by gun ownership rates. We argue that this is due to the impulsiveness of young adults in committing suicide, indicating that removing firearm access for young adults may provide a critical deterrent to suicide.

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1 Introduction

In 2021, about 12.3 million American adults had serious suicidal ideation, and 48,000 people died by suicide (CDC, 2023). Given the gravity and prevalence of deaths by suicide, preventing suicide is an increasingly important policy priority and research interest. 988, the Department of Health and Human Services's new Suicide and Crisis Lifeline launched in 2022, has received nearly a billion dollars in federal funding alone (HHS, 2023). Economists have identified important predictors of suicide, such as social cohesion (Becker and Woessman, 2018), income inequality (Daly, Wilson and Johnson, 2013), and unemployment (Breuer, 2014), highlighting the impact of economic and social policy on suicidality. Moreover, economic research has directly measured the beneficial effects of particular policies on suicide rates, such as cash transfers (Christian, Hensel and Roth, 2019), unilateral divorce (Stevenson and Wolfers, 2006), and required mental health benefits as part of health insurance coverage (Lang, 2013). One significant challenge in translating these research findings to policy, however, lies in the fact that much of the economic research studies the effects of socio-economic phenomena and policies on a large and diverse group of adults. As such, the overall findings in these studies may mask substantial heterogeneous effects within different subpopulations. In particular, descriptive evidence indicates that patterns and circumstances of suicide vary substantially across age, suggesting that the effect sizes of various suicide prevention policies may also vary by age (McLone et al., 2016).

One key difference in risk for suicide between younger and older adults is the difference in impulsivity[1] between these two groups. Psychologists and public health researchers have posited several linkages between impulsive tendencies and suicidal behavior, with some attributing suicide

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[1]While there exist many measures of impulsivity (Mccullumsmith et al., 2014), we define an impulsive suicide as one that could be prevented by a delay in access to a chosen suicide mechanism, namely firearms. This definition of impulsive suicide follows naturally from our study context, and it is also a reasonable definition of impulsive suicide for policymakers and public health officials. Additionally, given that elevated states of suicidal thinking typically only last on average a few hours, the interventions we study that delay firearm purchase by several days will encapsulate most short-term episodes of elevated suicidal thinking (Coppersmith et al., 2023).

outcomes directly to elevated impulsivity compared to nonsuicidal mental patients and healthy controls (Anestis et al., 2014; Conner et al., 2004; Dumais et al., 2005). Additionally, McGirr et al. (2008) finds that the impulsivity-suicidality relationship weakens with age. However, there is an absence of quasi-experimental evidence quantifying this relationship. Motivated by this gap in estimating the relationship between age and suicide impulsivity, we use handgun purchase delay laws as an avenue to investigate how disruptions in impulsive firearm[2] suicide plans may have heterogeneous impacts by age. Leveraging differential timing in the adoption and repeal of purchase delay laws, our difference-in-differences estimates suggest that impulsivity directly increases suicide risk through the sudden ideation of suicide plans that could be disrupted by a "cooling off" period and that suicidal impulsivity in adults wanes with age.

Beginning with Cook and Ludwig (2000), which studied the effect of the waiting period provision of the 1994 Brady Handgun Violence Prevention Act, economists have long identified the beneficial effects of adopting firearm purchase delay laws. The Brady Act instituted a five-day waiting period on handgun purchases from federally licensed firearm dealers between February 1994 and November 1998. While Cook and Ludwig (2000) find an imprecisely estimated negative effect of the Brady Act on firearm suicides, more recent studies leveraging longer panels and more treatment variation (beyond Brady) find evidence of statistically significant declines in firearm suicide as a result of handgun purchase delay laws (Edwards et al., 2017; Luca, Malhotra and Poliquin, 2017). We confirm the direction of these prior findings on firearm suicide across all adults, and demonstrate that the "cooling-off" effect of state-mandated delays in handgun purchase leads to a larger decline in firearm suicides for young adults than for older adults.

