European Economic Review 145 (2022) 104113

Contents lists available at ScienceDirect

European Economic Review

journal homepage: www.elsevier.com/locate/eer

# Is assisted suicide a substitute for unassisted suicide?

Sourafel Girma [a], David Paton [b,*]

[a] Nottingham University School of Economics, Nottingham University, University Park, Nottingham, NG7 2RD, UK
[b] Nottingham University Business School, Jubilee Campus, Nottingham NG8 1BB, UK

ARTICLE INFO

*JEL codes:*
D81
I12
J18
K36

*Keywords:*
Posner
Physician assisted suicide
Assisted dying

ABSTRACT

Posner hypothesised that the legalisation of assisted suicide may substitute for unassisted (unregulated) suicide. We test predictions arising from this hypothesis using data from US states that have legalised assisted suicide. Event study regression estimates provide strong evidence that legalisation of assisted suicide is associated with an increase in total suicides, especially for females and older people. There is some evidence that assisted suicide laws are also associated with a smaller increase in unassisted suicide, though the statistical significance is weaker than for total suicides. Results using the synthetic control method (SCM) are generally consistent with the regression estimates. Overall the US experience to date provides little evidence in support of the Posner substitution hypothesis.

## 1. Background

Laws permitting assisted suicide, sometimes also referred to as physician assisted dying or medical aid in dying, allow terminally ill patients to receive a prescription for a lethal drug which the patient may then administer to cause their own death.

Notably, since 1997, assisted suicide laws have been introduced in a variety of US states, starting with Oregon in 1997. Although there are some differences in eligibility across states, most of the laws are very similar to that implemented in Oregon[1] and this is the model that a number of other legislatures around the world have sought to copy.[2]

Although assisted suicide laws have been the subject of significant analysis and debate in the fields of moral philosophy and medical ethics, little attention has been paid to them in the economics literature. An important exception is the work of Richard Posner predating the implementation of the Oregon law and which examined the impact of assisted suicide laws on suicide in general.

Posner (1995) argued that assisted suicides are likely to substitute for unassisted (unregulated) suicides. Further, he argued that having the option of assisted suicide would cause people considering suicide to delay their decision. As a result, he predicted not only that legalisation of assisted suicide would lead to a reduction in unassisted suicide but that, conceivably, the total number of suicides (assisted and unassisted) could decrease.

Posner (1995) attempted to estimate the impact of assisted suicide using state-level data but as his analysis pre-dated the current wave of assisted suicide legalisations, there was insufficient variation in the policy variable to come to any strong conclusion. Since

* Corresponding author.
*E-mail addresses:* Sourafel.Girma@nottingham.ac.uk (S. Girma), David.Paton@nottingham.ac.uk (D. Paton).

[1] The main exception is Montana where assisted suicide was effectively decriminalised through a state supreme court decision rather than a new law.

[2] Examples include Portugal, New Zealand and New South Wales. In other countries such as Canada and the Netherlands, terminal illness is not a necessary requirement for assisted suicide or euthanasia.

https://doi.org/10.1016/j.euroecorev.2022.104113
Received 20 November 2021; Received in revised form 24 March 2022; Accepted 27 March 2022
Available online 9 April 2022

then, there have been very few attempts to test Posner's hypotheses. An exception is Paton and Jones (2015) who present evidence from the earliest legalising states, finding no evidence that assisted suicide causes unassisted suicides to decrease. More recently Jones (2022) looks at trends in a number of European countries that have introduced either assisted suicide or euthanasia concluding that there is "no reduction in non-assisted suicide relative to the most similar [non-legalising] neighbour and, in some cases, there is a relative and/or an absolute increase in non-assisted suicide" (p.1). Finally, although Canetto and McIntosh (2022) do not directly seek to test the Posner hypothesis, they identify a relative increase in suicides amongst older adult women in Oregon since assisted suicide was legalised.

The limited amount of empirical evidence relating to Posner's hypotheses is surprising given that the issue has featured heavily in policy and legal debates surrounding the legalisation of assisted suicide.[3]

In this paper, we seek to address the limited amount of empirical evidence on Posner's hypotheses using state-level data on assisted and unassisted suicide. The fact that assisted suicide has been legalised by different states at different times allows us to exploit state and time-variation to identify possible effects. One difficulty in testing for suicide substitution is that other effects may be observed at the same time. For example, it has been argued (see, for example, Dugdale and Callahan, 2017), that by lowering taboos against suicide, legalisation of assisted suicide may actually lead to an increase in suicide rates. We exploit the fact that assisted suicide is mainly taken up by older adults to construct a more robust test of suicide substitution. We also seek to provide a more formal test of the suggestion by Canetto and McIntosh (2022) that the legalisation of assisted suicide affects suicide patterns in women relative to men.

The fact that there are now data for ten states, and the time series are relatively long for several of these, suggests that an empirical assessment of the impact of legalising assisted suicide in the US so far is warranted.

In the next section of the paper, we elaborate on the Posner hypothesis. In Section 3, we outline our empirical approach and introduce our data. In Section 4, we report the key empirical results. Some concluding remarks are made in Section 5.

## 2. The Posner hypothesis

The essence of Posner's argument for why legalising suicide may actually decrease suicides is the option value of future suicide. People who have no immediate wish to die by suicide may gain utility from knowing that they can exercise the option of suicide at some point in the future should their life experience deteriorate. Someone in the early stages of a degenerative illness may worry that when their quality of life has deteriorated to such an extent that they no longer wish to live, they will no longer be able to die by suicide without assistance. As a result, they die by suicide at an early stage.

Now if that same person has the assurance that assisted suicide will be available should they end up wishing to die in the future, they may be less likely to die by suicide at an early stage of their illness and their suicide is delayed. Assisted suicide will substitute for unassisted suicide.

Further, the actual state of mind of that person in the future cannot be known with certainty. At the point when the person believed they would wish to die by suicide, they may actually find that they no longer wish to do so. Or, alternatively, a diagnosis may be wrong or unduly pessimistic and future life is less painful or debilitating than originally expected. In either event, the person may never actually take up the option of suicide. In this case, the availability of assisted suicide will reduce the total number of suicides.[4]

A simple model of expected utility can demonstrate these effects formally. Following Posner (1995), consider a person who has been diagnosed with a degenerative illness and is considering dying by suicide as a result. If she does not die by suicide and the diagnosis proves correct, she will eventually end up in a state in which (she believes) her quality of life will be worse than if she was dead, i.e. the utility if the diagnosis is right ($U_R$) is negative. Further, in that state, she will be so incapacitated as to be unable to die by suicide without assistance. If the diagnosis proves to be wrong, she believes her future utility ($U_W$) will be positive. The probability of the diagnosis being correct is defined as $p$. No diagnosis is 100% certain, but it is reasonable that the person believes it more likely than not to be correct, i.e. $p > (1 - p)$. Finally, suicide is assumed to involve a cost, c, which might include pain, moral objections and so on.

If the expected utility is greater than zero, the individual will never die by suicide. If expected utility is negative, the individual will die by suicide if the value of the negative expected utility exceeds the cost of suicide, i.e.:

$$pU_R + (1-p)U_w < -c \tag{1}$$

Now consider the case in which the individual knows that, in the event of the diagnosis being correct and a terminal diagnosis received, assisted suicide will be legally available. For simplicity, assume assisted suicide is available at the same cost, c. If the diagnosis is correct, and assuming $c < -U_R$, the individual will now die by (unassisted) suicide if:

$$-pc + (1-p)U_w < -c \tag{2}$$

which simplifies to:

$$U_W < -c \text{ which is never satisfied for } U_W > 0,\ c > 0 \tag{2a}$$

In other words, as long as there is a positive probability of a diagnosis being wrong, assisted suicide will deter unassisted suicide prior to the degenerative state being experienced. So in the first place, assisted suicides substitute for unassisted ones and the former

[3] For example, see Carter vs Canada (Attorney General) (2015) and Dignitas (2014).

[4] A third possibility not explored in our model is that the passage of disease is less serious than originally anticipated.

take place later than the latter would have done. Further, some unassisted suicides are avoided completely. This would be the case for the proportion, 1 – p, of cases where the diagnosis turns out to be wrong but also where the diagnosis is correct, but the actual utility of living in the degenerative state turns out to be positive. As a result, Posner suggests that, when assisted suicide is legalised, there will be fewer suicides in total (assisted and unassisted) and those that do take place occur later than they would otherwise have done.

