International Review of Law & Economics 75 (2023) 106141

Contents lists available at ScienceDirect

International Review of Law & Economics

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

Estimating the effect of U.S. concealed carry laws on homicide: A replication and sensitivity analysis[☆]

Matthew V. Bondy[a,*], Samuel V. Cai[a], John J. Donohue[a,b]

[a] Stanford Law School, 559 Nathan Abbott Way, Stanford, 94305, CA, USA
[b] National Bureau of Economic Research, 1050 Massachusetts Avenue, Cambridge, 02138, MA, USA

ARTICLE INFO

Dataset link: https://works.bepress.com/john_donohue/

Keywords:
Guns
Crime
Right-to-carry
Homicide

ABSTRACT

In this article, we perform sensible robustness checks and estimation techniques that are broadly applicable to researchers studying the effects of concealed carry laws and apply them to recent work by Moody and Lott (2022). While Moody and Lott claim to have found that shall-issue and permitless-carry laws reduce homicide using data ending in 2014, our event-study analysis demonstrates that their model violates the conditional parallel-trends assumption. Additionally, applying methodology from Broderick et al. (2021), we show Moody and Lott's results are highly sensitive to the removal of a small number of observations. We examine and reject Moody and Lott's hypothesis that early- and late-adopting "shall-issue" states experienced different outcomes through sensitivity testing of their early–late threshold and applying the Goodman-Bacon (2021) decomposition. Following De Chaisemartin and d'Haultfoeuille (2020), we show Moody and Lott's results are biased by heterogeneous treatment effects. Overall, our results highlight the importance of conducting principled validity and sensitivity checks before introducing outlier estimates into the empirical literature.

1. Introduction

The last quarter of the 20th-century witnessed substantial expansion of the right-to-carry (RTC) concealed handguns in the United States. Most states that chose to deregulate gun carrying initially adopted "shall-issue" statutes, defined as laws mandating that authorities shall issue a concealed-carry permit to any individual who requests one with only narrow exceptions. In 1975, five states allowed the carrying of concealed handguns in public. An additional eight states adopted shall-issue laws in the 1980s and another 18 states did so in the 1990s. While Lott and Mustard (1997), using a staggered-adoption difference-in-differences design, found that shall-issue laws decreased crime, other scholars in the late 1990s came to different conclusions (see e.g. Dezhbakhsh and Rubin (1998), Ludwig (1998) and Black and Nagin (1998)). The recent literature has been able to take advantage of longer panels with greater treatment variation, which has enabled a strong body of evidence to emerge that the criminogenic effects of RTC dominate any deterrence effect, leading to an overall increase in violent crime. The Amicus Brief by Social Scientists and Public Health Researchers in Support of Respondents in *New York State Rifle & Pistol Association v. Bruen*, cited 14 studies in the last five years that found that RTC laws increased violent crime, and further confirming studies continue to be completed (Van Der Wal, 2022; Doucette et al., 2022b; Donohue et al., 2022; Doucette et al., 2022a). RAND has conducted an assessment of the entire literature on RTC laws and has now concluded, at its highest evidence rating, that RTC laws increase total and firearm homicides (RAND Corporation, 2023).

The past decade has seen a second wave of concealed-carry deregulation. Perhaps the most notable legal event has been the Supreme Court's June 2022 decision in *New York State Rifle Association v. Bruen* to strike down New York's statute requiring that individuals prove "proper cause" in order to obtain a concealed carry permit. In effect, the Court's decision has laid the foundation for a nationwide individual right to obtain a permit to carry a concealed weapon. But about half of the states have already deregulated further than shall-issue regimes by adopting "permitless carry" (also known as "constitutional carry")

☆ We are grateful to Carlisle Moody and John Lott, who have made the data and replication code for their study publicly available, and to Stanford Law School for research support. Additionally, we thank Arjun Ravi for outstanding research assistance.

* Corresponding author.
E-mail address: mvbondy@law.stanford.edu (M.V. Bondy).

[1] This statement is based on a search in September 2022 for ("permitless carry" OR "permitless concealed carry" OR "constitutional carry") on Google Scholar for peer-reviewed empirical research articles. Two additional empirical studies on state-level firearm legislation report coefficients associated with permitless carry laws, and both find null effects of permitless carry on categories of violent crime. Siegel et al. (2019) studies a suite of ten categories of firearm laws, of which permitless carry is one, and Adams (2022) focuses on stand-your-ground laws but includes permitless carry as a control.

https://doi.org/10.1016/j.irle.2023.106141
Received 23 November 2022; Received in revised form 4 May 2023; Accepted 10 May 2023
Available online 15 May 2023
0144-8188/

regimes which do not require individuals to have a permit to carry a concealed weapon. This latest deregulatory trend is so recent that the vast majority of the states moving to permitless carry did so since 2015. In light of this, it is not surprising that Moody and Lott (2022), a paper arguing that concealed carry laws reduce homicide, is the only peer-reviewed article that focuses on attempting to estimate the separate effect of permitless carry on homicide rates.[1] A survey of the current peer-reviewed literature indicates that most empirical studies of RTC regimes try to estimate the combined effect of both shall-issue and permitless carry on violent crime.

The recent work of Moody and Lott (2022) that attempts to influence the current public policy debate on permitless carry[2] follows the same pattern of the earlier work of Lott and Mustard (1997), in which their enthusiastic early results from a limited number of states adopting right-to-carry laws energized the adoption of these laws across the country. This wave of deregulation then allowed researchers to benefit from many more years of post-treatment data and more adopting states to amass a strong body of evidence that RTC laws tend to *elevate* violent crime, as noted above. The lessons from this previous experience suggest caution in drawing conclusions about the impact of permitless carry from the very limited data examined by Moody and Lott, which only extends through 2014. This history highlights one of the challenges in the realm of empirical work, where policymakers need to make choices today – when the possibilities for valid inference may be highly constrained – while the literature might not coalesce around a strong and reliable conclusion for years or even decades until adequate data are available and rigorously analyzed. It took that long to generate sufficient data for a consensus to emerge – as reflected in the RAND judgment – that shall-issue laws lead to higher crime rates. At this point, we do not have anywhere near that amount of data for permitless carry laws.