Beyond providing the first evidence on the differential impacts of purchase delay laws' ability to disrupt suicidal plans by age group, our paper brings superior data to bear on this issue while making a key methodological contribution to the literature. First, previous research on the effect of purchase delay laws on suicide, as well as the broader economic literature on firearms and suicide, has primarily relied on state-year panel data (Depew and Swensen, 2022; Lang, 2012). Instead, we obtained restricted access CDC mortality files that enabled us to use county-year as the unit of observation in our panel data, thereby generating more precisely estimated effect sizes than models using comparable state-year panels.[3]

Second, we develop and implement estimators that are robust to heterogeneous treatment effects. In recent years, researchers have developed new approaches to difference-in-differences estimators (Roth et al., 2023) that are not biased by differential treatment effects across groups in staggered treatment adoption settings. We use a stacked regression approach popularized by Cengiz et al. (2019), which, like the local projection difference-in-differences approach (Dube et al., 2023), is flexible to non-absorbing treatments and specifications with interaction terms as a variable treatment.[4] Additionally, we introduce a novel stacked triple-differences estimator to confirm the robustness of our main findings.

The remainder of this paper proceeds as follows. Section 2 describes the data used to complete

[2]Our paper specifically looks at firearm suicide, and we do not suggest that our results on suicide impulsivity are generalizable to non-firearm suicides, although they may be.

[3]We show results from comparable state-year panels in the Appendix.

[4]We do not use interaction terms in our main specification but some of our robustness checks do include them.

our empirical analysis. Section 3 presents our methodology. Section 4 presents and discusses our empirical estimates. Section 5 concludes.

2 Data

Our county-level data spans all states from 1987-2019, with our sample limited to counties included in the American Community Survey (ACS) in 2019. Our outcome of interest is firearm suicide among adults subdivided into three different age groups, which we measure by aggregating individual-level CDC mortality data to the county-year level. We also create a cause-of-death category for non-firearm suicide. We use the RAND Corporation's state firearm law database (Cherney et al., 2022) to identify changes in handgun waiting period laws (state-mandated delay in receiving a handgun after the initial intent to purchase), handgun permit-to-purchase laws, and background check laws for firearms purchased from federally licensed dealers. Consistent with the prior literature on handgun purchase delay laws, we consider a state to have a purchase delay regime if it has either an active waiting period law or a permit-to-purchase law.[5] In practice, permit-to-purchase laws always lead to nonzero administrative turnaround time to purchase a handgun. Between the late 1920s and the early 1990s, 21 states introduced handgun waiting period laws, with a minimum of 2 days and a maximum of 15 days, and often accompanied by a background check. In 1994, under the federal Brady Handgun Violence Prevention Act, the remaining 29 states adopted a 5-day waiting period and background check. The Brady waiting period requirement was sunsetted after five years and replaced by the National Instant Criminal Background Check System (NICS) in November 1998. Sixteen states stopped enforcing handgun waiting periods at seven points spanning 1996 through 2015.

Our specifications also include socioeconomic and demographic controls associated with suicide. We collected information on state-year level ethanol consumption from The National Institute on Alcohol Abuse and Alcoholism (Kaplan, 2021*a*). For our primary regressions, we obtain county-year level covariates (population density, household income, percent in poverty, percent Black, percent 21-34, and percent 35-54) from US Census data via Social Explorer (Census, 2023). Data was linearly interpolated in non-Census years. Our dataset contains approximately the largest quarter of US counties by population and 85 percent of the total US population in 2019.

In supplemental results, we study the impact of household gun ownership on the effect of handgun purchase delay laws, constructing a state-age group estimate of household gun ownership using data from the University of Chicago's General Social Survey (Smith and Son, 2015) and estimates from the RAND Corporation (Schell et al., 2020) from 1987-2018. For this state-level analysis, we obtain state-age-year-level (household income, percent in poverty, percent Black, percent living in a metropolitan statistical area) covariates from the US Census via IPUMS (Ruggles et al., 2023). More details on our household gun ownership proxy and other data sources can be found in the Data Appendix.

[5] We round the date of adoption or repeal of these laws to the nearest year.

3 Methods

Our approach leverages differential timing in the adoption and repeal of handgun purchase delay laws across US states using a difference-in-differences design. As described by Goodman-Bacon (2021), estimates recovered from staggered-adoption difference-in-difference analyses are a weighted average of individual 2x2 difference-in-difference comparisons. Some of these comparisons, such as using an earlier-treated group as a control for a later-treated group, yield biased estimates if there are heterogeneous treatment effects across treated groups. To overcome this issue in our analysis, our main results use a stacked-regression approach that is robust to heterogeneous treatment effects (Baker, 2022).