Note that an individual for whom the cost of suicide exceeds the anticipated disutility of the degenerative state ($c > -U_R$) will not die by suicide whether or not assisted suicide is legal.

The situation is a little more complicated if we relax the assumption that the cost of assisted and unassisted suicide are the same. With assisted suicide, drugs are supplied by a doctor and advice and guidance on dying by suicide is provided. For some people, legal sanction with assisted suicide may also reduce the cost of any moral reservation about suicide. As a result, we can expect the costs of assisted suicide to be lower than the costs of unassisted suicide. Defining the cost of assisted suicide to be $c_A$ where $c_A < c$, Eq. (2) above becomes:

$$-pc_A + (1-p)U_w < -c \tag{3}$$

Now the effect of legalising assisted suicides on total suicides becomes ambiguous. Amongst individuals for whom $c < -U_R$ (i.e. the cost of unassisted suicide is less than the anticipated disutility of the degenerative state), assisted suicide continues to substitute for unassisted suicides and total suicides reduce. Some individuals for whom the cost of unassisted suicide was prohibitively high, may take advantage of lower-cost assisted suicide if they end up in the degenerative state. Amongst this group, assisted suicides have not substituted for unassisted ones.

The empirical predictions of Posner's model are two-fold. First legalising assisted suicide should reduce unassisted suicides to some extent. Further, legalisation may, under some circumstances, actually reduce the total number of suicides (assisted and unassisted) combined.

The latter prediction appears counterintuitive. The key to understanding it is to recognise that unassisted and assisted suicide are two distinct 'goods' which substitute for each other. As Posner (1995) puts it:

> "lowering the price of the second (by legalising it) will reduce the demand for the first and nothing in economics teaches that this reduction must be fully offset by the increased demand for the second good." (p.250).

We can imagine other effects of legalising assisted suicide which could affect the Posner predictions. In the first place, for some patients, the course of disease may end up being worse than envisaged at the time of the diagnosis, thus inducing some patients who would not otherwise have died by suicide to take advantage of assisted suicide further into disease progression.

It has also been argued (Niederkrotenthaler et al., 2012; WHO, 2008; Sisask and Värnik, 2012) that publicity and media coverage of suicide can induce an increase in suicide in a phenomenon known as 'suicide contagion'. Given that assisted suicide laws are generally associated with significant media discussion and coverage and also that passing of such a law may lead to a weakening of more general societal taboos against suicide (implying a reduction in the cost, $c$, of unassisted suicide), then assisted suicide may be associated with an increase in unassisted suicide, countering (to a greater or lesser extent) the Posner effects.

Empirical analysis of actual assisted suicide laws should, at a minimum, help to identify what the net effect is of these two opposing factors on total suicide rates. Given that the effects are not mutually exclusive, we are less likely to be able to identify the magnitude of each effect separately. However, a feature of Posner's analysis may be able to help this identification. Posner predicts that any suicide-reduction effect will be observed most strongly amongst older adults. The reason is that assisted suicide laws are targeted at those with terminal illness, something which is much more prevalent in older adults. In contrast, suicides amongst working age people are relatively more likely to be associated with depression or economic factors such as job loss (McKeown et al., 2006; Nordt et al., 2015). A clear empirical prediction is that, if Posner's substitution effect is present, we are likely to see a bigger (negative) effect of assisted suicide laws on unassisted suicides amongst older people relative to younger age groups.

## 3. Empirical approach and data

### 3.1. Panel event study

We are interested in estimating the causal effects of the passage of an assisted suicide law in state $s$ at time $t$, on suicide rates which we denote as $y_{st}$. We define a binary variable, $Assisted_{st}$ (the event), which takes the value of 1 if state $s$ adopted the law at time $t$, with the possibility of different states adopting the law at different times.

In our empirical implementation, we use the following policy states with the assisted suicide passage year in parentheses: Oregon (1998); Washington (2009); Montana (2010), Vermont (2013), California (2016), Colorado (2016), the District of Colombia (2017), Hawaii (2019), Maine (2019) and Montana (2019).

Our baseline model is a two-way fixed effects panel event study specification which is expressed as follows:

$$y_{st} = \alpha + \sum_{j=2}^{7} \lambda_j \ PAS_{st+j} + \sum_{k=0}^{7} \gamma_k Assisted_{st-k} + X_{st}'\beta + \mu_s + \tau_t + \varepsilon_{st} \tag{4}$$

In the above equation $\varepsilon_{st}$ is a stochastic error term; $\mu_s$ represents state-specific unobserved fixed effects and $\tau_t$ denotes year fixed effects that encapsulate aggregate trends in suicide rates. On the other hand, $\lambda_j$ and $\gamma_k$ are the coefficients associated with lead and lag terms of the event respectively. We amalgamate the lead and lags terms beyond 7 years, and as is the convention, we also omit the first

lead variable. The inclusion of the lead variables is designed to rule out the possibility that any observed post-policy differentials are due to pre-policy suicide trends. $X_{st}$ are time-varying covariates, and include a host of characteristics that are likely to affect the variation of suicide rates across and within states (see Section 3.3 for details).

We run the model for both total and unassisted suicide rates using all 51 US states, weighting the regressions by state populations. In each case, we present estimates for population-wide suicide rates and separate estimates for males and females, and for three age groups: under-35, 35–64 and over 64s. We compute the standard errors of the regression parameter estimates by allowing the disturbances to be heteroscedastic and contemporaneously correlated across states. The disturbances are also allowed to exhibit first-order serial correlation within states in order to account for persistent unobservable factors driving state-specific suicide rates dynamics.

We explore the robustness of the baseline estimates in a number of ways. In the first place, we estimate specifications including state-specific trends. Given that we already include pre-event variables to control for pre-existing trends, adding in differential trends is a particularly demanding test of how robust are our baseline estimates.

Next, we estimate Eq. (4) using two placebo dependant variables: death rate by injury and the birth rate. These are unlikely to be affected in any significant way by assisted suicide laws. If our event variables are estimated to have a significant impact on our placebo variables, it would cast doubt on our ability to infer any causal relationship from the estimated impact on suicide rates.

We also consider an alternative approach to estimating standard errors by implementing the wild bootstrap procedure proposed by Clarke and Tapia-Schythe (2021).[5]

Finally, we address the possibility that the two-way fixed effects difference-in-differences (DD) estimator may be biased due to variation in treatment timing and treatment effect heterogeneity. Goodman-Bacon (2021) shows that the DD coefficient is a weighted average of all 2*x*2 DD designs that can be constructed from the data, where the weights are functions of the timing of the treatment and the subsamples share of observations. Unfortunately, some of these weights can be negative, implying that the two-way fixed effect DD estimate might not even be a convex combination of the 2*x*2 DD effects. To explore the existence of such a possibility in our baseline estimates, we compute these weights for our suicide rate variables and confirm the absence of negative weights.[6] In fact, 80% of the weights are due to comparing states with no PAS law versus those that adopted the law across the 7 timing groups. This suggests that the theoretical bias associated with using the standard two-way fixed estimator is unlikely to be materially significant in our sample. Despite this and for completeness, we also present estimates of a staggered difference-in-difference model with dynamic and heterogeneous treatment effects following the approach of Sun and Abraham (2021).[7]

### 3.2. Synthetic control method

Given the small number of states in which assisted suicide has been legalised, a natural question is whether any association in standard fixed effects models is causal or, rather, due to systematic differences in trends between states that legalise assisted suicide and others. Our inclusion of lead terms in the event study framework is one way of controlling for this. We also explore another approach to the problem by presenting estimates using the synthetic control method (SCM) due to Abadie et al. (2010, 2015).

The SCM has proved a powerful policy evaluation tool in that it creates synthetic control states that simulate the suicide rates paths of affected states had they not passed the law. The creation of such a hypothetical counterfactual proceeds by taking a weighted average of pre-policy characteristics (also known as predictor variables) of non-affected states (or donor states). The predictor variables are typically variables that affect both the outcome variable (in our case suicide rates) and the policy itself.

To the extent that the resulting synthetic sates closely mimic the policy states during the pre-policy period, any difference in outcome between the two groups in the post-policy period can be taken as the causal effect of the policy. A useful feature of the SCM is that, unlike the difference-in-differences and panel fixed effects strategies, it does not rely on the parallel trends assumption for identification. Moreover, the weights assigned to the donor states are made explicit by the method so that one can gauge the similarity of the control states to the relevant policy state.

