Empir Econon
https://doi.org/10.1007/s00181-017-1415-2

The effects of gun control on crimes: a spatial interactive fixed effects approach

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Received: 19 April 2017 / Accepted: 19 November 2017

Abstract This paper examines the effect of right-to-carry laws on crimes. We relax the assumption that unobserved time effects have homogeneous impacts on states; therefore, states with right-to-carry laws may follow different time trends which might be stronger or weaker than those of other states including states with no right-to-carry laws. The heterogeneous time trends are modeled by a factor structure where time factors represent time-varying unobservables, and factor loadings account for their heterogeneous impacts across states. No assumption is imposed on the shape of the time trend. Crime statistics exhibit spatial dependence, and a state's adoption of right-to-carry law may have external effects on its neighboring states. Using a dynamic spatial panel model with interactive effects, we find positive spatial spillovers in crime rates. Depending on a crime category, an average 1% reduction in crime rates in neighboring states can decrease crime rates by 0.069–0.287%, with property crimes exhibiting higher degrees of spatial dependence than violent crimes. We find that although the passage of right-to-carry laws has no significant effects on the over-

This project was supported by the Fundamental Research Funds for the Central Universities (Project No. 17JNQN009). We would like to thank the editor Badi Baltagi, an anonymous referee, Paul Elhorst, and participants of the XI World Conference of the Spatial Econometrics Association at the Singapore Management University, and seminars at Jinan University for helpful comments. The usual disclaimers apply.

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Published online: 26 December 2017

all violent or property crime rates, they lead to short-term increases in robbery and medium-term decreases in murder rates. The results are robust to the number of factors, a different sample ending point, and some alternative spatial weights matrices and model specifications.

Keywords Spatial panel data · Cross-sectional and spatial dependence · Interactive fixed effects · Crime

1 Introduction

Ownership of guns and proposals for gun control have always been contentious in the USA. Since 1977, more and more states have passed right-to-carry laws that make it relatively easy for residents to obtain permits that allow them to legally carry concealed weapons in public. In theory, the impact of such a law on crime rates is ambiguous because under it, non-criminals can obtain weapons to more effectively defend themselves and potential criminals may thus be deterred, but the increased availability of weapons may facilitate criminal activity and escalate altercations into more serious forms of violence. The empirical evidence is also ambiguous. For example, using data on crimes from 1985 to 1991, Lott and Mustard (1997) find that the passage of right-to-carry laws decreases violent and property crimes, but Black and Nagin (1998) and Dezhbakhsh and Rubin (1998) cast doubt on their findings. Using gun magazine sales data as a proxy for gun ownership, Duggan (2001) finds that the passage of right-to-carry laws does not lead to higher gun ownership rates and that Lott and Mustard (1997)'s main results are not robust with clustered standard errors and minor changes to model specifications. In a survey, National Research Council (2005) reports that existing estimates are not robust to more recent years of data and variations in the set of explanatory variables. In this paper, we revisit this issue by using crime statistics up to 2012 and using factor models which was developed recently for panel data models to capture confounding factors. We also examine to what extent higher crime rates in a state can influence its neighbors.

Broadly speaking, policy evaluation is generally challenging using non-experimental data. A main problem is that the counterfactual of not receiving the policy intervention is not observable. When detailed individual-level data are not available, researchers often have to resort to data aggregated at regional levels and the treatment and control samples may differ in important ways. Difference in difference (DID) is a frequently used empirical method, and equal trend between the treatment and control units is a crucial assumption, under which the counterfactual can be computed using the pre-treatment outcomes of the treated and time effects estimated using the non-treated groups. Recently, several methods have been proposed to relax the equal trend assumption. Abadie and Gardeazabal (2003) propose that the counterfactual can be estimated using a weighted average of units in the control sample (the synthetic controls), and the weights are estimated such that the weighted average of outcomes of units in the control sample is close to that of treated units. The synthetic control method allows units in the control group and the treatment group to be affected differently by time effects, hence relaxing the equal trend assumption in DID. There is a close connection between linear factor models and the synthetic control method. Gobillon and Magnac (2016) show that the average treatment effect estimated using a regression panel with factors can be alternatively obtained using the synthetic control method, if the support of the control contains the support of the treated because weights in synthetic controls are nonnegative. Whether the support condition is satisfied depends on the data. As we will show in Sect. 4, the support condition may not hold here and the factor model approach may be preferable. Panel data models with an unobserved factor structure (or interactive fixed effects) in the error term have been studied by Pesaran (2006), Bai (2009), Ahn et al. (2013), and Moon and Weidner (2015), where time factors are unobserved time-varying effects and their impacts on states are captured by factor loadings which can be different among states so that states can be differently affected by the trend. Recently similar models have been applied to estimate the effect of divorce law reforms on divorce rates in the USA (Kim and Oka 2014), the effect of political and economic integration of Hong Kong with mainland China (Hsiao et al. 2012), and the effect of housing market regulation on house prices in China (Du and Zhang 2015), to name a few. Time factors can reflect policy interventions,[1] social, economic, and demographic changes that have systematic impacts on states. Treating factor components as fixed effects imposes few restrictions on their stochastic properties and allows the timing of the policy intervention to correlate with those factors.