For the estimation of our main results, we first construct a dataset specific to each treatment event, h, defined as the adoption or repeal of a purchase delay law in a particular year. Each event h-specific dataset includes all counties whose treatment status was affected by event h and all clean control countries across a 10-year panel by event time, from $t = (-5, \ldots, 4)$. Our preferred approach for control groups is to use only never adopters or always adopters, depending on whether the treatment event is the adoption or the repeal of a purchase delay law. We select the control group that matches the treatment status of the county prior to event h.[6]

We stack all event h-specific datasets together to calculate an average effect of purchase delay laws across all events (Cengiz et al., 2019). We employ a Poisson regression model, since a count model is most appropriate for our empirical context. We prefer a Poisson fixed effects model over a negative binomial model because the negative binomial fixed effect approach does not properly account for time-constant variables (Wooldridge, 1999). Our main specification takes the following form:

$$Y_{it} = \alpha + \beta PurchaseDelay_{it} + \sum_{j \in M} \gamma_h^{'} \chi_{it} I(h = j) + \delta_{ih} + \lambda_{th} + \epsilon_{ith}$$

where Y_{it} represents the number of firearm suicides in county i in year t, and X_{it} represents a set of covariates, and M represents the set of all stacks h.[7] The coefficient represents the average estimated treatment effect of adopting a purchase delay law on firearm suicides with stack-specific county and year fixed effects. Standard errors are clustered at the state-stack level, since all purchase delay laws in our sample are changed at the state level, and thus the state is the level at which "random assignment" occurs. In all specifications, we use population as an exposure variable.[8]

[6]In other words, the control group for adopter treatment-stacks would be the never-treated group. The control group for the repealer stacks would be the always-adopter group.

[7]For our regressions analyzing the impact of purchase delay laws on firearm suicides across all adults aged 21 and over, the covariates are ethanol consumption (measured at the state-year level), the presence of a required background check for firearm purchase from a federally licensed dealer (which applies nationally after 1994, but was in place earlier for 21 states), population density, median household income, percent of people living below the poverty line, percent of people who are Black, the percentages of people within the age groups 21-34, 35-54, and 55 and over, and population as an exposure variable. For our analyses of firearm suicides for a subset of adults, we use all the same covariates, except we do not include the percentages of people within various age groups as controls.

[8]When using count data as an outcome variable, the incidence of the event of interest in an observed group is affected by the size of the group and length of observation; in our context, the larger of two counties with the same suicide rate will see more individual suicides. We choose to include population as an exposure variable simply to constrain its coefficient to less than 1, reflecting our expectation that a one-person increase in population will not lead to a one-incident increase in suicides. In practice, including population as a regular explanatory variable minimally

We estimate the average effect of purchase delay laws on firearm suicides across different age groups using the static model presented above and then provide event-study analyses that allow us to assess the conditional parallel trends assumption. Our event study regresses firearm suicides on a set of yearly dummies for each of the 5 years prior and 4 years after a change in purchase delay laws, omitting the dummy for one year prior to adoption. The following equation shows the regression model underlying the event study analyses, where ψ_h is equal to the year of the relevant event for stack h and θ_h is equal to 1 if the stack h pertains to an adoption event and -1 if the stack h pertains to a repeal event:

$$Y_{it} = \alpha + \sum_{k \in (-5,-4,\ldots,3,4)/(-1)} \beta_k I[t = \psi_h + k]\theta_h + \gamma' X_{it} \sum_{j \in M} I(h = j) + \delta_{ih} + \lambda_{th} + \epsilon_{ith}$$

The stacked-regression approach relies on a dataset constructed of many treatment event-specific datasets that include only the treated groups and "clean" control groups. Among many new estimators that are robust to heterogeneous treatment effects, the stacked-regression estimator has many attractive properties that make it suitable for our empirical setting. First, the stacked-regression estimator is flexible to the non-absorbing treatment setting and allows us to select the control groups that would be expected to most closely predict the counterfactual path of the treatment group. The estimator is also easily adaptable to include interaction terms to study the treatment's effectiveness conditional on a second variable. Additionally, the stacked-regression estimator, like many new robust estimators, relies on weaker assumptions in the inclusion of covariates than traditional two-way fixed effects estimators. In particular, the stacked-regression restricts covariate estimation to only the time period of each stack, rather than assuming a uniform estimate of impact of a selected covariate across all group-time observations in the data. Lastly, while the other new estimators that correct for bias in TWFE are restricted to OLS models, the stacked estimator allows for Poisson regression as well.