One downside of our SCM approach is that we exclude from the analysis several states that passed an assisted suicide law late in our sample and for which we do not have sufficient post-policy observations. Specifically, we only use the following policy states: Oregon, Washington, Montana and Vermont. The remaining policy states are excluded from the analysis altogether, and the control group consists of the 'never treated' states.

We match states with and without assisted suicide based on the predictor variables presented in Eq. (4) and the relevant outcome variable averaged over the relevant pre-policy period. Our preferred approach is to match on the levels of the outcome variable. However, we also report estimates based on matching on trends which scales the dependant variable for both policy and control states to one at the year of the passing of the law.

Inference in SMC is based on p-values which are calculated as the proportion of placebo effects that are at least as large of the effect amongst the treated units, and standardized p-values obtained by first dividing all the effects by the corresponding pre-treatment match quality. This helps correct for the possibility that placebo effects may be quite large amongst untreated units with poor pre-

[5] We use the user-written Stata program "eventdd" as explained in Clarke and Tapia (2021). We do not use a totally unrestricted within-state correlation as this is theoretically problematic given that the number of clusters in our sample (N = 51) is not only moderate, but also not substantially larger than the number of observations per state (T = 30).

[6] We use the "bacondecomp" described in Goodman-Bacon, Goldring and Nichols (2019).

[7] We employed the user-written Stata program "eventstudyinteract" described in Sun and Abraham (2021).

**Table 1**
Timing of Assisted Suicide laws/decriminalization.

| State | Assisted Suicide law in effect | Notes |
|---|---|---|
| Oregon | January 1998 | The law was notionally in effect from 27th October 1997 but in practice the law was only applied from January 1998. |
| Washington | March 2009 | |
| Montana | January 2010 | There is no assisted suicide law in place in Montana but the state supreme court ruled that assisted suicide should be considered decriminalised from January 2010 |
| Vermont | June 2013 | |
| California | July 2016 | Due to a court ruling, subsequently overturned, the law was in abeyance for approximately 1 month in 2018. |
| Colorado | December 2016 | |
| District of Columbia | June 2017 | |
| Hawaii | January 2019 | |
| New Jersey | August 2019 | |
| Maine | September 2019 | |

treatment match quality, resulting in p-values that are too conservative.[8]

### 3.3. Data

We collected data from published sources (chiefly the CDC) on the annual number of suicides in each state between 1990 and 2019. We supplemented this with data from State Health Departments on the number of assisted deaths in states where assisted suicide has been legalised. Note that assisted deaths are not included as suicides in the CDC series. We then constructed annual series of unassisted and total suicide rates per 100,000 population. We also collected suicide rates separately for males and females and for three age groups: under-35, 35–64 and over-64.

We constructed an indicator variable for whether an assisted suicide law was in effect (or for Montana, decriminalised) in a state in that year. By the end of 2019, 10 states had some form of assisted suicide law in effect. The timing of the introduction of these laws is summarised in Table 1.

Suicide rates in a state may be affected by a range of other variables. Some of these will be accounted for by year and state fixed effects. However, we also collected data on a range of demographic and socio-economic factors which vary over time and across states.

In the first place, suicide rates vary considerably between different demographic groups (Stack and Kposowa, 2007). For example, suicide is much less likely amongst the young and also amongst certain racial or ethnic groups. For this reason, we include as co-variates the percentage of the population in each state-year that is under-20, over-64, black, Hispanic, Asian or Pacific Islander and American Indian or Alaskan Native. Data on these variables are taken from the CDC state bridged-race population estimates.

Economic circumstances such as unemployment are known to affect suicide rates (Nordt et al., 2015) albeit estimates of the association vary by country, sex and ethnicity (see, for example, Gassman-Pines, Oltmans and Gibson-Davis, 2014). We include state unemployment rates, taken from the Bureau of labour Statistics, and per capita disposable income (adjusted for inflation) from the Bureau of Economic Analysis.

A number of studies have found substance abuse to be associated with suicide rates (for example, Elder et al., 2010; Freeman, 2007) and so we include variables to pick up effects relating to alcohol and drugs. Interestingly the direction of the association is not always clear. Anderson et al. (2015) report some evidence that Marijuana legalisation may have led to fewer suicides. On the other hand, the CDC has recently suggested that opioid drug abuse may have contributed to recent increases in US suicide rates.

To allow for such effects, here we include the per capita alcohol consumption in each state-year using data from the National Institute on Alcohol Abuse and Alcoholism, Surveillance Reports. We further include indicator variables for state-years in which medical marijuana was legal and for whether marijuana possession was decriminalised. Consistent data on opioid use are not available for the relevant period. Instead, we collect the deaths rate in each state-year from accidental poisoning from the CDC morality data.[9]

Finally, gun availability has frequently been cited as a contributory factor in suicides (Brent and Bridge, 2003). We follow other studies (see, for example, Azrael et al., 2004) in using the proportion of suicides in which firearms were used as a proxy for availability.[10]

[8] All SMC estimations are conducted using the user-written Stata package "synth_runner" as described in Galiani and Quistorff (2017).

[9] The vast majority of accidental poisoning deaths are known to be the result of drug use (CDC, 2007).

[10] We also experiment with other measures of gun ownership including proportion of the population with hunting licenses. These alternative measures have very little impact on our key results.

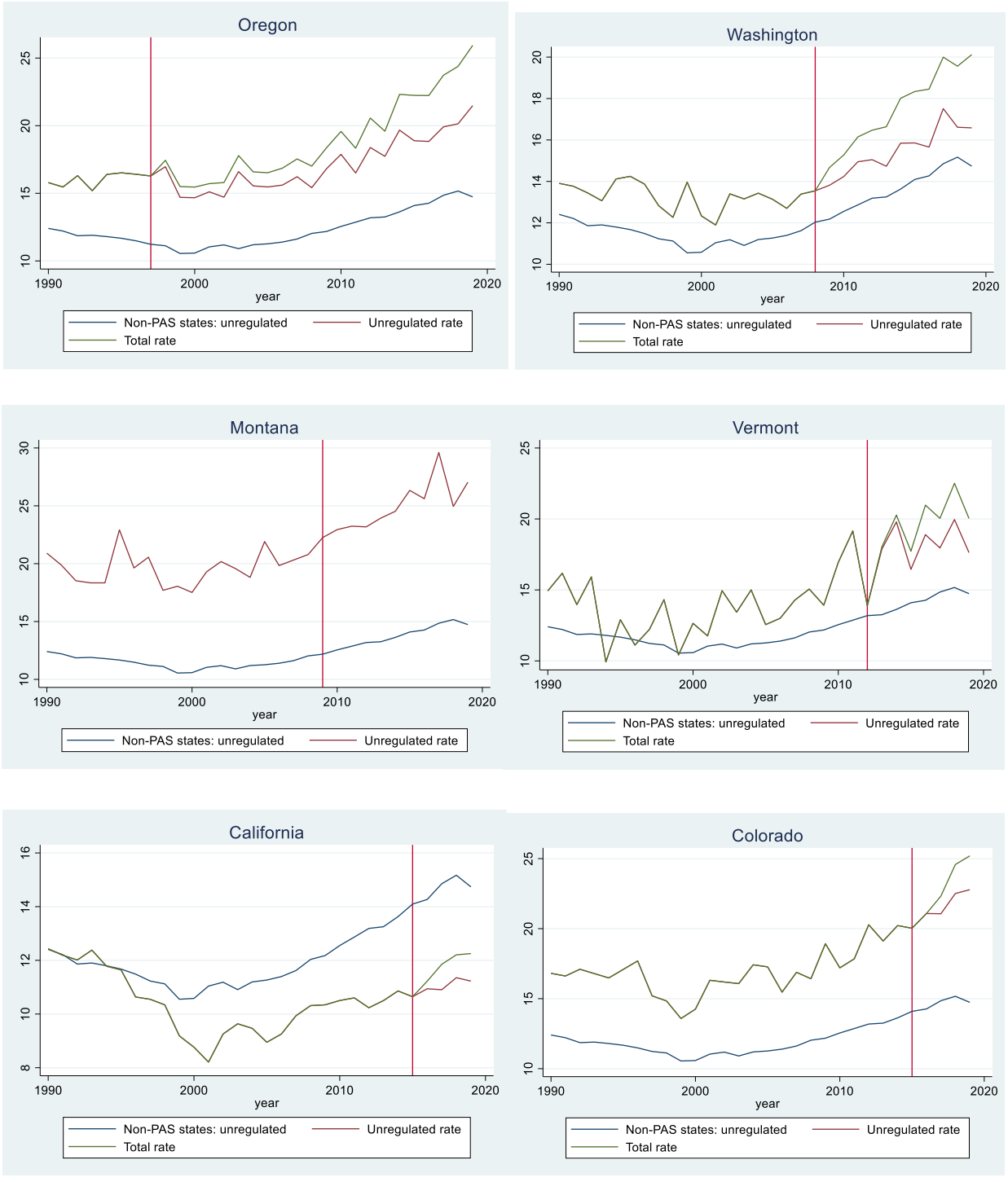



**Fig. 1.** Trends in annual suicide rates per 100,000: assisted suicide states vs others. **Notes:** (i) Vertical line indicates the final year before an assisted suicide law was in effect in that state. (ii) There are no data on the numbers of assisted suicides for Montana.