The issue of concealed-carry policy at this moment is a topic of high political salience at both the state and national level. Given the rapid adoption of permitless carry across many states and the uncertain status of concealed-carry policy in New York and the seven other states with similarly restrictive concealed carry laws in light of the *Bruen* decision, it is imperative that decision-makers have an accurate understanding of the empirical landscape. A significant challenge for the public, policymakers, and courts is that the large number of possible specification choices means that researchers can generate a wide range of estimated effects of concealed carry laws on crime, not all of which are equally valid.

In this article, we perform reasonable robustness and sensitivity analyses on Moody and Lott (2022) and try to set forth a principled set of objective tests of the validity of various modeling assumptions that they neglected but are advisable for all similar state panel data analyses. Using event-study regressions and a robustness-assessment procedure developed by Broderick et al. (2021), we illustrate that the Moody and Lott permitless-carry and shall-issue models do not identify a causal effect and depend on a handful of anomalous observations from low-population states. We argue that these findings reflect insufficient treatment variation leading to improperly calculated standard errors in the case of permitless carry and deeper model specification concerns. We also analyze Moody and Lott's hypothesis that the states that were earlier to adopt shall-issue laws experienced the greatest reductions in homicide, and show that the results hinge on an arbitrary cutoff. Furthermore, we draw on recent innovations in the econometrics literature that has shown heterogeneous treatment effects can produce biased average treatment effects using standard OLS two-way fixed effects models (Roth et al., 2022) in two ways. First, we use Goodman-Bacon (2021) decomposition to examine the average treatment effect of shall-issue laws on homicide conditional on year of adoption. While the size of the treatment effect does not appear to be wholly independent of the year of adoption, it does not follow the early-versus-late distinction that Moody and Lott propose. Second, we correct for the bias caused by heterogeneous treatment effects using the estimator by De Chaisemartin and d'Haultfoeuille (2020), and find that when one applies this estimator to Moody and Lott's data and covariates, the average effect of shall-issue and permitless-carry laws on homicide rates are both *positive*, although there is no evidence of significance.

The paper is organized as follows. Section 2 provides a brief overview of the empirical modeling decisions in Moody and Lott (2022) and presents our attempt at an exact replication of their results. We use this replication as a baseline for our subsequent analysis testing the robustness of Moody and Lott (2022)'s findings. In Section 3, we conduct an event-study analysis to examine the likelihood that Moody and Lott's shall-issue and permitless-carry models adhere to the conditional parallel-trends assumption, keeping with standard practice in the difference-in-differences literature. Given that the event-study plots undermine the parallel-trends assumption and thus any causal claim about the impact of permitless carry, we next test in Section 4 whether small variations in the data are driving the coefficients and standard errors in the static models. In Section 5, we test the sensitivity of Moody and Lott's heterogeneity analysis and how heterogeneous treatment effects may be biasing the results of their static two-way fixed effects models. In Section 6, we summarize and discuss the implications of our findings for future research.

2. Replicating Moody and Lott (2022)

Moody and Lott (2022) make three empirical claims: that (1) overall, shall-issue laws reduce homicide, (2) permitless-carry laws reduce homicide even more than shall-issue laws, and (3) states that were later to adopt shall-issue laws made it difficult to obtain a concealed carry license compared to earlier-adopting states, so the early adopters experienced more substantial reductions in homicide. None of these claims is validly supported by their work.

The main specifications for Moody and Lott's analyses are two-way fixed effects models including a lagged dependent variable as depicted in Eqs. (1) and (2):

$$Y_{it} = \beta_1 Permitless_{it} + \beta_2 Shall_{it} + \gamma' X_{it} + \alpha_t + \delta_i + \psi Y_{it-1} + \epsilon_{it} \quad (1)$$

$$Y_{it} = \beta_1 Permitless_{it} + \beta_2 Early_{it} + \beta_3 Late_{it} + \gamma' X_{it} + \alpha_t + \delta_i + \psi Y_{it-1} + \epsilon_{it} \quad (2)$$

where Y_{it} represents the log rate of a given outcome variable in state i at time t and X represents a constant and a set of covariates.[3]

While Eq. (1) includes only a single dummy variable for the presence of a shall-issue law, Eq. (2) includes separate indicator variables for state-years under a shall-issue law that went into effect on or before January 1, 1991 ("early") and for state-years under a shall-issue regime where the first *full* year in effect was 1992 or later ("late"). Moody and Lott's early and late dummies are switched to 0 once the permitless carry dummy is set to 1.

[2] John Lott has authored op-eds in Georgia and Pennsylvania newspapers in which he cited Moody and Lott (2022) as evidence that "murder rates fall even more when states move [from shall-issue laws] to Constitutional Carry laws" (Ward and Lott, 2021; Lott, 2022b). In 2022, he presented empirical research on permitless carry from the Crime Prevention Research Center (CPRC), of which he is the president and Carlisle Moody is the chief economist, at legislative hearings in Nebraska and Indiana (Lott, 2022a,d). CPRC research was also cited in a legislative hearing in Ohio (Lott, 2022c).

[3] The covariates are: lagged incarceration per capita; lagged police per capita; the unemployment rate; construction workers per capita; effective abortion rates; spirit, wine, and beer consumption per capita (separately); percent of population between ages 15 and 39; percent of population consisting of Black males between 15 and 39; and population density. The regressions with violent or property crime as their outcome variable include additional covariates as discussed in footnote 7.

This decision is potentially problematic because shall-issue laws have long-run effects on crime that are not "undone" by virtue of the passage of permitless carry. Moody and Lott's permitless carry variable does not represent the marginal effect of shifting from shall-issue to permitless carry, but rather the combined effect of the two policies. A better approach would be to assess the incremental effect of a state moving from shall-issue to permitless carry. However, since our goal is to probe the reliability of the Moody and Lott model, we do not change this coding. Both specifications include a dummy variable for permitless carry, which they refer to as "constitutional carry". Their regressions also include a lagged dependent variable and are not weighted by population.