4 Results

4.1 Main Specification

We begin by estimating the effect of purchase delay laws on the firearm suicide rate of the entire adult population as well as within three age groups: the young (21-34), middle-aged (35-54), and old (55+) age groups. Table 1 presents these results from both our preferred stacked estimation approach as well as results using a traditional (non-stacked) TWFE estimation approach.[9] The estimates presented in Table 1 and throughout the paper (unless otherwise noted) use incident-rate-ratios (IRRs) for ease of interpretation.

changes our results.

[9]The non-stacked TWFE estimation follows a specification similar to stacked estimates. The general regression equation for the non-stacked estimation is: $Y_{it} = \alpha + \beta PurchaseDelay_{it} + \chi'_{it} + \delta_i + \lambda_t + \epsilon_{it}$.

Table 1: Purchase Delay Laws Effect on Firearm Suicide by Age Group, 1987-2019

	(1)	(2)	(3)	(4)
Stacked Results				
Handgun Purchase Delay	0.967*	0.939**	0.973	0.976
	(0.013)	(0.019)	(0.019)	(0.018)
Age	All Adults 21+	Young	Middle Aged	Old
N	25580	25580	25580	25560
	(1)	(2)	(3)	(4)
Non-Stacked TWFE Results				
Handgun Purchase Delay	0.930***	0.905***	0.897***	0.943**
	(0.014)	(0.020)	(0.020)	(0.017)
Age	All Adults 21+	Young	Middle Aged	Old
N	24849	24849	24849	24849

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Note: County-level Poisson panel data estimates with state and year fixed effects, 1987-2019. Cluster-robust standard errors with clustering at the state level shown in parentheses. All models include covariates as described in Section II. All regressions use population as an exposure variable.

Because our research design exploits the staggered adoption and repeal of purchase delay laws, a traditional non-stacked TWFE estimator is vulnerable to bias for the reasons stated in Section III. Although the bias can theoretically favor any direction, Table 1 reveals that using a non-stacked estimator substantially overstates the beneficial impact of purchase delay laws on reducing firearm suicides. While our non-stacked results are not directly comparable to those of previous papers studying the impact of purchase delay laws because of our county-level data, Poisson regression specification, and slightly different time period and covariates, our results provide suggestive evidence that prior research on purchase delay laws may have yielded biased estimates due to invalid comparisons embedded within the TWFE estimator.

