Age and Suicide Impulsivity: Evidence from Handgun Purchase Delay Laws

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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 on three different age groups. Prior studies of firearm purchase delay laws use traditional two-way-fixed-effects estimation, but we demonstrate that bias due to dynamic effects in non-treatment variables may have inflated previous estimates relative to our stacked-regression approach. We find that purchase delay laws reduce firearm suicide for the overall adult population, but this effect is largely driven by a 6.6 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 immediate firearm access for young adults may provide a critical deterrent to suicide.

1 Introduction

In 2021, about 12.3 million American adults had experienced serious suicidal ideation, and about 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. In 2022, the Department of Health and Human Services launched a new Suicide and Crisis Lifeline – "988" – that has already 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). Research on the

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effects of socio-economic phenomena and policies on a large and diverse group of adults may not provide sufficient guidance for policy if the overall findings mask substantial heterogeneous effects within different subpopulations. In particular, descriptive evidence indicates that patterns and circumstances of suicide vary substantially by 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 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. Using three years of post-adoption data, Cook and Ludwig (2000) found 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 advances the literature in two ways: by using superior data and a superior methodological estimation approach. 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 (e.g. (Depew and Swensen, 2022; Lang, 2012)). Instead, we obtained restricted access CDC mortality files that enabled us to use county-year as

[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 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).

[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.

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 implement estimators that are more robust to dynamic non-treatment effects. While researchers studying and applying difference-in-difference estimators have become increasingly worried about the bias caused by heterogeneous treatment effects (Roth et al., 2023), less attention has been paid to the impact of dynamic effects of *nontreatment* variables (the bias introduced by the "variance weighted common-trends" described in equation (15b) of Goodman-Bacon (2021)). While we do not expect a dynamic treatment effect of a purchase delay law on firearm suicides, we do have reason to be concerned that dynamic effects in nontreatment variables may be biasing estimates of our treatment effect due to the substantially different state trends in firearm suicides in recent years (Edwards et al., 2017).

The problem of dynamic effects in nontreatment variables, whether included as covariates or not, is less tractable than that of dynamic treatment effects, but researchers can still employ methods to constrain its influence. To address dynamic effects in nontreatment variables, we use a stacked regression approach popularized by Cengiz et al. (2019). While the stacked-regression approach is typically thought of as a technique that can eliminate bias due to heterogeneous *treatment* effects, it also has attractive properties that can help minimize the influence of heterogeneous *non-treatment* effects.

The remainder of this paper proceeds as follows. Section 2 describes the data used to complete 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 main 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, since 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 purchase delay 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

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

November 1998. Sixteen states stopped enforcing handgun waiting periods at seven points between 1996 and 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). 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 (Bureau, 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). The appendix contains more details on our household gun ownership proxy and other data sources.

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 while employing a newer estimation approach to address some important possible threats to identification. One such threat is the existence of dynamic effects in both observed and non-observed variables that affect suicide in our study period. States have exhibited substantially different trends in firearm suicides over the past 30 years, suggesting that including geographic and year fixed effects alone cannot sufficiently address these underlying trends (Edwards et al., 2017). While researchers studying suicide have typically used geographic-specific time trends to address this concern,[4] this approach can still be problematic. These time trends can only capture geographic specific effects that move according to the pre-specified trend (e.g., in a linear or in a quadratic fashion) and they also have the potential to increase bias on the treatment effect coefficient in the absence of a trend (Wolfers, 2006).[5]

An additional concern is that if the relationship between a covariate and firearm suicide is changing over time, including the covariate in a regression is not sufficient to satisfy a conditional parallel trends assumption. Thus, we may also be concerned about dynamic effects over time for our observed covariates as well. While the racial composition of a geographic unit is commonly used as a covariate in regressions studying suicide, race-specific firearm suicide rates have been trending quite differently post-2000. Alcohol consumption per capita, also considered an important predictor of suicide, has declined dramatically since the 1970s, suggesting that the relationship between alcohol

[4] See e.g., Classen and Dunn (2012); Edwards (2013); Edwards et al. (2017); Ruhm (2016).