Our first step was to attempt an exact replication using Moody and Lott's publicly available replication materials. The main findings from Moody and Lott come from four regressions with logged homicide rates as the dependent variable using panel data from the 50 states, which they report in Table 3 of their article.[4] The first model is estimated over the period from 1970 to 2014 and corresponds to Eq. (1), above. Moody and Lott's second model is also fitted on the full sample of years from 1970 to 2014, but uses Eq. (2) with separate early and late dummies. Moody and Lott's third and fourth models restrict the sample to 1991–2014 and 2000–2014, respectively, and therefore only examine the effects of shall-issue laws in late-adopting states.[5] In addition to the four models reported in their article, Moody and Lott also rely on two additional regressions found only in their publicly available replication materials; these models measure logged non-homicide violent crime (i.e. rape, robbery, and aggravated assault) and logged property crime excluding larceny (i.e. burglary and motor vehicle theft).

Our effort at replication identified a non-trivial error in their reported estimate for the impact of permitless carry on homicide. Moody and Lott report a coefficient of −15.85 for their "Constitutional Carry" variable in Column (1) of their Table 3, implying that permitless-carry laws reduce the homicide rate by an average of about 14.7 percent, substantially larger in magnitude than the coefficient we obtained from their model of −10.54 (corresponding to an effect size of −10.0 percent). When we probed further we found that our resulting point estimates and standard errors matched the results reported in their Stata log file output, so somehow the Moody and Lott estimate of −10.54 that is recorded in their Stata log file and what we obtained in our replication was erroneously published as −15.85. (We do not know from where their −15.85 number originated.) Our replication code is available online.[6]

Our subsequent efforts to replicate Moody and Lott's results made only very minor adjustments for two coding errors[7] and two factual errors[8] that do not meaningfully affect the results. Our results are reported in Table 1, which, for the remainer of our paper, we use as a baseline.[9] Columns (1) through (4) of our table correspond to Columns (1) through (4) of Moody and Lott's Table 3, and our columns (5) and (6) correspond to the two additional regressions found only in Moody and Lott's replication materials. These corrected results are virtually identical in terms of magnitude and significance to Moody and Lott's Table 3 with the exception of the discrepancy we noted above regarding the erroneous permitless-carry coefficient shown in their in Column (1).[10] We provide a table containing treatment dates in our Online Appendix.

Moody and Lott's interpretation of Model (1) is that both shall-issue laws and permitless-carry laws reduce homicide rates. They interpret Model (2) as evidence that "early and late adopters both had lower murder rates, with later adopters experiencing somewhat less reduction than early adopters". When limiting the sample to more recent years, the "late" coefficient becomes positive, which Moody and Lott view as "the relatively smaller reduction in crime due to the smaller increase in permits for these states, rather than any crime-increasing effect of RTC laws". Moody and Lott rely on Models (5) and (6) to argue that concealed carrying does not have deleterious effects on non-homicide violent crime or on property crime. For reasons we describe in the following sections, we do not impart a causal interpretation to any of the regressions shown in Table 1 due to our reservations regarding the validity and robustness of the Moody and Lott specifications.

3. Assessing the parallel-trends assumption

The key identification assumption underlying difference-in-differences research designs is the conditional parallel-trends assumption: conditional on observed covariates, the change in outcome for control units would be equivalent to the change in outcome for treatment units had the treatment units not been treated. While this assumption is inherently unobservable, economists follow many standard practices and guided by expert knowledge of the empirical setting being studied argue why the assumption is likely to hold in their identification strategy. Many empirical studies, including Moody and Lott (2022), pay insufficient attention to this assumption, providing no quantitative or qualitative justifications for why they expect the counterfactual would likely follow the conditional parallel-trends assumption. When we adhere to the current best practice on addressing this crucial assumption, the defects in the Moody and Lott model as a basis for causal inference become evident.

Indeed, the original Lott and Mustard (1997) specification generated such erroneous estimates of the impact of shall-issue laws in part because of its notable violation of the parallel-trends assumption. This was previously pointed out in Donohue et al. (2019), which presented an event-study analysis revealing a very strong pre-trend in the Lott–Mustard model estimated for firearm homicides over the period 1977–2014. This analysis clearly showed a downward trend in firearm homicide *prior to* the adoption of shall-issue laws—a trend that abruptly ended with the adoption of these laws.

Although the current Moody and Lott paper changes the data period to 1970–2014 and uses a new specification with different controls and a lagged dependent variable, they take no steps to validate their new

[4] Moody and Lott (2022) exclude Washington, DC.

[5] These models are effectively a modified version of Eq. (1), with a "late" variable in lieu of a "shall" variable, or alternatively, a modified version of Eq. (2) without an "early" variable.

[6] We opted to perform our analysis in R while they used Stata, which can lead to minor discrepancies.

[7] First, although Moody and Lott consider North Carolina to have been treated starting in 1996, an omission in their code causes that state to be grouped with the early-treated states. Second, they control for the dependent variable in their logged violent crime and logged property crime models by including the unlogged violent crime and unlogged property crime rates as regressors. Models (1) through (4) of Table 1 shown below correspond to Moody and Lott's models of homicide rates in their Table 3. Models (5) and (6) correspond to their models of the non-homicide violent crime and property crime rates, which are discussed in their paper as evidence that "RTC laws had no significant effect on other violent or property crime", although they report coefficients only in the replication materials.

[8] Moody and Lott make the decision to count the first *full* year of treatment as the adoption year rather than use partial-year values or round to the nearest year. However, they do not apply this rule consistently. The Illinois law went into effect on January 5, 2014 (Associated Press, 2014), so under their criteria the first year in which that state was listed as having shall-issue should have been 2015, but they list it as 2014. Wyoming passed its law on October 1, 1994 (Legislature of the State of Wyoming, 2022), but Moody and Lott count it as having shall-issue starting in 1996.