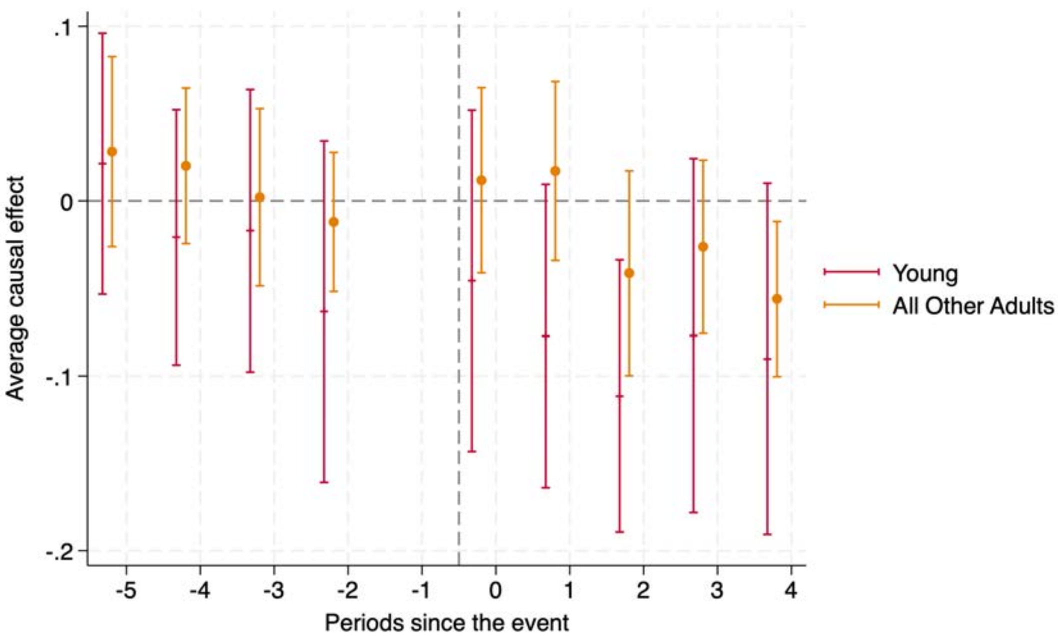
Given the flaws in the non-stacked TWFE estimator, for the remainder of the paper, we only discuss results from our preferred stacked estimator. For the overall adult population, we find that the presence of a purchase delay decreases the incidence of firearm suicide by a modest yet statistically significant 3.3% (IRR=.967). This overall estimate, however, masks heterogeneity of effect size by age, as shown by columns (2) through (4). The overall effect on adults is driven largely by the young age group, whose estimate is almost two times as large as the overall population: adults aged 21-34 experience a 6.1% (IRR=.939) drop in firearm suicide.[10] For middle-aged and

[10] A two sample t-test confirms that the drop in firearm suicides due to handgun purchase delay laws is statistically

older adults, we estimate weaker, non-statistically significant effects. In the Appendix, we show that purchase delay laws have a null effect on non-firearm suicide, providing evidence that there are minimal spillover effects of handgun purchase delays into non-firearm suicides. That is, individuals are not resorting to other means to commit suicide, and the reduction in suicides by purchase delay laws is an absolute decrease in total suicides.

In Figure 1, we also present event plots corresponding to the analyses in Table 1.[11] We find no evidence of non-parallel pre-trends across age groups.[12]

Figure 1: Purchase Delay Laws Effect on Firearm Suicide by Age Group, 1987-2019, Poisson Stacked Event Plot



Note: 95 percent confidence intervals with cluster-robust standard errors displayed.

4.2 Triple-Difference Estimation

To confirm that our results are not driven by trends in risky behaviors like suicide relative to adoption or repeal of handgun purchase delay laws, we introduce a novel triple-difference stacked regression approach. Our triple-difference analysis compares the effect of handgun purchase delay laws on trends in firearm suicides and arrests made for driving under the influence (DUI). We argue that DUI arrests reflect a risky behavior that may proxy for the underlying level of the self-endangering "acquired capability" for suicide that is described in Joiner (2007). We prefer to use

larger for young adults than it is for middle aged or older adults at the p<.01 level.

[11] To clarify the figure visually, we run event study analyses for young adults and all other adults rather than having four different event study analyses corresponding to the four columns of Table 1. The event studies directly corresponding to each column of Table 1 are qualitatively similar and are available upon request.

[12] The p-value on the F-test of pre-passage dummies is p=0.60 and p=0.62 for the young adults and all other adults respectively.

DUI arrests because they do not vary significantly over time, unlike many other lower-level arrests.[13] Our triple-difference specification assumes that the rate of firearm suicides would have evolved in parallel with the rate of DUI arrests in a county, were it not for the adoption or repeal of a purchase delay. This design flexibly absorbs state-year shocks unrelated to purchasing law changes such as economic conditions or national changes in suicidality.

We aggregate age-specific arrest data from the FBI's Uniform Crime Reporting (UCR) Program (Kaplan, 2021*b*) from the agency-level up to the county-level, following a conservative imputation procedure for missing months and dropping agencies that do not report all sample years, as outlined in the Appendix. We estimate the triple-difference using the following equation:

$$Y_{itd} = \alpha + \beta \cdot PurchaseDelay_{it} \cdot I\{d = FS\} + \delta_{ihd} + \lambda_{ith} + \sigma_{dth} + \epsilon_{ithd}$$

where d represents whether the outcome being observed is firearm suicides (FS) or DUIs.