[5] Furthermore, one could incorporate geographic-specific time trends into the stacked approach we use, so using the stacked approach does not preclude the possibility of using geographic specific time trends. However, given the stacked approach's narrow time window, we believe that a geographic-specific time trend would be inappropriate for our current context.

consumption and suicide may have been changing over our study period.

To combat bias introduced by heterogeneous effects from non-treatment variables (both omitted and observed), we implement a stacked-regression approach. The stacked regression constrains the event window studied, removing the observations temporally furthest away from the treatment event. It is reasonable to assume that the difference in the size of the dynamic non-treatment variable effects increases as the length of the time interval increases, so constraining the event window eliminates the observations that most strongly bias our estimate. The stacked regression, however, also has other attractive properties beyond simply constraining the event window. The stacked specification includes both observed covariates and geographic fixed effects for each treatment event (a 10-year panel in our case). This flexible approach allows covariate estimation to be more narrowly tailored to smaller time frames rather than assuming a uniform impact of a covariate across all 33 years of data. The county fixed effects now control for all variables that stay constant within the county for the 10 years surrounding the treatment event, instead of only variables that stay constant within the county for the entire 33-year study period. Note that there is nothing inherently unique about the stacked estimator that leads to these properties – for example, one could also construct a non-stacked regression that contained the same construction of covariates and geographic fixed effects as a stacked regression. However, the stacked regression approach is appealing because it naturally sets the intervals captured by the fixed effects and covariates to the most relevant time periods. To minimize the impact of dynamic non-treatment variable effects on our treatment variable, our stacked-regression approach will absorb the impact of covariates and geographic fixed effects on the time period directly surrounding each treatment event.[6] Using the stacked-regression approach also allows us to flexibly select which control groups are used for each treatment event.

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.[7] 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 $t = (-5, \ldots, 4)$.[8] 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 respectively. We select the control group that matches the treatment status of the county prior to event h.[9]

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 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:

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

[6] Again, we are assuming that the size of the dynamic effects grows over time.

[7] Here, we round adoption and repeal dates to the nearest year.

[8] In the appendix, we show that re-estimating this stacked regression using either an 8 or 6 year panel yields qualitatively similar results.

[9] 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.

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.[10] The coefficient β 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.[11]

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:

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

While we have stressed the potential bias-reducing advantages of our use of a narrow window in our estimation approach, one might worry that the full impact of legal interventions might take longer than four years to fully manifest themselves. In this case, the narrow window might underestimate the full impact of the treatment. This concern may well be significant for legal interventions that influence behavior only as the public becomes aware of and responds to the legal change, which in many cases could be a slow process that might take years. For example, a legislative increase in a criminal sanction or a prosecutorial decision to seek longer sentences could only generate deterrence once the potentially criminal population becomes aware of the change, which a large literature has shown is rarely immediate and never complete. Our legal intervention is not of that type, however, since the purchase-delay laws we study curtail the immediate purchase by changing the behavior of the licensed firearm seller, so public ignorance is irrelevant and one would expect to see a far quicker impact from such an intervention.

[10] 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.

[11] 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 be equal to one, effectively studying the rate of suicides instead of the count of suicides. In practice, including population as a regular explanatory variable minimally changes our results.

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.[12] The estimates presented in Table 1 and throughout the paper (unless otherwise noted) use incident-rate-ratios (IRRs) for ease of interpretation.

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

	(1)	(2)	(3)	(4)
		Stacked Results		
Handgun Purchase Delay	0.964**	0.934**	0.968	0.976
	(0.013)	(0.020)	(0.020)	(0.018)
Age	All Adults 21+	Aged 21-34	Aged 35-54	Aged 55+
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+	Aged 21-34	Aged 35-54	Aged 55+
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 2. All regressions use population as an exposure variable.