**Fig. 1.** (*continued*).

**Table 2**
Descriptive statistics for all variables in 1997.

| | All states | | Assisted suicide states | | Non-assisted suicide states | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD |
| Unassisted suicide rate | 12.51 | 3.62 | 12.32 | 4.26 | 12.55 | 3.50 |
| Unassisted suicide rate male | 20.59 | 5.87 | 19.71 | 6.87 | 20.81 | 5.68 |
| Unassisted suicide rate female | 4.68 | 1.49 | 5.09 | 1.62 | 4.58 | 1.46 |
| Unassisted suicide rate O64 | 17.46 | 6.43 | 17.05 | 8.11 | 17.56 | 6.07 |
| Unassisted suicide rate 35–64 | 16.07 | 4.79 | 16.09 | 5.46 | 16.07 | 4.69 |
| Unassisted suicide rate U35 | 8.60 | 2.61 | 8.22 | 2.82 | 8.72 | 2.60 |
| Black | 10.94 | 11.96 | 9.56 | 18.91 | 11.28 | 9.88 |
| Hispanic | 6.89 | 8.42 | 8.96 | 9.00 | 6.38 | 8.31 |
| Asian or Pacific Islander | 3.30 | 8.63 | 9.45 | 18.84 | 1.80 | 1.22 |
| American Indian or Alaska Native | 1.60 | 2.99 | 1.21 | 1.78 | 1.69 | 3.23 |
| Over-64 | 28.84 | 2.17 | 27.89 | 1.75 | 29.08 | 2.22 |
| Under-20 | 12.63 | 1.98 | 12.39 | 1.34 | 12.69 | 2.11 |
| Unemployment | 4.73 | 1.17 | 5.43 | 1.28 | 4.56 | 1.10 |
| Real income | 29,409 | 3919 | 31,448 | 4936 | 28,912 | 3525 |
| Alcohol | 25,652 | 5699 | 27,824 | 4781 | 25,123 | 5830 |
| Medical marijuana | 0.02 | 0.14 | 0.10 | 0.32 | 0.00 | 0.00 |
| Decriminalised marijuana | 0.20 | 0.39 | 0.30 | 0.48 | 0.17 | 0.37 |
| Drug deaths | 3.21 | 1.94 | 3.83 | 1.77 | 3.06 | 1.97 |
| Suicide by guns | 57.75 | 12.46 | 50.70 | 13.09 | 59.46 | 11.84 |
| Number of states | 51 | | 10 | | 41 | |

**Notes:** figures are state means and standard deviations for the year 1997, the year immediately prior to the first state (Oregon) legalising assisted suicide. Assisted suicide states are California, Colorado, District of Columbia, Montana, Oregon, Vermont and Washington, New Jersey, Maine and Hawaii. Suicide rates are per 100k population.

**Table 3**
Total suicide rates and assisted suicide laws.

| | Overall | Female | Male | Under 35 | 35–64 | Over 64 |
|---|---|---|---|---|---|---|
| Years from passing of law: | | | | | | |
| *Same year* | 0.812*** | 0.436* | 1.143** | 0.254 | 1.156** | 1.704** |
| | (0.3049) | (0.2417) | (0.4966) | (0.2514) | (0.5487) | (0.7528) |
| *One* | 0.967** | 0.943*** | 0.892 | −0.198 | 0.782 | 5.201*** |
| | (0.3829) | (0.3025) | (0.6195) | (0.3083) | (0.6821) | (0.9183) |
| *Two* | 1.228*** | 1.305*** | 1.136* | 0.136 | 1.368* | 4.427*** |
| | (0.4016) | (0.3239) | (0.6314) | (0.3078) | (0.7097) | (0.9593) |
| *Three* | 1.722*** | 1.205*** | 2.160*** | 0.699** | 1.344* | 5.498*** |
| | (0.4232) | (0.3203) | (0.6731) | (0.3279) | (0.7372) | (0.9416) |
| *Four* | 1.886*** | 1.537*** | 2.287** | 0.257 | 1.441 | 9.443*** |
| | (0.6236) | (0.5525) | (0.9502) | (0.5124) | (0.9834) | (1.9039) |
| *Five* | 3.581*** | 2.964*** | 4.059*** | 0.330 | 5.152*** | 10.114*** |
| | (0.6393) | (0.5637) | (0.9622) | (0.5214) | (0.9965) | (1.9130) |
| *Six* | 3.249*** | 4.022*** | 2.390** | 0.465 | 4.048*** | 9.864*** |
| | (0.6077) | (0.5429) | (0.9313) | (0.5124) | (0.9444) | (1.9684) |
| *Seven and more* | 4.267*** | 3.695*** | 4.770*** | 0.681** | 4.829*** | 13.459*** |
| | (0.4427) | (0.3357) | (0.6624) | (0.3318) | (0.6530) | (1.0161) |
| N | 1520 | 1513 | 1513 | 1513 | 1513 | 1513 |
| ant_p | 0.763 | 0.741 | 0.623 | 0.838 | 0.857 | 0.499 |
| post_p | 0.000 | 0.000 | 0.000 | 0.200 | 0.000 | 0.000 |
| ATT | 17.711 | 16.107 | 18.836 | 2.626 | 20.118 | 59.709 |
| ATT_p | 0.000 | 0.000 | 0.000 | 0.166 | 0.000 | 0.000 |

**Notes:** (i) Heteroscedasticity and within-state serial correlation corrected standard errors in parentheses.
(ii) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
(iii) N Number of observations.
(iv) ant_p: p-value from test of joint significance of the pre-policy effects.
(v) post_p: p-value from test of joint significance of the policy effects.
(vi) ATT: Total policy effect
(vii) ATT_p: p-value from test that ATT=0.
(viii) Full regression results are given in Appendix B.

## 4. Empirical results

### 4.1. Descriptive statistics

In Fig. 1, we report trends of suicide rates (assisted and, where available, total suicides) for each of the ten states which have introduced some form of assisted suicide law. In each case, we also show the trend for states that have never legalised assisted suicide.

For each of those states where assisted suicide data are available, there is a clear increase relative to the trend in total suicide rates from the point of assisted suicide being legalised. There is little discernible pattern for unassisted suicides, either in the trend line for each state or relative to the trend for all other states.

In Table 2, we present descriptive statistics for all states, states which ever legalise assisted suicide and other states for the year 1997, i.e. just before the first case of assisted suicide legalisation. Unassisted suicide rates were very much higher for males relative to females and for people over the age of 35. Those states which (ever) legalised assisted suicides had slightly higher unassisted suicide rates for males in 1997 compared to other states. Other variables indicate some further small differences between the two groups of states.

### 4.2. Panel event study results

We report the baseline fixed effects estimates of the impact of assisted suicide laws on total (unassisted plus assisted suicides) and unassisted suicide rates in Tables 3 and 4 respectively. In each case we report results for the whole population, for females and males and for three age groups: over-64s, 35–64s and under 35s. In the tables below we report only the coefficients on the assisted suicide variables and lags. The full regression results are presented in Appendix B.

For total suicides (Table 3) the total policy impact of assisted suicide laws is estimated to be positive and strongly significant. The average treatment effect on the treated (ATT) of 17.71 implies an increase of around 2 suicides per 100 thousand per year in assisted suicide states than might have been the case without assisted suicide. Given pre-legalisation suicide rates, that implies an increase of around 18%. Although the ATT is slightly lower for females (16.11), female suicide rates are typically about a quarter those of males. This means the implied percentage increase in total suicides is much bigger (closer to 40%) for females. The estimated effect on total suicides is also relatively large over-64s. In contrast, the ATT for under-35s is small (though still positive) and statistically insignificant, consistent with the fact that assisted suicide is almost non-existent in that group.