Results from our stacked-regression approach are displayed in Table 2. The event-study plot is shown in the Appendix. We find that our triple-difference results are consistent with and buttress the findings from our main stacked specification in Table 1. Young adults aged 21-34 see a large, significant drop in firearm suicides of roughly 10% while the depressing effect of purchase delay laws on suicides for older adults is far more muted and not statistically significant.

Table 2: Purchase Delay Laws Effect on Firearm Suicide, 1987-2019, Triple Difference Stacked Estimates

	(1)	(2)	(3)	(4)
Handgun Purchase Delay	0.953	0.897***	0.960	0.972
	(0.034)	(0.038)	(0.038)	(0.043)
Age	All Adults 21+	Young	Middle Aged	Old
N	31520	31320	31480	31200

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

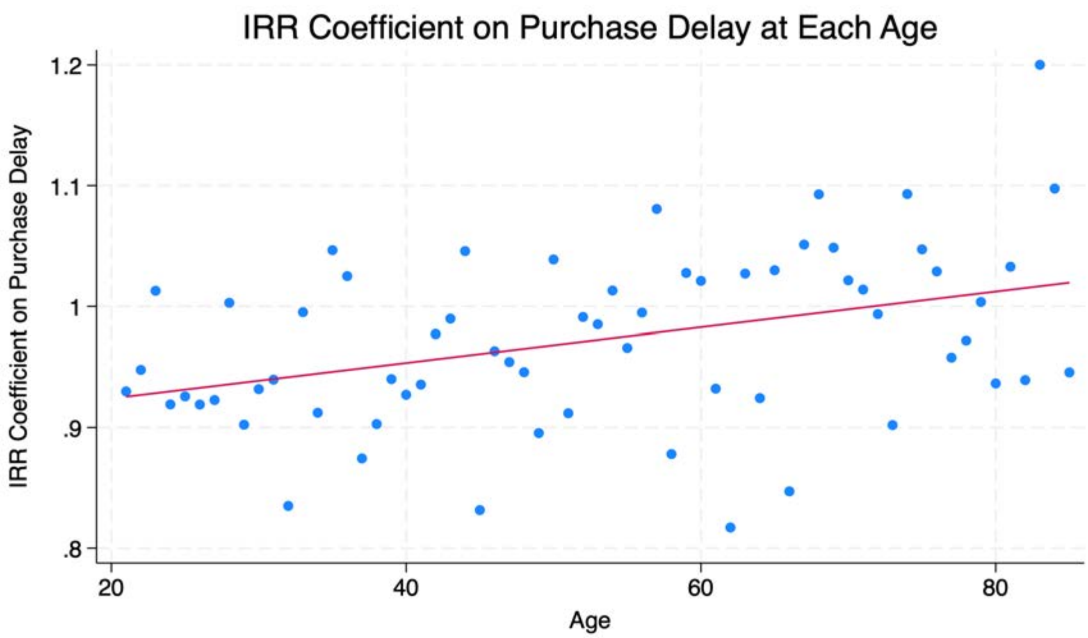
Note: County-level Poisson panel data estimates with state and year fixed effects, 1987-2019. Cluster-robust standard errors with clustering at the state-level shown in parentheses. Due to lack of DUI observations for treated groups, we do not include the 2001 or 2009 treatment events in our triple difference analysis.

[13]Other potential control groups could have been non-firearm suicides, drug overdoses, or deaths due to alcoholic liver disease. Case and Deaton (2020) have considered suicides, drug overdoses, and deaths due to alcoholic liver disease collectively to be "deaths of despair',' making these other categories of deaths seem like viable control groups. However, using non-firearm suicides in our triple-difference specification would be problematic due to potential spillover effects that would bias results away from zero (although we find no evidence of substantial spillover). Additionally, deaths by alcoholic liver disease are not a practical outcome given our interest in age-specific suicide risk because young people very rarely die of cirrhosis or other alcoholic liver diseases, making this an inappropriate comparison given our interest in age-specific suicide risk. Finally, drug overdoses display significant trends over our study period due to the crack and opioid epidemics. Thus, we opt not to rely on any of the categories traditionally classified as deaths of despair.