A traditional non-stacked TWFE estimator is vulnerable to bias from dynamic effects in non-treatment variables for the reasons stated in Section 3. Although the bias can theoretically operate in 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

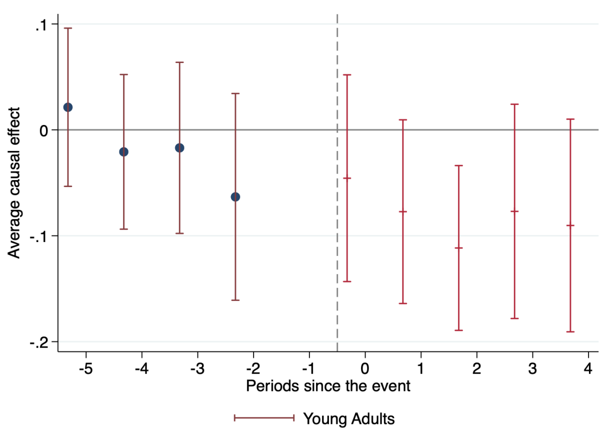
[12] The non-stacked TWFE estimation follows a specification similar to stacked estimates. The general regression equation for the non-stacked estimation is: $Y_{it} = \alpha + \beta PurchaseDelay_{it} + \chi_{it}^{'} + \delta_i + \lambda_t + \epsilon_{it}$.

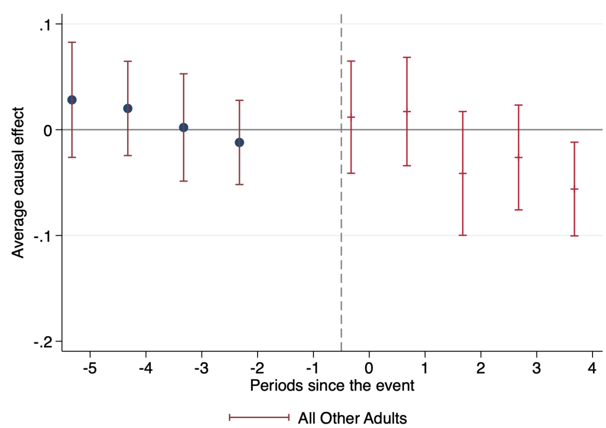
because of our county-level data, Poisson regression specification, and slightly different time period and covariates, our results provide suggestive evidence that prior research on purchase delay laws may have yielded biased estimates due to heterogeneous effects in non-treatment variables.

Given the weaknesses 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 3.6% (IRR=.964). 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 6.6% (IRR=.934) drop in firearm suicide.[13] For middle-aged and 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 reflects an absolute decrease in total suicides.

In Figure 1, we also present event plots corresponding to the analyses in Table 1.[14] The finding that the pre-passage dummies are not statistically significantly different from zero supports the assumption of parallel trends across age groups.[15]

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





Note: The vertical lines show the 95 percent confidence intervals with cluster-robust standard errors.

[13]A two-sample t-test confirms that the drop in firearm suicides due to handgun purchase delay laws is statistically larger for young adults than it is for middle-aged or older adults at the p<.01 level.

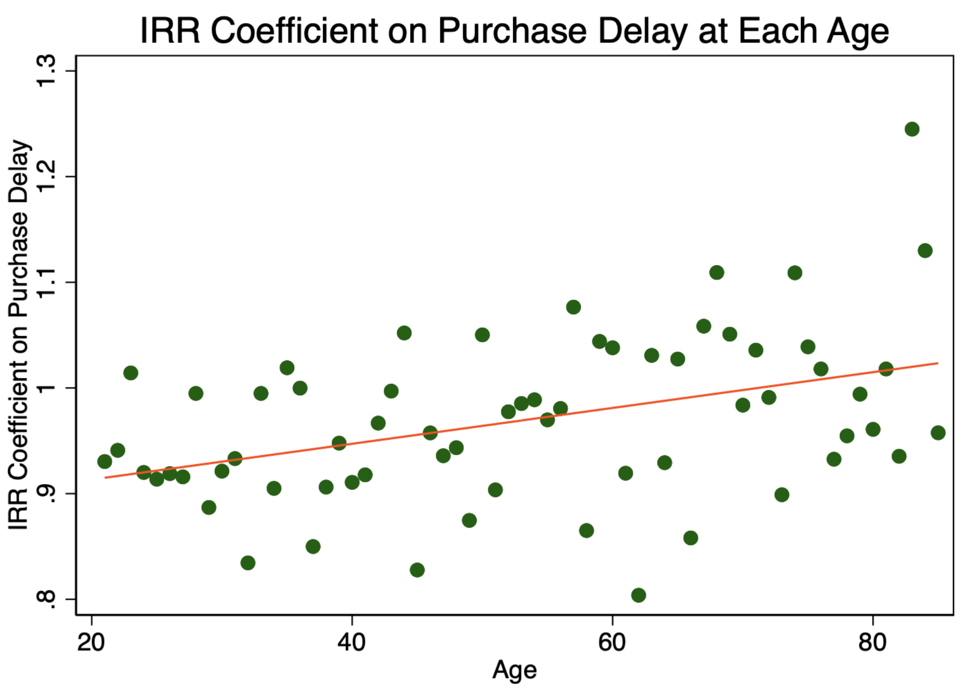
[14]To clarify the figure visually, we run event study analyses for young adults and all non-young adults rather than having four different event study analyses corresponding to the four columns of Table 1. The event studies directly corresponding to each column of Table 1 are qualitatively similar and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