[9] Note that whenever we refer to a table or figure by number, we are referring to a table or figure shown in this article unless it is explicitly stated that we are referring to Moody and Lott (2022).

[10] When we follow the standard approach of weighting by population, the only coefficient in Table 1 that is significant at conventional levels is the permitless carry coefficient for the violent crime model in Column (5), with a value of 3.25 ($t = 2.76$).

Table 1
Replication of Moody and Lott regressions, Minor Coding and factual errors corrected.

	100 times log rate of...					
	Murder (1970–2014) (1)	Murder (1970–2014) (2)	Murder (1991–2014) (3)	Murder (2000–2014) (4)	Violent Crime (1970–2014) (5)	Property Crime (1970–2014) (6)
All RTC states	−5.68 t = −2.38**					
Early adopters		−6.95 t = −1.89*			−1.50 t = −1.07	−1.03 t = −1.48
Late adopters		−4.83 t = −1.76*	1.01 t = 0.31	2.91 t = 0.65	0.05 t = 0.06	0.40 t = 0.67
Constitutional carry	−10.55 t = −2.93***	−15.36 t = −3.30***	−3.15 t = −0.59	−0.64 t = −0.08	0.87 t = 0.44	−0.15 t = −0.12
Observations	2249	2249	1200	750	2249	2249
R^2	0.91	0.91	0.92	0.92	0.98	0.98
Adjusted R^2	0.91	0.91	0.91	0.91	0.98	0.98

Note: Standard errors clustered at the state level. Results not population-weighted.
*p < 0.1.
**p < 0.05.
***p < 0.01.

model. Our Fig. 1 presents an event-study analysis of the new Moody and Lott model, which should be a minimum first step to evaluate whether conditional trends are parallel in the pre-treatment period and whether the evolution of the treatment effect over time conforms to the researchers' hypotheses. Fig. 1(a) presents the year-by-year event-study plots underlying Moody and Lott's estimates of the effect of permitless carry on murder rates.[11] If there is a causal effect identified by the panel data model, one would expect to see a flat trend close to zero in the pre-treatment period and then either a stark break or else a noticeable change in trend after treatment. If the model is truly capturing the causal impact of permitless carry, one would expect it would be generating zero values in the years *prior* to adoption. It is evident from an examination of the permitless-carry event-study in Fig. 1(a) that this model cannot be expected to provide useful information about the impact of adopting a permitless carry regime on homicide. The pre-treatment values are often dramatically different from zero and exhibit highly unstable values over time. Indeed, the estimated value two years *prior to permitless-carry adoption* suggests a 35 percent drop in the murder rates—which is clearly not a causal effect. For the period five years through two years *prior* to adopting permitless carry, the Moody and Lott model suggests that permitless carry caused homicide to fall by 20.5 percent,[12] with the corresponding dummy variables jointly nonzero ($F = 12.84$, $df = 49$, $p < 0.001$). This particularly unpromising pre-treatment pattern is likely due to how few states contribute to each individual year-level estimate. As the Figure indicates, for most of the post-adoption period the presented estimates are based on only the single atypical, low-population, treated state of Alaska, and only 3 states go into the presented pre-treatment values.

The Moody and Lott analysis highlights the important lesson that unprobed and unsubstantiated panel data estimates can generate results that can be highly misleading. Specifically, while the Table 1 Moody and Lott estimates that underpin Fig. 1 event-study plots suggest that permitless-carry and shall-issue laws have a statistically significant and important impact that reduces the rate of murder, Fig. 1 plots give no sign of any trend break at the point of policy adoption as one would expect if these laws had any beneficial impact. While the shall-issue event-study plot in Fig. 1(b) is less erratic in its estimated year-over-year effects, it still does not suggest any notable trend break that occurs at the point of policy adoption.[13] Given that most year-level estimates post shall-issue adoption event-study in Fig. 1(b) are very close to zero with large standard errors, it is surprising that the static two-way fixed effects model reports a statistically significant, negative effect of shall-issue laws on homicide. The event plot suggests that this may be due to a single estimated value eight years following adoption, as this is the only year-level estimate that is statistically distinct from a null finding and is several times larger in absolute value than the estimates for the years immediately preceding and following (−16.45 versus −2.80 and −3.20, respectively). It is highly improbable that these values could be accurately capturing the causal impact of RTC laws. In the following section, we quantitatively demonstrate the substantial impact that minimal treatment variation in permitless carry and outliers in the shall-issue model have on the coefficients in the static two-way fixed effects model.

4. Assessing influential observations

The event-study analyses of Fig. 1 underscore the threat to causal inference posed by the limited number of permitless-carry observations in the Moody and Lott data, and by a single anomalous estimate eight years after RTC adoption. In this section, we further illuminate these problems in the Moody and Lott analysis by applying the Broderick et al. (2021) Approximate Maximum Influence Perturbation (AMIP) procedure, which estimates which observations have the greatest influence on a given result when dropped and then estimates what proportion of the sample if adversarially removed would reverse the significance or the sign of a regression coefficient. The Broderick et al. (2021) procedure illustrates the sensitivity of the findings to these weaknesses within the model. Additionally, for the permitless-carry results, we apply an alternative approach to computing standard errors that does not suffer from the downward bias present in traditional

[11] We regressed logged outcome variables on yearly dummies for each of the 10 years prior to RTC adoption to 10 years after shall-issue adoption as well as binned variables for 11 or more years before and 11 or more years after the policy change, omitting the year prior to adoption. We repeated this process for permitless-carry. That is, we computed the least squares fit for: $y_{it} = \sum_{k\in\{-11^+,-10,\ldots,10,11^+\}\setminus\{-1\}} \beta_k \mathbf{1}[t = AdoptYear_i + k] + \gamma X_{it} + \alpha_t + \delta_i + \epsilon_{it}$, where X represents covariates and α and δ represent time and unit fixed effects, respectively.