4.3 Single Age Estimation

The age groups presented in our Table 1 and Table 2 analyses were based on data availability in our *county*-level population data. However, we now show that our results showing the increased effectiveness of purchase delay laws on reducing firearm suicide in young adults are not simply an artifact of these age cutoffs. To do this, we run our preferred specification for each individual age at the *state*-level.[14] In Figure 2, we plot coefficients of purchase delay laws for every age between 21-85 using a state-year panel. The resulting regression line shows that purchase delay laws dampen suicides for the youngest adults by about 9 percent but the effect falls to zero by around age 75.[15] The figure provides compelling evidence of the relationship between impulsive suicide and age. The suicide-dampening effect of purchase delay laws subsides as age increases, demonstrating that however we set age group cutoffs, the effect is most prominent for young people.

Figure 2: Purchase Delay Laws Effect on Firearm Suicide by Single Age at State Level, 1987-2019



Note: State-level panel data Poisson estimates with state and year fixed effects, 1987-2019. Cluster-robust standard errors with clustering at the state level shown in parentheses. All models include covariates as described in Section II, with coefficients reported per single age. All regressions use population as an exposure variable.

[14] We use the same model as we used for our county-level results shown in Table 1. While we still control for socioeconomic and demographic conditions, we use slightly different covariates due to data availability. In the state level regressions, we control for: the presence of a required background check for firearm purchase from a federally licensed dealer, ethanol consumption, percent of individuals living in metropolitan statistical areas, household income per capita, percent of individuals who are Black, and population as an exposure variable. We perform this analysis at the state-level to increase the average population of our geographic unit of analysis since studying single ages will dramatically reduce the population represented by each observation.

[15] The slope of the fitted line is .001 with a p-value of .001.

4.4 Gun Ownership and Effect Size

For purchase delay laws to work, we assume that individuals do not already have a firearm accessible to them and must purchase a new firearm. One possible omitted variable that we are not able to control for in the county-level results is a measure of gun prevalence. It could be that young people are mechanically most impacted by purchase delays because they have a lower level of pre-existing household gun ownership. However, we now show that our results hold at the state-level when we for gun ownership.[16] We consider how the effectiveness of purchase delay laws vary by *both age and household gun ownership*. We use a dataset at the state-age group-year observation level[17] and run the following interaction models shown in columns 1 and 2 of Table 3, respectively:

$$Y_{stk} = \alpha + \beta_1 x_1 + \beta_2 x_2 + \beta_3 x_1 x_2 + \beta_4 x_1 o + \beta_5 x_1 m + \sum_{j \in M} \gamma_h^{'} \chi_{it} I(h = j) + \delta_{skh} + \lambda_{tkh} + \epsilon_{stkh}$$

$$Y_{stk} = \alpha + \beta_1 x_1 + \beta_2 x_2 + \beta_3 x_1 x_2 + +\beta_4 x_1 p + \sum_{j \in M} \gamma_h^{'} \chi_{st} I(h = j) + \delta_{skh} + \lambda_{tkh} + \epsilon_{stkh}$$

where Y_{stk} represents the logged firearm suicide rate in state s in year t in age-group k, x_1 is equal to $PurchaseDelay_{st}$ as shown in previous equations; x_2 is equal to household gun ownership at time t for age group k in state s; o represents a dummy variable equal to 1 for the old age group and 0 for all other groups; m represents a dummy variable equal to 1 for the middle age group and 0 for all other groups; and p is a dummy variable equal to 0 for the young adult age group, 1 for the middle-aged group, and 2 for the old age group. The second model assumes an equal gap between the young, middle-aged, and older adults, as is implied by Figure 2, whereas model 1 uses a more flexible approach. Because the unit of analysis is at the state-age bucket level rather than the county-age bucket level, we opt to use a OLS regression instead of a Poisson estimation. We weight our regression by population.

[16] We note that throughout the 1980s and 1990s, which includes the time period of the Brady Act waiting period that contributes a substantial part of the treatment variation in our data, household gun ownership rates were not meaningfully different for young adults and elderly adults.

[17] Reliable and nationally representative measures of gun ownership are only available at the state level, requiring us to move to the state level rather than the county level. For reference, when weighted by state population, the mean gun ownership in our sample is 37% with a standard deviation of 3.5%.