Crime statistics exhibit large cross-sectional and intertemporal variations that are not sufficiently explained by exogenous costs and benefits of crimes, and social interactions have been proposed to explain the large covariance in crime statistics over time and across space (e.g., Sah 1991; Glaeser et al. 1996). Glaeser et al. (1996) have discussed in detail mechanisms through which agent-to-agent interactions can generate large differences in city crime rates among cities with similar characteristics. Generally, there is positive interactions where it is more likely for an individual to commit a crime if the individual's peer is a criminal, which can arise from information flows (a criminal peer provides information on the payoff, cost or techniques of committing crimes), tastes (more criminal peers lowers the stigma of crimes), costs (more criminal peers lowers the probability of arrests), etc. When social networks span two states, state-level crime rates can also exhibit positive spatial dependence. As our data are aggregated at the state and year level, general equilibrium effects and sorting may in theory also generate negative spatial dependence in crime rates. As a state becomes more dangerous, returns to lawful activities may decrease due to increased probability of theft, and individuals who intend to follow a career in crime may migrate from neighboring states and law-abiding individuals may migrate to neighboring states, and as a result, crime rates in neighboring states become lower. Therefore, theoretically the sign of the interaction effect in the state-level annual crime data is ambiguous.

As a result from spatial dependences in crimes, policies that impact a crime rate in a state can have indirect effects on crime rates in its neighbors. In addition, in many regional policy evaluations, control units may also be affected by spillovers from the treated. A convenient way to model such spatial interactions is to use a $N \times N$ spatial

[1] Besides the adoption of right-to-carry laws, another important legislation is the Brady Bill which mandates background checks on firearm purchases and was implemented in 1994.

weights matrix W_N with zero diagonal, where N is the number of states. Analogous to a time lag in a time series model, the spatial lag of the $N \times 1$ outcome variable y_{Nt} is $W_N y_{Nt}$ and it measures how a crime rate in a state is influenced by crime rates in its neighbors. This paper applies the model in Shi and Lee (2017) to estimate the effect of right-to-carry laws on crimes and shows how a spatial structure can help disentangle direct and indirect effects of the policy while controlling for interactive fixed effects in unobservables. If spatial spillovers are ignored, counterfactuals may be contaminated. The total effect of a policy consists of its direct effect on the treated and the indirect effect through spillovers. Understanding how spillovers work may also be of interest.

It should be noted that there are other ways to model spatial dependence. One alternative is a spatial error model where disturbances of a regression model can have spatial spillovers in the form of spatial autoregressive or moving average type (e.g., Kelejian and Prucha 1999; Kapoor et al. 2007; Baltagi and Pirotte 2011; Su and Yang 2015). The errors can have both spatial dependence and common factors (Pesaran and Tosetti 2011; Bhattacharjee and Holly 2011). This paper considers spatial interaction in the outcome variable in the form of its spatial lag. The passage of right-to-carry laws in a state has external effects on other states because crime rates in neighboring states are affected by changes to the crime rate in the own state. This specification can be interpreted as describing a spatial equilibrium (LeSage 2014) where changes to a crime rate in a state set in motion a chain of adjustments through its immediate neighbors, neighbors of its neighbors, etc.

The rest of the paper is organized as follows. Section 2 describes the data on right-to-carry laws and crimes. Section 3 discusses the empirical specification, and Sect. 4 presents the findings. The last section concludes.