The joint impact of the pre-event variables in this and most other specifications do not come close to statistical significance (p-value

**Table 4**
Unassisted suicide rates and assisted suicide laws.

| | Overall | Female | Male | Under 35 | 35–64 | Over 64 |
|---|---|---|---|---|---|---|
| Years from passing of law: | | | | | | |
| *Same year* | 0.511* | 0.208 | 0.803* | 0.224 | 1.000** | 0.086 |
| | (0.2669) | (0.2185) | (0.4251) | (0.2262) | (0.4705) | (0.6425) |
| *One* | 0.046 | 0.063 | 0.009 | −0.133 | 0.334 | −0.260 |
| | (0.3274) | (0.2658) | (0.5182) | (0.2740) | (0.5796) | (0.7657) |
| *Two* | 0.280 | 0.323 | 0.206 | 0.159 | 0.685 | −0.644 |
| | (0.3420) | (0.2830) | (0.5258) | (0.2734) | (0.6028) | (0.7991) |
| *Three* | 0.603* | 0.233 | 0.956* | 0.740** | 1.034* | −1.367* |
| | (0.3608) | (0.2857) | (0.5594) | (0.2911) | (0.6249) | (0.7877) |
| *Four* | 0.337 | 0.326 | 0.347 | 0.109 | 1.101 | −0.009 |
| | (0.5279) | (0.4679) | (0.7973) | (0.4966) | (0.8791) | (1.2987) |
| *Five* | 1.750*** | 1.055** | 2.372*** | 0.230 | 4.079*** | 0.997 |
| | (0.5465) | (0.4816) | (0.8128) | (0.5002) | (0.8944) | (1.3324) |
| *Six* | 1.110** | 1.780*** | 0.281 | 0.839* | 2.262*** | −0.319 |
| | (0.5273) | (0.4665) | (0.7934) | (0.4918) | (0.8681) | (1.3509) |
| *Seven and more* | 1.650*** | 1.307*** | 1.896*** | 0.803** | 3.159*** | 0.575 |
| | (0.3909) | (0.3049) | (0.5787) | (0.3291) | (0.6084) | (0.8026) |
| N | 1530 | 1530 | 1530 | 1530 | 1530 | 1530 |
| ant_p | 0.657 | 0.670 | 0.469 | 0.744 | 0.761 | 0.275 |
| post_p | 0.000 | 0.000 | 0.001 | 0.048 | 0.000 | 0.655 |
| ATT | 6.287 | 5.294 | 6.870 | 2.971 | 13.654 | −0.941 |
| ATT_p | 0.005 | 0.004 | 0.036 | 0.087 | 0.000 | 0.841 |

**Notes:**
(i) Heteroscedasticity and within-state serial correlation corrected standard errors in parentheses.
(ii) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
(iii) N Number of observations.
(iv) ant_p: p-value from test of joint significance of the pre-policy effects.
(v) post_p: p-value from test of joint significance of the policy effects.
(vi) ATT: Total policy effect.
(vii) ATT_p: p-value from test that ATT=0.
(viii) Full regression results are given in Appendix B.

indicated by 'ant_p' in the tables) giving some confidence that the pre-policy trends were not significantly different in assisted suicide and other states.

Looking at unassisted suicides only (reported in Table 4), the total policy effect of assisted suicide laws remains statistically significant and positive, though much smaller than for total suicides. As with total suicides, the ATT implies a much bigger proportionate impact for women than for men. The ATT for under-35s is now weakly statistically significant whilst for the over-64 group it is negative, but not close to statistical significance. The relative differences in the estimated policy effect by age are potentially consistent with the Posner hypothesis that suicide substitution is more likely amongst older adults, albeit we do not find evidence of a significant net reduction in unassisted suicides even for over-64s.

#### *4.2.1. Robustness checks*

We report the various robustness checks of our baseline model in Tables 5-7. In the first two columns of Table 5 are the results from supplementing Eq. (4) for total and unassisted suicide rates by including a full set of state-specific trends.[11] The estimated total policy impact for total suicides is reduced from the main models, but still positive and statistically significant. For unassisted suicides, the estimate of the total policy impact is negative and no longer close to statistical significance.

We report the results of estimating Eq. (4) with the two placebo variables in the next two columns of Table 5. Reassuringly, the estimated total policy effect of assisted suicide laws on both injury deaths and births is statistically insignificant.

In the final columns of Table 5, we report the estimates using wild bootstrapped standard errors. These results are very similar to the baseline models with overall policy effect positive and statistically significant for both total and unassisted suicide rates. However, the pre-trend effects are jointly statistically significant, casting doubt on the parallel trends assumption. That said, the magnitude of the implied pre-policy effects is very much smaller than the post-policy effects suggesting that any potential bias is unlikely to be enough to affect our conclusions substantially.

The results from using the staggered difference-in-differences estimator are presented in Tables 6 and 7 (for total and unassisted suicides respectively). There continues to be evidence of a positive impact of assisted suicide laws on total suicides. Indeed, for overall total suicides, the estimated effect is positive and statistically significant at 5% level or better for all but one of the policy years under consideration. For unassisted suicides, the estimated effects are positive for most years but statistically significant in only a few cases.

[11] We include here the models for overall rates. Further specifications such as the gender and age breakdown are provided in Appendix B.

**Table 5**
Robustness checks: estimates from models with:.

| | state-specific trends | | placebos | | wildbootstrapped standard errors | |
|---|---|---|---|---|---|---|
| | Totalsuicide rate | Unassistedsuicide rate | Injuryrate | Birthrate | Totalsuicide rate | Unassistedsuicide rate |
| Years from passing of law: | | | | | | |
| *Same year* | 0.508* | 0.259 | 0.512 | 0.149 | 0.851*** | 0.546*** |
| | (0.2772) | (0.2537) | (0.4038) | (0.1088) | (0.1570) | (0.1356) |
| *One* | 0.502 | −0.310 | 0.578 | 0.178 | 1.018*** | 0.083 |
| | (0.3264) | (0.2920) | (0.5163) | (0.1601) | (0.2669) | (0.2344) |
| *Two* | 0.704** | −0.113 | −0.121 | 0.094 | 1.258*** | 0.300 |
| | (0.3417) | (0.3035) | (0.5252) | (0.1806) | (0.4454) | (0.2955) |
| *Three* | 1.071*** | 0.116 | −0.617 | 0.210 | 1.768*** | 0.638* |
| | (0.3462) | (0.3078) | (0.5594) | (0.1940) | (0.5237) | (0.3443) |
| *Four* | 0.333 | −0.697 | −0.646 | 0.141 | 2.085*** | 0.522 |
| | (0.5109) | (0.4530) | (0.8421) | (0.1870) | (0.5953) | (0.4203) |
| *Five* | 1.637*** | 0.507 | 0.406 | 0.164 | 3.563*** | 1.797*** |
| | (0.5216) | (0.4697) | (0.8799) | (0.1868) | (0.3356) | (0.2644) |
| *Six* | 0.790 | −0.423 | −0.231 | 0.096 | 2.948*** | 1.066*** |
| | (0.5167) | (0.4759) | (0.8826) | (0.1927) | (0.7270) | (0.3719) |
| *Seven and more* | 0.835* | −0.497 | −1.767** | 0.130 | 4.259*** | 1.643*** |
| | (0.4271) | (0.4184) | (0.8701) | (0.2049) | (0.4487) | (0.3486) |
| N | 1520 | 1530 | 1530 | 1275 | 1520 | 1530 |
| ant_p | 0.187 | 0.448 | 0.146 | 0.778 | 0.000 | 0.001 |
| post_p | 0.031 | 0.083 | 0.036 | 0.835 | 0.000 | 0.000 |
| ATT | 6.380 | −1.157 | −1.886 | 1.162 | 17.749 | 6.592 |
| ATT_p | 0.003 | 0.570 | 0.619 | 0.337 | 0.000 | 0.000 |

**Notes:**
(i) Heteroskedasticity and within-state serial correlation corrected standard errors in parentheses.
(ii) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
(iii) N: Number of observations.
(iv) ant_p: p-value from test of joint significance of the pre-policy effects.
(v) post_p: p-value from test of joint significance of the policy effects.
(vi) ATT: Total policy effect.
(vii) ATT_p: p-value from test that ATT=0.
(viii) Full regression results are given in Appendix B.