[12] This average effect is calculated by taking the mean of the coefficients $\hat{\beta}_{-5} = 6.53$, $\hat{\beta}_{-4} = -40.91$, $\hat{\beta}_{-3} = -14.98$, and $\hat{\beta}_{-2} = -42.34$, and then making the necessary adjustment to infer the percentage change.

[13] Additionally, we note that the deviations from parallel trends during the pre-treatment years are often even larger in magnitude than the treatment effect, which implies that a violation of the conditional parallel-trends assumption of a magnitude that was typical in the years before treatment could reverse the sign of their point estimate (Manski and Pepper, 2018). The magnitude of the violation in the conditional pretrends undermines the validity of the conditional parallel-trends assumption in Moody and Lott's models.

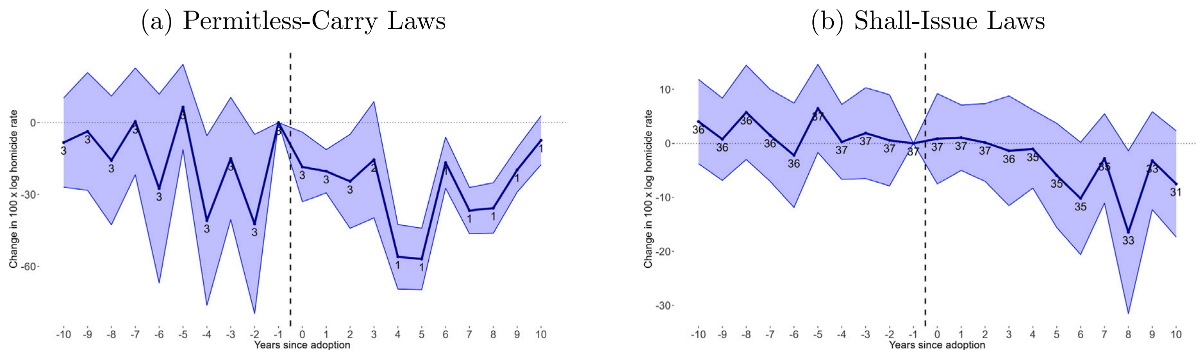

Fig. 1. Event Studies of Impact on Homicide Using Moody and Lott (2022) Model (1), 1970–2014. 95 percent confidence intervals and number of treated states contributing to each estimate are displayed.

clustered standard errors when there are a small number of treated units.

4.1. Permitless-carry

The unstable year-over-year estimates of the effect of permitless carry on homicide depicted in Fig. 1(a) are striking but not surprising given the small number of states and limited time duration in which this treatment is observed during the Moody–Lott data period. Moody and Lott (2022) use a panel of 2249 state-year observations in their regressions of homicide rates on permitless carry. Of these, only 18 observations have an active permitless carry regime: Alaska alone accounts for 11 of these, Arizona has four, and Wyoming has the last three. A DiD model estimated using a panel with such a small number of treated units yields clustered standard errors that are biased downwards, and therefore, a placebo test should be used in such a setting to determine significance in lieu of traditional p-values (Conley and Taber, 2011).[14]

We begin our analysis by following a placebo test procedure similar to Conley and Taber (2011) and Arkhangelsky et al. (2021). For each of 1000 iterations, using a panel that excludes the three permitless-carry states, we randomly selected three shall-issue states to "adopt" permitless carry – one in 2004, one in 2011, and one in 2012 (the adoption dates for Alaska, Arizona, and Wyoming, respectively) – and then estimated the treatment effect of permitless carry using Moody and Lott's Models (1) and (2). We obtained permitless-carry coefficients larger in magnitude than in Model (1) in 365 iterations and larger than Model (2) in 167 iterations, reflecting p-values of $p = 0.365$ and $p = 0.167$, rather than the $p < 0.01$ reported in Moody and Lott (2022).

To see the fragility of the Moody and Lott results, consider the point estimate for four years after permitless-carry adoption in Fig. 1(a). This point estimate suggests that homicide rates dropped more than 40 percent relative to the year prior to adoption. But this estimate is based on data from a single state, Alaska, which went, according to the UCR data that Moody and Lott use in their dataset, from having 39 murders in the year prior to permitless carry adoption and 43 murders in the third year after adoption down to 27 murders in the fourth year of permitless carry. (Note while Alaska has 41 murders in 2014, the last year of the Moody and Lott data, Alaska murders jumped to 59 in 2015 and 69 in 2019, which highlights the variability of homicide in small states.) Researchers who rely on such small changes in data from so few observations can generate estimates that are highly vulnerable to outliers that may cause substantial swings in their estimates.

Applying the AMIP methodology from Broderick et al. (2021), we find that the ostensible statistical significance of the permitless-carry coefficient in Moody and Lott (2022) depends on a small number of observations. The permitless carry coefficient in Model (1) loses significance if just three observations are removed, all of which were among the small group of treated observations (Arizona in 2014 and Wyoming in 2012 and 2014). The finding on permitless carry in Model (2) is not robust to the removal of seven observations, or 0.3 percent of the sample.

4.2. Shall-issue laws

Even though the severely limited data available for treatment evaluation is not a problem for shall-issue laws in Moody and Lott's model in the way that it is for permitless carry, the AMIP methodology reveals an even more extreme dependence on a small number of observations. The shall-issue coefficient in Moody and Lott's Model (1) loses statistical significance if just two observations are removed (which is less than 0.09 percent of the sample). The first observation is North Dakota in 1994, one of the observations contributing to the anomalously low point estimate for eight years post shall-issue adoption in the event-study analysis from Fig. 1(b). North Dakota experienced only one murder that year, whereas it had 11 murders the year prior, six the year after in 1995, and an average of about eight per year in the 1990s. The second is South Dakota in 1992, with four murders, compared to 12 the year prior, 24 the year after in 1993, and an average of about 12 per year in the 1990s. In logarithmic terms, these represent enormous year-over-year changes that are likely more noise than signal. Note that the states that mar the results of the Moody and Lott analysis – North Dakota, South Dakota, Alaska, and Wyoming – are four of the five smallest states, which highlights the dangers of Moody and Lott's puzzling decision to depart from the traditional approach in state panel data studies to weight the results by population.[15] The fact that Moody and Lott's findings are both contrary to the literature and only achieve statistical significance because of a few influential outlier observations from the smallest states suggest that they are chasing false signals rather than providing valid evidence of causality. Had Moody and Lott followed the customary approach in crime regressions of weighting by state population, used by Lott in his initial work on RTC laws, their results would not have been marred by this high level of instability.