2 Data and descriptive statistics

In this paper, we examine the effect of right-to-carry law legislation on crimes. In general, there are four types of policies regarding the carry of concealed weapons (often handguns) in public. In some states, a permit may not be required to carry a concealed weapon ("unrestricted"). Some states require permits to carry a concealed weapon, but will issue the permit if the applicant meets certain requirements specified in law which can include having no significant criminal record or history of mental illness ("shall-issue"). A state is classified as a "may-issue" state if the local authority has discretion over whether the permit will be issued, and oftentimes the applicant is required to demonstrate a need to carry a concealed weapon. A crucial difference between shall-issue and may-issue policies is that under the former the local authority generally has no discretion over the issuance of the permit. A state has a "no-issue" policy if it is not legal for private citizens to carry concealed weapons in public with very limited exceptions. The distinction between may-issue and no-issue policies may be few in practice as authorities with discretionary power may rarely issue permits. We consider a state to have right-to-carry law if it maintains an unrestricted or shall-issue policy.

Ayres and Donohue (2003) provide years of passage of right-to-carry laws till 1996. Between 1997 and 2012, 10 more states passed right-to-carry laws and their years of passage are obtained from various sources.[2] Figure 1 shows the evolution of right-to-carry legislation. Most states were may-issue or no-issue states prior to 1977. Since then, many states have transitioned to less restrictive laws toward concealed carry and adopted right-to-carry laws. The timing of right-to-carry legislation differs across states.

The data on state-level crimes were obtained from the FBI's Uniform Crime Reporting (UCR) Program which has a balanced panel structure. We use data on all states in the continental USA from 1977 to 2012. The UCR program collects data on violent and property crimes. Violent crime includes murder and non-negligent manslaughter, rape,[3] robbery, and aggravated assault. Property crime includes burglary, larceny, and motor vehicle theft. Let z_{it} be the number of reported crimes per 100,000 state residents in certain category in state i and year t. The natural log of a crime rate is often used in the literature, $y_{it} = \log(z_{it})$. Figures 2 and 3 plot the average violent and property crime rates and their components. Clear time trends emerge. Average violent crimes spiked in the early 1990s and declined thereafter while property crimes generally followed a downward trend. The average proportions of crime within violent crimes were generally stable with a small noticeable increase in the share of assault and a decrease in the share of robbery over the sample period. Within property crimes, the share of larceny increased till the late 1990s, while the share of burglary decreased till the late 1990s and increased slightly thereafter, and the share of auto theft was slightly higher between the late 1980s and late 2000s.

Many studies on the effect of right-to-carry laws on crimes work with the level of crime rates (National Research Council 2005). Observing the persistent patterns in crime rates, we test the hypothesis of unit root. The Breitung (2001) and Im–Pesaran–Shin (2003) unit root tests that we use assume large N and T and should be appropriate for the size of our sample. The null hypothesis is that unit roots are present. Table 1 reports the p value of the tests on levels of log crime rates. With the exception of murder and rape, in general, the null hypothesis of unit root cannot be rejected at conventional significance levels. Therefore, in this paper we will work with first differenced log crime rates.

Variables that are often used to control for unobservables that affect both right-to-carry legislation and crimes include per-capita income, population density, percentages of state residents in various race, age and gender categories, size of the police force. As noted in National Research Council (2005), the current set of explanatory variables may not be a proper set of control variables as different studies with minor changes to the data or a model specification have reported sometimes conflicting estimates of effects of right-to-carry laws on crime. Belloni et al. (2016) estimate the effect of gun prevalence on homicide and using Cluster–Lasso method, and from a set of 978 variables the selected variables are persons 5 years and older who live in the same house for the last 5 years, votes cast the third party candidate in the election for

[2] Colorado 2003, Iowa 2011, Kansas 2006, Michigan 2001, Minnesota 2003, Missouri 2003, Nebraska 2006, New Mexico 2003, Ohio 2004 and Wisconsin 2011. See http://www.gun-nuttery.com/rtc.php and the cited sources therein.

[3] The definition of rape was revised in 2011. In this paper, statistics on rape are under the old definition ("forcible rape").