Taken together, the event study estimates are consistent in suggesting that assisted suicide laws are associated with an increase in total suicide rates. There is also some evidence that legalisation of assisted suicide is associated with a smaller increase in unassisted suicide rates, but this finding is not so robust as for total suicides. The results provide no evidence of assisted suicide laws inducing any significant Posner-type substitution effect sufficient to reduce even unassisted, let alone total suicides.

### *4.3. Synthetic control method results*

The inclusion of pre-event variables increases our confidence that the observed associations are likely to be causal. However, we also look at the issue in another way by estimating the impact of assisted suicide laws using the synthetic control method (SCM).

We report the SCM effect of assisted suicide laws on total and unassisted suicide rates in Figs. 2 and 3 respectively. The tables accompanying the figures provide the point estimates of the effects for each year from the start of the assisted suicide law along with indicators of statistical significance based on matching both on levels and trends. Note the model fit statistics suggest matching by levels provides a better fit for total suicides, whilst matching by trends provides a slightly better fit for unassisted suicides. We report indicators using both approaches for comparison.

Consistent with the fixed effects results, the SCM estimated effects on total suicide rates (Fig. 2 and associated table) indicate that treated states had higher suicide rates overall after assisted suicide was legalised than 'synthetic control' states.

For unassisted suicides (see Fig. 3 and associated table), the estimated effects are in most (but not all) cases positive, but with a lower level of statistical significance and generally smaller in magnitude than for total suicide.[12]

In sum, the SCM estimates are generally consistent with the event study regression results. In neither case do we find any evidence of significant net substitution away from unassisted suicide when assisted suicide is legalised as predicted by Posner. Indeed, there is quite strong evidence that total suicides increase following implementation of assisted suicide laws and somewhat weaker evidence

[12] The demographic breakdowns are reported in figures in Appendix C. For total suicide rates, there is a clear, positive impact for females, ages 35-64 and over-64s. There is little observable impact of assisted suicide laws for males or for under-35s. For unassisted rates, there seems to be some effect for both males and females and for the 35-64 age group. As with the regression results, there is no clear effect for over-64s.

**Table 6**
Total suicide rates and assisted suicide laws: staggered difference-in-differences estimates.

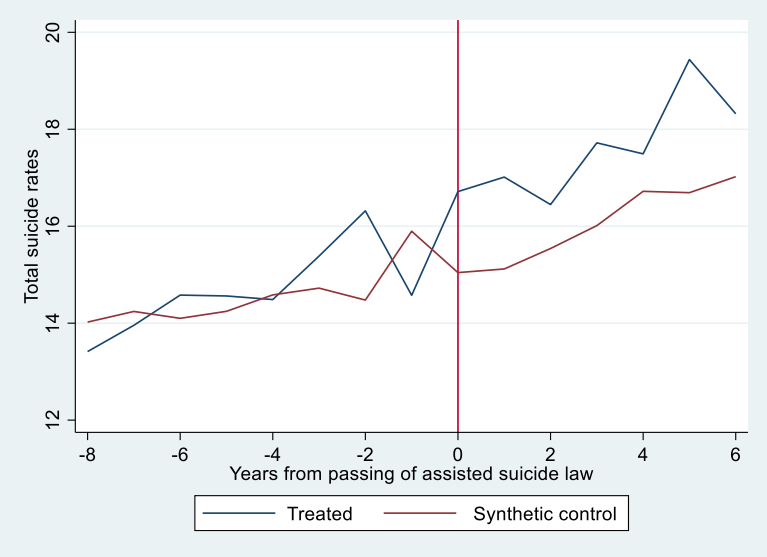
| | Years from passing of law | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 |
| Overall | 1.524*** | 1.732*** | 1.410*** | 2.465*** | 2.047*** | 3.826*** | 2.470*** | 1.944*** | 2.412*** | 2.411*** | 2.289* | 3.048*** |
| | (0.558) | (0.826) | (0.563) | (0.934) | (0.944) | (1.014) | (0.817) | (0.801) | (0.993) | (0.705) | (1.344) | (0.345) |
| Female | 0.759*** | 1.134*** | 2.099*** | 1.595* | 0.491 | 1.829 | 2.518*** | 1.124 | 2.222*** | 2.308*** | 1.902 | 2.382*** |
| | (0.229) | (0.554) | (0.549) | (0.847) | (1.648) | (1.106) | (0.903) | (0.860) | (0.993) | (1.030) | (1.209) | (0.286) |
| Male | 1.604 | 0.572* | 0.193 | 1.765*** | 1.836*** | 3.512*** | 1.025 | 2.705*** | 2.535*** | 2.456*** | 2.622* | 3.692*** |
| | (0.983) | (0.315) | (1.132) | (0.749) | (0.429) | (0.451) | (0.833) | (0.791) | (1.012) | (0.582) | (1.484) | (0.483) |
| Under 35s | 0.759 | −0.565* | −0.058 | 0.689 | 0.244 | 0.094 | −0.080 | 0.020 | 0.402 | 0.058 | −0.625 | 0.400 |
| | (0.502) | (0.331) | (0.536) | (0.476) | (0.614) | (0.610) | (0.479) | (0.521) | (0.331) | (0.372) | (0.419) | (0.320) |
| 35–64 years | 1.147*** | 1.285* | 1.235 | 1.259 | 0.120 | 3.413*** | 1.780*** | 1.496*** | 2.045* | 1.809*** | 2.546*** | 2.057*** |
| | (0.348) | (0.762) | (0.808) | (0.870) | (0.612) | (0.399) | (0.456) | (0.709) | (1.127) | (0.479) | (1.263) | (0.704) |
| Over 64s | 2.517*** | 4.875*** | 5.184*** | 5.984*** | 7.956*** | 9.702*** | 8.133*** | 9.423*** | 9.623*** | 11.102*** | 10.033* | 11.587*** |
| | (1.213) | (1.003) | (1.340) | (1.879) | (3.066) | (0.839) | (3.980) | (2.294) | (3.892) | (4.234) | (5.804) | (0.744) |

**Notes:**
(i) The 'never treated' states are used as the control group.
(ii) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
(iii) State-clustered standard errors in parenthesis.
.

**Table 7**
Unassisted suicide rates and assisted suicide laws: staggered difference-in-differences estimates.

| | Years from passing of law | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 |
| Overall | 1.045*** | 0.779 | 0.185 | 0.777 | 0.254 | 1.620*** | 0.389 | 0.997 | −0.188 | 0.386 | −0.244 | 0.227 |
| | (0.472) | (0.785) | (0.356) | (0.597) | (0.567) | (0.621) | (0.444) | (0.832) | (0.765) | (0.287) | (0.723) | (0.347) |
| Female | 1.044* | 0.892 | 0.843* | 1.162* | 0.996 | 1.106 | 0.865 | 0.267 | 0.296 | 1.741 | −0.445 | −0.251 |
| | (0.541) | (0.539) | (0.442) | (0.652) | (1.339) | (0.689) | (0.937) | (0.792) | (0.382) | (1.150) | (0.683) | (0.289) |
| Male | 1.040 | 0.661 | −0.517 | 0.377 | −0.560 | 2.084*** | −0.189 | 1.653* | −0.743 | −1.019 | −0.098 | 0.680 |
| | (0.722) | (1.292) | (0.758) | (0.819) | (0.503) | (1.034) | (0.635) | (0.892) | (1.272) | (1.074) | (0.794) | (0.487) |
| Under 35s | 0.489 | 0.350 | 0.103 | 1.126*** | −0.089 | −0.117 | 1.422 | 0.547 | −0.077 | 0.487 | −0.640 | 0.369 |
| | (0.445) | (0.974) | (0.504) | (0.491) | (1.024) | (0.959) | (0.925) | (0.617) | (0.503) | (0.529) | (0.423) | (0.322) |
| 35–64 years | 1.184*** | 1.048 | 0.118 | 0.497 | 1.025 | 2.967*** | −0.643 | 0.956 | 0.246 | −0.619 | 0.971 | 0.276 |
| | (0.578) | (0.651) | (0.558) | (0.552) | (1.048) | (0.635) | (0.638) | (0.831) | (0.840) | (0.875) | (0.833) | (0.708) |
| Over 64s | 2.174* | 1.536 | 0.757 | 0.494 | 0.289 | 3.904*** | 0.855 | 2.729 | −0.897 | 2.836* | −2.738 | −1.363* |
| | (1.192) | (1.879) | (0.923) | (1.639) | (0.800) | (1.534) | (2.793) | (2.181) | (1.634) | (1.569) | (3.242) | (0.735) |