[14] For more recent discussion of randomization-based inference and applied work building on Conley and Taber (2011), see for example Arkhangelsky et al. (2021), MacKinnon and Webb (2020), Moody and Marvell (2020) and Barati and Adams (2019).

[15] In fact, Lott noted that one should weight by population in his first article on the subject and did so consistently in his book, as did the National Research Council report on concealed carry laws (Lott and Mustard, 1997; Lott, 2010; National Research Council, 2005).

5. Assessing heterogeneous treatment effects

Moody and Lott (2022) introduce a novel argument that shall-issue laws are subject to heterogeneous treatment effects by year of adoption. The paper argues that states adopting shall-issue laws prior to the beginning of 1991 made it easier for citizens to apply for and obtain a permit by imposing fewer or no training requirements or fees in contrast to later-adopters who imposed more restrictions on concealed carrying, leading to fewer permits issued. Moody and Lott posit that this difference would lead to a stronger deterrent effect on crime in the early-adopting states, and that by ignoring this divide between "early" and "late" adopters, the literature has produced "biased measurement". Of course, if the important factor were some indication of "restrictiveness", one would use that as the basis for differentiating the alleged heterogeneous effects, rather than a crude date cutoff that would be unlikely to map onto the legal features defining restrictiveness. Additionally, the argument that early adopters of shall-issue laws experienced better outcomes than later adopters does not follow from the models presented in their paper. Moody and Lott calculated an F-test comparing the early and late coefficients from their Model (2) and found an F-statistic of 0.38 ($df = 49$, $p = 0.540$), thereby failing to reject the null that their coefficient of −6.78 for "early" is statistically any different from their coefficient of −4.20 on "late".[16]

The Moody–Lott demarcation of an early/late divide is further compromised by the fact that it appears to be wholly ad hoc rather than premised on some convincing theoretical rationale. They write in their introduction that early and late shall-issue laws "are not the same because late-adopting states (*post-1990*) tend to impose more restrictive regulations ..." (emphasis added). Then in the following paragraph they show a difference in the growth rates of concealed-carry permits between early- and late-adopting shall-issue states and all other states where the dividing line is 1999 rather than 1990, echoing an earlier working paper draft of their article where they used that year as the threshold defining late adopters. Ultimately in the empirical analysis of their published paper, they define late-adopting states as those where the first full year of shall-issue was 1992 or later.

5.1. Directly testing the role of training requirements and fees

We begin our heterogeneity analysis by conducting a direct test of whether safety training requirements and fees required to obtain a concealed-carry permit are associated with the estimated change in homicide rates due to shall-issue laws. Moody and Lott correctly observe that researchers should when possible "limit the [counterfactual] comparison to primarily may-issue states". To accomplish this, we calculate a separate 10-year average treatment effect for each of the 30 shall-issue states that have ten years of available data both pre- and post-adoption in Moody and Lott's sample, that is all states that adopted shall-issue laws between 1980 and 2004, inclusive. For each of these 30 states, we computed our corrected version of Moody and Lott's Model (1) restricting the sample to the treated state and all never-treated states from 10 years before to 10 years after shall-issue adoption. Taking the average shall-issue coefficient from these 30 regressions, the mean state experiences an *increase* in homicide of about 11 percent.

In Table 2, we report the average effect of RTC on 100 times the log homicide rate conditional on whether the state requires concealed carry permitholders to receive safety training, which we obtain for each state from Giffords Law Center (2023), and whether the state imposes a positive number of hours of safety training or a positive permitting fee, both from Moody and Lott's replication materials.

Table 2
Estimated increase in 100 times log homicide rate from RTC adoption based on permitting requirements.

Category	Yes	No
Safety requirement (Giffords Center)	7.27	23.91
Training requirement (Moody and Lott)	1.18	15.94
Fee requirement (Moody and Lott)	4.62	14.78

Table 2 indicates that every category of shall-issue-adopting states is associated with *increased* rates of homicide, and for the states with fewer requirements for obtaining permits, the homicide increases are all dramatically greater.[17] While we do not make any causal claim based on Table 2 since we are not vouching for the Moody–Lott model, we simply note that restricting the sample to clean comparisons of switcher states with adequate data pre- and post-adoption to never-adopting states but using Moody and Lott's set of controls leads to estimates that completely contradict all of their empirical claims about RTC laws. Taken together, these results suggest that if there is a relationship between the permissiveness of a concealed carry regime, within the class of shall-issue states, it points in the opposite direction that Moody and Lott (2022) argue.

5.2. Examining the relationship between treatment year and effect size

Next, we examine the early versus late hypothesis more directly. Given that Moody and Lott (2022) are performing a heterogeneity analysis on an ordered variable with many categories (year of shall-issue adoption), a simple sensitivity test is to determine how varying the cutoff year for "early" and "late" shall-issue adoption affects results. Moody and Lott (2022) use the year 1992 as the cutoff between early and late adopters of shall-issue.[18] We demonstrate that regardless of what cutoff year is chosen to demarcate early adopters from late adopters of RTC, there is no statistical difference between the early and late coefficients.[19] Furthermore, as shown in Fig. 2, had the threshold chosen been 1989, 1990, or 1991, the coefficient on early in the homicide regressions would have been statistically indistinguishable from zero at the $\alpha = 0.10$ level. Note that we group cutoff years together if no states adopted RTC during that range of years, so there would be no difference as to which states are designated as early versus late shall-issue adopters.