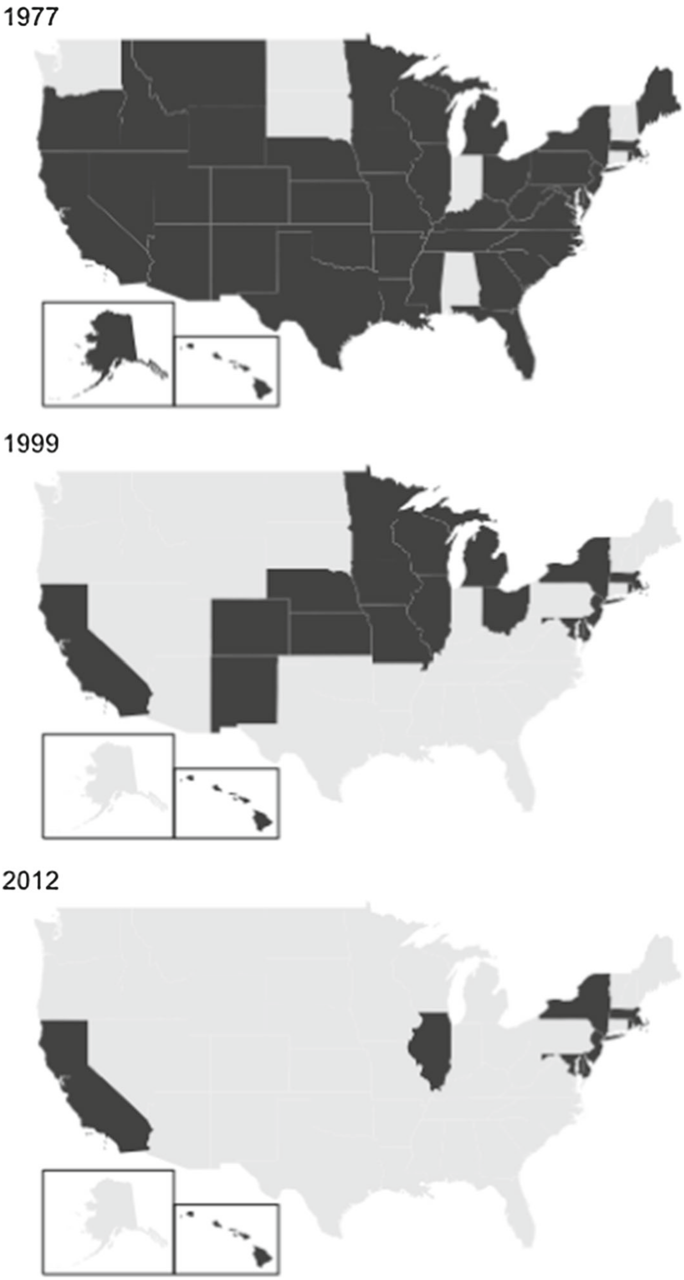

Fig. 1 Adoption of right-to-carry laws. Light color indicates that a state maintains an unrestricted or "shall-issue" policy toward concealed carry