**Notes:**
(i) The 'never treated' states are used as the control group.
(ii) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
(iii) State-clustered standard errors in parenthesis.




| | Matching on levels | | | Matching on trends | | |
|---|---|---|---|---|---|---|
| | Effect | p-value | Stand. p-value | Effect | p-value | Stand. p-value |
| Same year as assisted suicide law | 1.671 | 0.033 | 0.009 | 0.147 | 0.002 | 0.007 |
| One year after assisted suicide law | 1.895 | 0.007 | 0.004 | 0.173 | 0.000 | 0.004 |
| Two years after assisted suicide law | 0.907 | 0.248 | 0.406 | 0.094 | 0.063 | 0.235 |
| Three years after assisted suicide law | 1.707 | 0.066 | 0.025 | 0.144 | 0.008 | 0.034 |
| Four years after assisted suicide law | 0.773 | 0.355 | 0.447 | 0.099 | 0.088 | 0.171 |
| Five years after assisted suicide law | 2.746 | 0.005 | 0.005 | 0.229 | 0.000 | 0.002 |
| Six years after assisted suicide law | 1.297 | 0.208 | 0.251 | 0.116 | 0.065 | 0.189 |
| Model fit statistics | | | | | | |
| | PRE | POST | | PRE | POST | |
| | 0.243 | 0.086 | | 0.107 | 0.020 | |

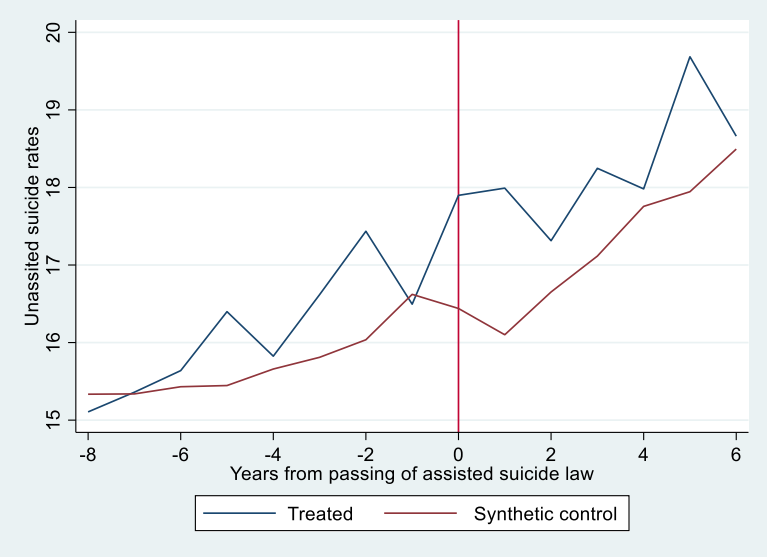
**Fig. 2.** Synthetic control method estimates: total suicide rate. **Notes:** (i) Policy states (and year of assisted suicide law) are: Oregon (1998); Washington (2009); Montana (2010) and Vermont (2013). (ii) The potential pool donor states consists of all other US states except for the following three states that passed an assisted suicide law but not included in the analysis because of lack of sufficient post-policy observations: Colorado (2016); California (2017) and District of Colombia (2017). (iii) The following pre-policy variables are used as predictors: log unemployment rate; log real disposable income per capita, log alcohol consumption per capital; log rate of death by accidental poisoning; proportion of suicides in which firearms used;% of months in year in which pot decriminalised; proportion of population aged under 20; proportion of population aged over-64; proportion of population rates classified as black; proportion of population ethnicity classified as non-white Hispanic; proportion of population classified as Asian or Pacific Islander; proportion of the population classified as American Indian or Alaska Native; unassisted suicide rate per 100,000 people in 1990 and 1995. Unless and otherwise stated, all other predictors are averaged over the pre-policy period. (iv) 30% of the pre-event period is used as a training period, with the remaining 70% being the validation period. (v) Matching on trends is implemented by scaling the dependant variable for both policy and control states to one at the year of the passing of the law. (vi) P-values are calculated as the proportion of placebo effects that are at least as large of the effect amongst the treated units. (vii) Standardised p-value are obtained by dividing all the effects by the corresponding pre-treatment match quality. This helps correct for the possibility that placebo effects may be quite large amongst untreated units with poor pre-assisted suicide law match quality, resulting in too conservative p-values. (viii) PRE: proportion of placebos that have a pre-assisted suicide law RMSPE at least as large as the average of the treated units. (ix) POST: proportion of placebos that have a post-assisted suicide law RMSPE at least as large as the average for the treated units. (x) Estimation is conducted using Stata's synth_runner package (Galiani and Quistorff, 2017).

that part of the overall increase is driven by a net rise in unassisted suicides.[13]

## 5. Conclusions

The legalisation of assisted suicide is a contentious topic in which debates are rightly driven primarily by considerations of rights,

[13] Like two-way fixed effects regressions, standard SCM methods are not, strictly speaking, intended for situations where we have multiple events occurring at different time periods. However, Cavallo et al. (2013) show that one can still make inference about the average effect estimated across state-specific comparative case studies. Xu (2017) suggests an alternative approach.




| | Matching on levels | | | Matching on trends | | |
|---|---|---|---|---|---|---|
| | Effect | p-value | Stand. p-value | Effect | p-value | Stand. p-value |
| Same year as law | 1.458 | 0.028 | 0.020 | 0.077 | 0.047 | 0.030 |
| One year after law | 1.890 | 0.004 | 0.001 | 0.083 | 0.039 | 0.009 |
| Two years after law | 0.661 | 0.335 | 0.105 | 0.013 | 0.776 | 0.717 |
| Three years after law | 1.132 | 0.111 | 0.020 | 0.008 | 0.861 | 0.630 |
| Four years after law | 0.226 | 0.744 | 0.181 | -0.037 | 0.485 | 0.610 |
| Five years after law | 1.740 | 0.042 | 0.006 | 0.071 | 0.177 | 0.057 |
| Six years after law | 0.166 | 0.840 | 0.393 | -0.041 | 0.459 | 0.533 |
| Model fit statistics | | | | | | |
| | PRE | POST | | PRE | POST | |
| | 0.195 | 0.219 | | 0.198 | 0.360 | |

**Fig. 3.** Synthetic control method estimates: unassisted suicide rates.

morals and ethics. However, understanding the practical effects of assisted suicide laws can provide an important context for political debates.

The Posner hypothesis that assisted suicide can induce substitution from unassisted suicides is a potentially helpful contribution to these debates. Posner's analysis led to some clear empirical predictions of the impact of assisted suicide laws: first that total suicides may not necessarily increase following the introduction of assisted suicide; second that unassisted suicides should decrease; third that those effects are more likely to be observed amongst older adults. Here, we have examined whether the empirical evidence from experience in the US so far provides any support for the Posner hypotheses.

Using both an event study approach on panel data from all US states and a synthetic control method approach, we find evidence that legalisation of assisted suicide is associated with a significant increase in total suicides. The increase is observed most strongly for the over-64s and for women. There is weaker evidence that assisted suicide is also associated with some increase in unassisted (unregulated) suicides, most particularly in the 35–64 age group and for women.

In sum, we do not find any support either for the suggestion that legalising assisted suicide might reduce total suicides or that it will reduce unassisted suicides. The finding that unassisted suicides do not decrease and may increase following the introduction of assisted suicide does not necessarily imply that there is no Posner-type substitution. It is possible that there is actually some substitution from unassisted to assisted suicide but that this is balanced out by an increase in unassisted suicide arising from, for example, a reduction in societal taboos associated with suicide. That the evidence for assisted suicide being associated with an increase in unassisted suicides is stronger for 35–64 year olds than for over-64s is consistent with this possibility.