While Moody and Lott (2022)'s heterogeneity analysis is based on dividing the shall-issue states into two groups based on adoption before and after 1992, there are enough discrete adoption years to report the change in homicide by each year-of-adoption group. Analyzing and visualizing effect sizes by year-of-adoption cohort will provide a qualitative assessment of the claim that there are larger (more negative) treatment effects for early-adopting RTC states relative to late-adopting RTC states.

We examine heterogeneous treatment effects in two-way fixed effects model using the Goodman-Bacon (2021) decomposition. Using the "bacondecomp" package in R (Flack and Jee, 2020), we decompose Moody and Lott's Model (1) and examine only the component of the two-way fixed effects (TWFE) estimate that is derived from

[16] Additionally, we ran a version of Moody and Lott's Model (1) but adding a dummy variable for the presence of either a training requirement or fee to obtain a permit interacted with their shall-issue variable. The interaction coefficient was 1.38 and not significant ($t = 0.35$).

[17] Of the 30 shall-issue states used to compute the Table 2 estimates, 25 had a safety requirement (Giffords Center), 12 had a training requirement (Moody and Lott), and 14 imposed a fee (Moody and Lott).

[18] We define the cutoff as the earliest first full year of shall-issue that would qualify a state as a "late" adopter. Montana's shall-issue law went into effect in October 1991, so its first full year under shall-issue was 1992, and hence it is considered a "late" state under Moody and Lott's 1992 cutoff.

[19] For each cutoff year depicted in Fig. 2, an F-test fails to reject the null hypothesis that the early and late coefficients are the same, with the lowest corresponding p-value associated with the regression using a cutoff year in the range 1997–2002 ($F = 1.935$, $df = 49$, $p = 0.171$).

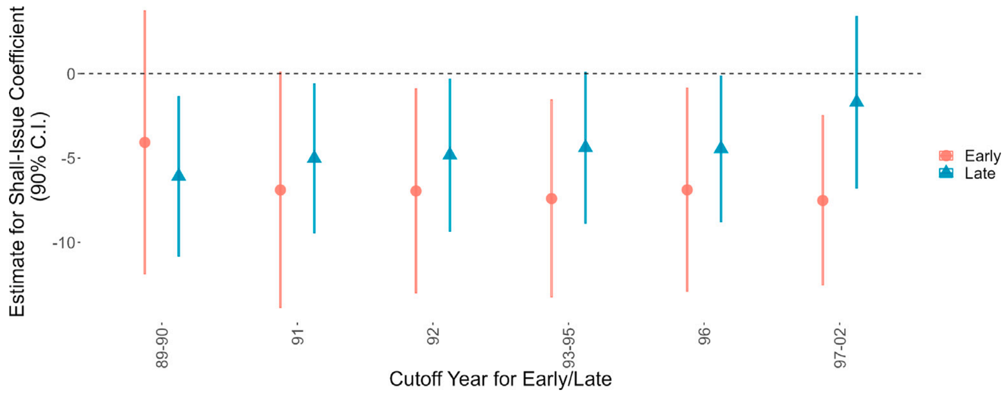

Fig. 2. Effect of Early/Late Threshold on "Early" and "Late" Coefficients, Moody and Lott Homicide Model. Cutoff year is the first year of adoption that would designate a state as "late". We combine cutoff years for which there would be no difference as to which states are designated as early-versus late-RTC adopters since there were no states that adopted RTC during that range of years.

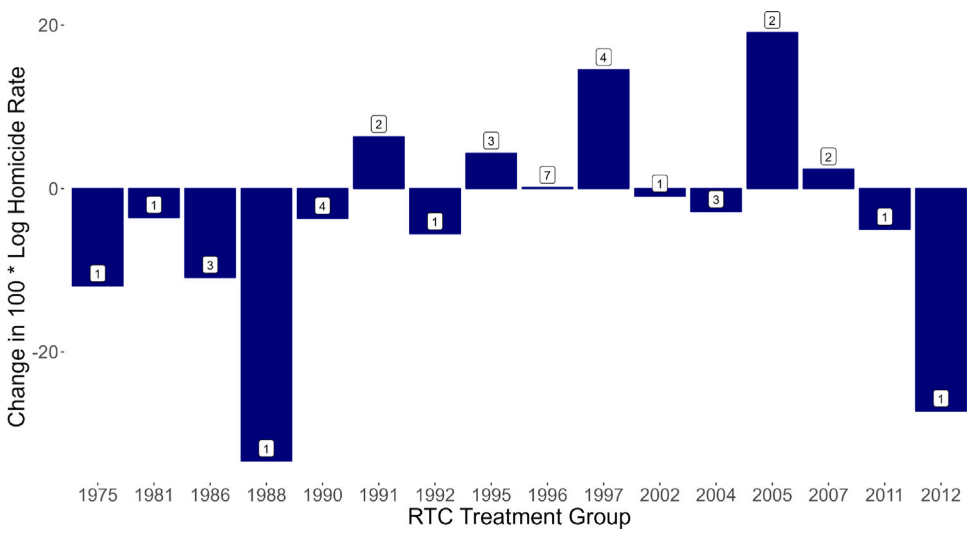

Fig. 3. Average treatment effect of Right-to-Carry on crime by year of passage, Goodman-Bacon (2021) Decomposition of Moody and Lott Homicide Model. Number of states in each RTC treatment group are displayed on the chart. Note that the two large negative estimates come from the single-state adoptions in Florida in 1988 and Wisconsin in 2012.

comparisons between shall-issue states and never-treated states. In Goodman-Bacon's framework, all states that passed shall-issue policies in the same year are considered part of one cohort, so not all cohorts are equally sized. The Goodman-Bacon (2021) decomposition allows us to visualize whether there are clear trends in effect heterogeneity by year-of-adoption. Fig. 3 shows how the average treatment effect of shall-issue regimes on homicide varies by each cohort, with the number of states in each cohort displayed. Even when using Moody and Lott's preferred models, there is no apparent relationship between the year of passage of shall-issue laws and the size or even direction of the effect on homicide rates.