[15]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.

4.2 Single Age Estimation

The age groups presented in our Table 1 analysis 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.[16] 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 8.5 percent but the effect falls to zero by around age 71.[17] 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 and use population as an exposure variable, as described in Section 2.

[16]We use the same model that 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.

[17]The slope of the fitted line is .002 with a p-value of .001.

4.3 Gun Ownership and Effect Size

Presumably purchase delay laws will only be effective at deterring suicide for those individuals who do not already have a firearm accessible to them and therefore must purchase a new firearm to achieve their goal – however evanescent – of ending life. 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.[18] However, we now show that our results hold at the state-level when we control for gun ownership. We consider how the effectiveness of purchase delay laws varies by *both age and household gun ownership*. We use a dataset at the state-age group-year observation level[19] and run the following interaction models shown in columns (1) and (2) of Table 2, respectively:

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

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

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 roughly 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 an OLS regression instead of a Poisson estimation. We weight our regression by population.

[18] 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.

[19] 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 household gun ownership in our sample is 37% with a standard deviation of 3.5%.

Table 2: Purchase Delay Laws Effect on Firearm Suicide Rates by Age Group and Gun Ownership at State Level, 1987-2019, OLS Stacked Estimates

	(1)	(2)
Handgun Purchase Delay	-0.109***	-0.110***
	(0.040)	(0.039)
Gun Ownership x Handgun Purchase Delay	0.085	0.083
	(0.087)	(0.086)
Gun Ownership	-0.056	-0.055
	(0.089)	(0.089)
Middle Effect (Relative to Young)	0.032	
	(0.036)	
Old Effect (Relative to Young)	0.074**	
	(0.036)	
Effect As Age Bucket Increases		0.037**
		(0.018)
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.

4.4 Further Robustness

In addition to providing evidence supporting the conclusion that our main results are robust to 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 heterogeneous 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 two significant contributions to the economics literature on the effect of handgun purchase delay laws on firearm suicide and impulsivity in firearm suicide. First, it applies the stacked regression estimator, which is primarily thought of as a way to combat bias caused by heterogeneous treatment effects, but we show it can also be effective at decreasing the bias caused by heterogeneous effects caused by non-treatment variables. Our analysis found that the difference between the results from the traditional TWFE regression and stacked regression approach was substantial, suggesting that the limitations of data quality and methodology of the prior literature may have led to inflated estimates of the magnitude of the benefits of handgun purchase delay laws as a policy intervention in reducing firearm suicide. Second, 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 to quasi-experimentally identify and estimate the relationship between age and suicide impulsivity, supporting hypotheses of this relationship and providing an estimate of the strength of this relationship.

Our paper adds support to the idea, so far understudied by economists, that young people are more impulsive in their decision to commit suicide. Our empirical approach also indicates that more research on suicide should test for heterogeneous treatment effects by age group. For example, to confirm the suicide-impulsivity hypothesis beyond firearm suicide, researchers may look at policies associated with non-firearm suicide by age. Doing so may provide additional clarity and robustness to researchers as well as important information to policymakers. 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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