Table 3: Purchase Delay Laws Effect on Firearm Suicide by Age Group and Gun Ownership at State Level, 1987-2019, OLS Stacked Estimates

	(1)	(2)
Handgun Purchase Delay	-0.158***	-0.161***
	(0.043)	(0.043)
Gun Ownership	-0.002*	-0.002*
	(0.001)	(0.001)
Middle Effect (Relative to Young)	0.019	
	(0.037)	
Old Effect (Relative to Young)	0.054	
	(0.038)	
Gun Ownership x Handgun Purchase Delay	0.002**	0.002**
	(0.001)	(0.001)
Effect As Age Bucket Increases		0.028
		(0.019)
Observations	4050	4050

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Note: State-age group level panel data estimates, 1987-2019. Cluster-robust standard errors with clustering at the state level shown in parentheses. Gun ownership is not controlled for separately for each individual stack, but rather a single variable across all stacks, for purposes of presentation. When we control for gun ownership separately by stack, we find similar results on each coefficient. Note that the estimates shown here are not IRRs.

These results suggest that even controlling for gun ownership, purchase delay laws have a strong negative effect on suicides. The results also support the conclusion that the effect weakens as age increases, although using our state-level panel makes it more difficult to precisely measure these effects. Overall, the findings provide evidence consistent with the public health and medical literatures suggesting suicide is an impulsive act for young people and access to a firearm eases barriers for them to commit suicide.

4.5 Further Robustness

In addition to providing evidence supporting the conclusion that our main results are robust to underlying trends in firearm suicides (relative to handgun purchase delay law adoption or repeal), varied age cutoffs, and variation in household gun ownership rates, we perform a battery of further robustness checks to confirm the validity of our findings in the Appendix. We use alternate covariates and clustering variables and find qualitatively similar results. We also show that the effects we identify are not driven by a few large counties that may be outliers. Further, we show that other demographic variables such as race and gender are relatively stable across young, middle aged, and older adult suicides, indicating that the heterogenous effects we identify by age are likely not due to confounding by another demographic variable. Moreover, our paper, like all prior studies of handgun purchase delay laws, combines the study of both the adoption and repeal of purchase delay laws. In the Appendix, we provide evidence that these effects are roughly reciprocal and can be studied simultaneously. Lastly, we show that our results hold at the state-level.

5 Conclusion

Our paper makes three significant contributions to the economics literature on the effect of handgun purchase delay laws on firearm suicide and impulsivity in firearm suicide. First, it adds to the mounting literature documenting bias in estimates from two-way-fixed-effects models and uses a stacked regression approach to address this issue. Our analysis found that this bias was fairly substantial. Specifically, we find that two-way fixed effects models estimate larger effect sizes than those using a stacked regression approach, which suggests that prior literature on the topic may have inflated the magnitude of the benefits of handgun purchase delay laws as a policy intervention in reducing firearm suicide. Second, we use triple-difference stacked estimation to demonstrate the robustness of our main findings, the first paper that we know of to use this technique. Our stacked triple-difference estimator can be flexibly deployed across many empirical settings and overcomes a separate threat to identification that cannot be addressed simply by using a stacked regression difference-in-differences approach. Third, by breaking down the traditional analysis of purchase delay laws and suicide into age groups within our stacked approach, we were able to identify substantial heterogeneity by age group – establishing that the primary benefit of purchase delay laws is to significantly reduce suicides by young adults. In doing so, we are the first study to quasi-experimentally identify and estimate the relationship between age and suicide impulsivity, confirming many hypotheses of the relationship with estimates.

Our paper adds support to the idea, so far understudied by economists, that young people are impulsive in their decision to commit suicide. Our empirical approach also indicates that more research on suicide should test for heterogenous treatment effects by age group. Doing so may provide additional clarity and robustness to researchers as well as important information to policymakers. There are also rich opportunities for further research. For example, to confirm the suicide-impulsivity hypothesis beyond firearm suicide, researchers may look at policies associated with non-firearm suicide by age. Ultimately, further research into understanding how different populations are affected by suicide prevention policies will help policymakers carefully tailor their approach to suicide prevention and more effectively fight this public health crisis.

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