5.3. Applying estimators to correct for bias from heterogeneous treatment effect

The Goodman-Bacon decomposition is a diagnostic tool researchers can use to determine whether their TWFE model is subject to potential bias from heterogeneous treatment effects. Although the Goodman-Bacon diagnostic is helpful, it does not allow the researcher to obtain an unbiased estimate of the average treatment effect along with standard errors. Several estimators have been proposed that provide these estimates (e.g. Borusyak et al., 2021; Cengiz et al., 2019; Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021). While there are some subtle differences between the way that these estimators work, user-friendly software packages have been provided to implement these estimators, and it has become the norm in recent years for researchers using a staggered-adoption DiD design to provide at least one of these estimators in addition to, or possibly in lieu of, TWFE OLS estimates. We note that we applied the De Chaisemartin and d'Haultfoeuille (2020) estimator to Moody and Lott's Model (1) and found a *positive* but not statistically significant estimate of the effect of shall-issue laws on 100 times the log homicide rate of 2.10 with a standard error of 8.18. For permitless carry, we find an even greater point estimate of the increase in homicide of 8.76.[20] This demonstrates that heterogeneous treatment effects across adopting states significantly bias Moody and Lott's results.

6. Conclusion

While there is urgent need for well-executed causal inference research on the impact of permitless carry regimes on violent crime, our analysis illustrates that Moody and Lott have no basis to argue

[20] This estimator allows for only one treatment variable at a time, so we run the model twice: with shall-issue as the treatment variable and permitless carry as the control, and then vice versa. Standard errors are calculated with state-level clustered bootstrapping. Due to limited treatment variation, R did not allow us to compute bootstrapped standard errors on the permitless-carry model; we ran 100 iterations for shall-issue.

that permitless carry or shall-issue laws reduce homicide. Nor do they have sufficient evidence to claim that "early" (or less-stringent) shall-issue regimes caused a reduction in homicide. Moody and Lott have inadvertently illustrated that there are dangers in concocting models without following the best practices for establishing the validity of their modeling assumptions, such as the critical conditional parallel-trends assumption, and conducting appropriate sensitivity tests, as evidenced by our analysis of slight adjustments to their "early/late" cutoff year and influential observations (Broderick et al., 2021).

So where does this analysis leave us in terms of our understanding of permitless carry laws? In the early days of the debate on RTC laws, some scholars initially claimed to have found evidence that shall-issue laws were associated with a reduction in crime. As more years of evidence accumulated, most researchers came to the opposite conclusion, as reflected in the RAND determination that shall-issue laws increase homicide, according to their highest evidentiary standard. We may be in a similar phase with respect to permitless carry, where there are simply too few years of observations to reach any strong conclusions one way or another.

Policymakers should, however, consider the large body of evidence showing that RTC laws increase violent crime before contemplating any further liberalizations in gun carrying outside the home. Recent work by Donohue et al. (2022) shows that this pernicious effect is in part because shall-issue laws result in more guns being stolen and a decline in the rates at which police are able to solve crimes and arrest the perpetrators. In the absence of more direct evidence, policymakers should consider whether permitless-carry laws are theoretically likely to exacerbate these issues, counteract these influences, or have no effect. Given the weight of the evidence and the fact that most empirical researchers choose to code permitless-carry and shall-issue regimes in the same way, we submit that ex ante, permitless-carry laws are most likely to operate in the same direction as shall-issue laws, although as stated previously we also believe that it is too early to perform rigorous empirical work to isolate the effect of permitless-carry laws separately from shall-issue laws, especially on a data set ending in 2014 as the Moody and Lott data does.

Our analysis offers several pieces of cautionary advice for the empirical literature on gun policy and for empirical researchers more broadly. The first lesson is that, while it may have been considered sufficient decades ago to merely present unadorned estimates from a static two-way fixed effects model as establishing a causal effect, this practice is no longer sufficient. At a bare minimum, researchers wishing to make a causal claim need to complement static models with event studies to ensure that prepassage trends appear flat and then exhibit a discontinuous jump or change in trend only after the treatment takes place. Given the emerging research examining the difference-in-differences literature showing that TWFE models are subject to bias in the presence of heterogeneous treatment effects, researchers will have to continue to update their estimation protocols as a new gold standard emerges. Second, researchers should examine their event-study plots closely and ask whether their model specification is so problematic that a trivial number of influential observations dictates their results, especially if these results are in contrast to the weight of the empirical and theoretical findings in the literature. Third, when only a small number of units are treated, it is important to correct standard errors accordingly, which can have a decisive effect on statistical significance as we showed was the case for the Moody and Lott estimates. Finally, heterogeneity analysis must be conducted systematically. Using standard methods, such as decile or quartile analysis for continuous variables, or testing different cutoff parameters for the threshold used to demarcate binary categories (such as large versus small or young versus old), are likely to help researchers ensure that their findings of heterogeneous treatment effects are robust, rather than artifacts of crude or arbitrary cutoffs.

Our results suggest some avenues for future research. As Moody and Lott (2022) contend, the literature has not sufficiently explored the question of whether RTC laws have heterogeneous treatment effects. Our analysis suggests that indeed, there may be, but at least at this point the evidence suggests, in contrast to the Moody–Lott assertions, that less-restrictive RTC laws appear to be considerably more damaging than more restrictive laws. One could also imagine that heterogeneous effects could emerge from differences beyond those of the various RTC laws but because of the nature of attributes of the states themselves, such as differences in demographics, population density, levels of inequality, rates of substance abuse, gun prevalence, and policing. Additional work will be needed to probe these possible differences.

CRediT authorship contribution statement

Matthew V. Bondy: Conceptualization, Methodology, Software, Writing – original draft. **Samuel V. Cai:** Conceptualization, Methodology, Software, Writing – original draft. **John J. Donohue:** Supervision, Methodology, Writing – review & editing.

Data availability

Replication code and data available at https://works.bepress.com/john_donohue/.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.irle.2023.106141.

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