The finding that assisted suicide laws have a relatively bigger effect on women is consistent with previous research by Canetto and McIntosh (2022) for the US and Steck et al. (2016) for Switzerland. This is particularly notable given that unassisted suicide rates tend to be lower amongst women than men. One interpretation of this result is that women seeking to die by suicide are, on average, less willing than men to utilise methods such as firearms which are commonly used in unassisted suicides. On this view, the higher take-up of assisted suicide may reflect women being empowered to take control over end-of-life decisions. Canetto and McIntosh (2022) posit the alternative perspective, i.e. that higher take-up may reflect disempowerment of those who are more vulnerable to social pressure to die by suicide, for example through feeling a burden to relatives or society and that women are overrepresented in such groups. The

confirmation in our research that suicide rates for women are relatively more affected by assisted suicide laws than for men makes the resolution of such debates even more critical.

A related issue is the important role of race and class in the assisted dying debates. Canetto (2019) argues that, in the US at least, the movement to legalise assisted suicide is dominated by "educated, White women" (p.39) whilst Tolle et al. (2004) report survey data suggesting that black people were "less likely to personally consider using assisted suicide to end their lives." (p.115). Although, data limitations mean it is difficult to explore this aspect in the context of a state-level econometric study, alternative research designs may be helpful in shedding more light on this issue.

The evidence in our paper helps to improve our understanding of the effect of assisted suicide laws in the US so far. We anticipate that our results will be of interest to policy makers in several European countries such as England, Scotland, Republic of Ireland and Portugal where assisted suicide laws are under active consideration.

We would like to note some caveats and limitations. First, it is well known that suicides can be undercounted on death certificates (Stone et al., 2021) though we are not aware of any evidence of specific issue with the CDC data. It is hard to be sure whether assisted suicide data will be more or less accurate. If the former, this could affect our results on total suicides, though it should not affect the estimates on unassisted suicides.

A further limitation of our work is that assisted suicide has still only been legalised in a limited number of states and many of these only have evidence for the first few years in which the laws have operated. This may be important if some of the effects of assisted suicide laws only become clear after several years of operation. Future research should carefully monitor the long run progress of unassisted and assisted suicide in states with assisted suicide laws. It is also unclear how generalisable estimates from US states will be to other jurisdictions with very different social and cultural contexts.

## Acknowledgements

we would like to thank David A Jones, participants at a Nottingham University Business School research seminar, the Editor (Rob Sauer) and two anonymous referees for many helpful comments and suggestions.

## Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.euroecorev.2022.104113.

## References

Abadie, A., Diamond, A., Hainmueller, J., 2010. Synthetic Control Methods for comparative case studies: estimating the effect of California's tobacco control program. J Am Stat Assoc 105 (490), 493–497.
Abadie, A., Diamond, A., Hainmueller, J., 2015. Comparative politics and the Synthetic Control Method. Am J Pol Sci 59 (2), 495–510.
Anderson, M.D., Rees, D.I., Sabia, J.J., 2015. Medical marijuana laws and suicides by gender and age. Am J Public Health 104 (12, Dec), 2369–2376.
Azrael, D., Cook, P.J., Miller, M., 2004. State and local prevalence of firearms ownership measurement, structure, and trends. J Quant Criminol 20, 43–62.
Brent, D.A., Bridge, J., 2003. Firearms availability and suicide: evidence, interventions, and future directions. American Behavioral Scientist 46 (9), 1192–1210.
Canetto, S.S., 2019. If physician-assisted suicide is the modern woman's last powerful choice, why are White women its leading advocates and main users? Professional Psychology: Research and Practice 50 (1), 39–50.
Canetto, S.S., McIntosh, J.L., 2022. A comparison of physician-assisted/Death-with-Dignity death and suicide patterns in older adult women and men. The American Journal of Geriatric Psychiatry 30 (2: Feb), 211–220.
Carter v. Canada (Attorney General) (2015), *SCR*, 1: 331) https://canlii.ca/t/gg5z4.
Cavallo, E., Galiani, S., Noy, I., Pantano, J., 2013. Catastrophic natural disasters and economic growth. Review of Economics and Statistics 95, 1549–1561.
CDC, 2007. Unintentional poisoning deaths – USA 1999-2004. MMWR Weekly 56 (05: Feb 9), 93–96.
Clarke, D., Tapia-Schythe, K., 2021. Implementing the panel event study. Stata Journal 21 (4), 853–884.
Dignitas (2014), *Submission 67 by Dignitas to Medical Services (Dying with Dignity) Exposure Draft Bill 2014*, 20 August, Parliament of the Commonwealth of Australia. https://www.aph.gov.au/DocumentStore.ashx?id=9d3ff5aa-2732-4402-ba87-3e7912088f1e&subId=299804.
Dugdale, L.S., Callahan, D., 2017. Assisted death and the public good. South. Med. J. 110 (9), 559–561.
Elder, R.W., Lawrence, B., Ferguson, A., Naimi, T.S., Brewer, R.D., Chattopadhyay, S.K., Toomey, T.L., Fielding, J.E., 2010. The effectiveness of tax policy interventions for reducing excessive alcohol consumption and related harms. American Journal of Preventative Medicine 38 (2, Feb), 217–229.
Freeman, D.G., 2007. Drunk driving legislation and traffic fatalities: new evidence on BAC 08 laws. Contemp Econ Policy 25 (3, July), 293–308.
Galiani, S., Quistorff, B., 2017. The synth_runner package: utilities to automate synthetic control estimation using synth. Stata J 17 (4), 834–849.
Gassman-Pines, A., Oltmans Ananat, E., Gibson-Davis, C.M., 2014. Effects of statewide job losses on adolescent suicide-related behaviors. Am J Public Health 104 (10, Oct), 1964–1970.
Goodman-Bacon, A., 2021. Difference-in-differences with variation in treatment timing. J Econom 225 (2), 254–277.
Goodman-Bacon, A., Goldring, T., Nichols, A., 2019. BACONDECOMP: stata module to perform a Bacon decomposition of difference-in-differences estimation. Statistical Software Components S458676. Boston College Department of Economics revised 04 Mar 2022.
Jones, D.A., 2022. Euthanasia, assisted suicide, and suicide rates in Europe. Journal of Ethics in Mental Health 1–35 open volume (Feb).
McKeown, R.E., Cuffe, S.P., Schulz, R.M., 2006. US suicide rates by age group, 1970–2002: an examination of recent trends. Am J Public Health 96 (10), 1744–1751.
Niederkrotenthaler, T., Fu, K.W., Yip, P.S., Fong, D.Y., Stack, S., Cheng, Q., et al., 2012. Changes in suicide rates following media reports on celebrity suicide: a meta-analysis. J Epidemiol Community Health 66 (11), 1037–1042.
Nordt, C., Warnke, I., Seifritz, E., Kawohl, W., 2015. Modelling suicide and unemployment: a longitudinal analysis covering 63 countries, 2000–11. Lancet Psychiatry 2 (1, Mar), 239–245.
Paton, D., Jones, D.A., 2015. How does legalization of physician assisted suicide affect rates of suicide? South. Med. J. 108 (10, Oct), 599–604.
Posner, R.A., 1995. Aging and Old Age. University of Chicago Press, Chicago.
Sisask, M., Värnik, A., 2012. Media roles in suicide prevention: a systematic review. Int J Environ Res Public Health 9, 123–138.
Stack, S., Kposowa, A.J., 2007. The association of suicide rates with individual-level suicide attitudes: a cross-national analysis. Soc Sci Q 89 (1, Mar), 39–59.

Steck, N., Egger, M., Zwahlen, M., 2016. Assisted and unassisted suicide in men and women: longitudinal study of the Swiss population. British Journal of Psychiatry 208 (5, May), 484–490.
Stone, D.M., Jones, C.M., Mack, K.A., 2021. Changes in Suicide Rates - United States, 2018-2019. Morbidity and Mortality Weekly Report 70 (8, Feb), 261–268.
Sun, L., Abraham, S., 2021. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. J Econom 225 (2), 175–199.
Tolle, S.W., Tilden, V.P., Drach, L.L., Fromme, E.K., Perrin, N.A., Hedberg, K., 2004. Characteristics and proportion of dying Oregonians who personally consider physician-assisted suicide. J Clin Ethics 15 (2, Summer), 111–122.
World Health Organization (WHO), 2008. Preventing Suicide: a Resource For Media Professionals. Department of Mental Health and Substance Use, WHO, Geneva.
Xu Y. (2017), 'Generalized synthetic control method: causal inference with interactive fixed effects models' *Political Analysis*, 25(1): 57–76..