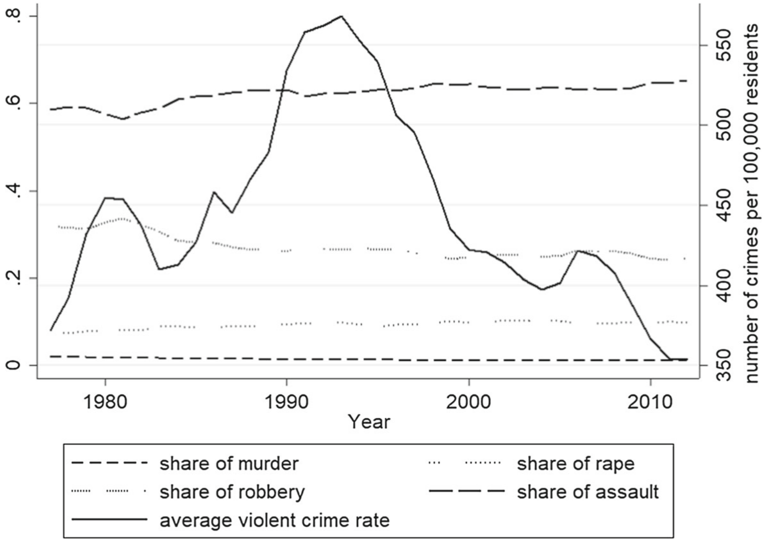

Fig. 2 Average violent crime rates and compositions, 1977–2012

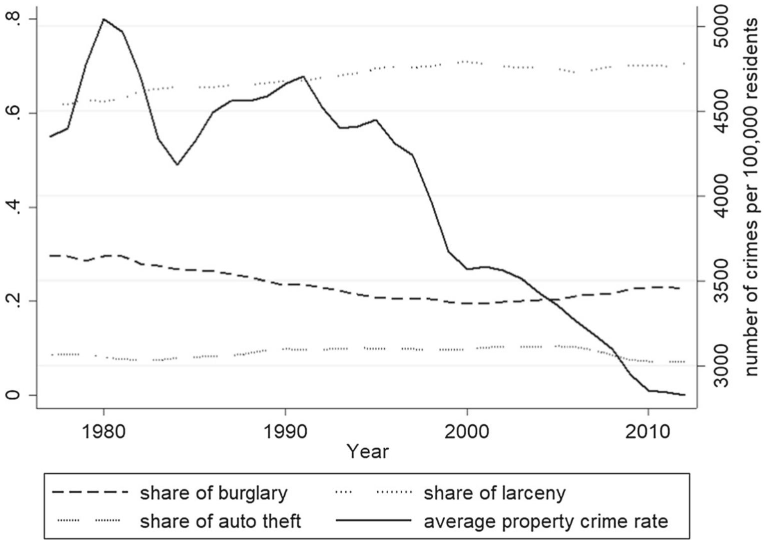

Fig. 3 Average property crime rates and compositions, 1977–2012

president, and valuation of new housing interacting with time trend. Note that those selected variables are outside the set of variables often used in the previous literature. In light of the uncertainty in which control variables are appropriate, in this paper we follow the approach in Kim and Oka (2014) where confounding factors are primarily controlled by interactive fixed effects.

Table 1 Panel unit root tests

Crime category	Breitung		Im–Pesaran–Shin	
	1977–2012	1977–1999	1977–2012	1977–1999
Violent crime	0.9123	0.9701	0.2474	0.1476
Murder and non-negligent manslaughter	0.0000	0.0000	0.0000	0.0000
Rape	0.0010	0.0058	0.0000	0.0054
Robbery	0.5688	0.9616	0.0085	0.0267
Aggravated assault	0.8537	0.7526	0.7726	0.0014
Property crime	0.7075	0.2840	0.1058	0.0001
Burglary	0.2777	0.0340	0.0192	0.0001
Larceny	0.6006	0.0874	0.0763	0.0000
Motor vehicle theft	0.9961	1.0000	0.2794	0.9815

p values are reported. The null hypothesis is that unit roots are present. Both tests subtract cross-sectional means and allow for a linear time trend. Lags of the dependent variable are added with its number selected by AIC to control for possible serial correlation in the Im–Pesaran–Shin test

3 Empirical framework

Define $\Delta y_{it} = y_{it} - y_{it-1}$ where $y_{is} = \log(z_{is})$ for $s = 1, \ldots, T$, which is approximately the growth rate of z_{it} because $\Delta y_{it} = \log\left(\frac{z_{it}}{z_{it-1}}\right) \approx \frac{z_{it}}{z_{it-1}} - 1$. We assume that the data generating process for Δy_{it} satisfies the following functional form,

$$\Delta y_{it} = \lambda \sum_{j=1}^{N} W_{N,ij} \Delta y_{jt} + \phi \Delta y_{it-1} + \rho \sum_{j=1}^{N} W_{N,ij} \Delta y_{jt-1} + t_{it} + u_{it}. \qquad (1)$$

t_{it} measures the effect of right-to-carry laws,

$$t_{it} = I_{t-2 \leq d_i \leq t-1} \tau_1 + I_{t-4 \leq d_i \leq t-3} \tau_2 + I_{t-6 \leq d_i \leq t-5} \tau_3 + I_{t-8 \leq d_i \leq t-7} \tau_4 + I_{d_i \leq t-9} \tau_5, \qquad (2)$$

where d_i is the year in which state i adopts an unrestricted or "shall-issue" policy toward concealed carry. The specification is flexible and allows the policy effect to vary over time. Similar treatment effect specification has been used in Wolfers (2006). The unobserved error u_{it} in (1) has the following structure,

$$u_{it} = c_i + \alpha_t + \gamma_i' f_t + e_{it},$$

where c_i and α_t are specific state and year fixed effects, $\gamma_i' f_t$ are interactive effects, and e_{it} is an i.i.d. idiosyncratic error. Similar specifications on t_{it} and u_{it} are used in Kim and Oka (2014) for a panel regression model.

Since the 1990s, crime rates in the USA have been declining, and more and more states adopt rights to carry laws. Crime rates might also have declined in the absence of right-to-carry legislation which might reflect structural changes in the society. The enactment of the right-to-carry laws might correlate with time trends in crime rates.

If legislators respond to heightened crime risk by relaxing gun control regulations, the adoption of such a policy may coincide with the peaking of violence, and crime rates will subside due to mean reversion even in the absence of changes in the law. The decline in crime rates may erroneously be attributed to the effect of the policy if these state-specific time trends are not properly accounted for. The standard strategy to control for unobserved heterogeneities is to use state fixed effects to control for time invariant heterogeneities for each state, and time fixed effects to control for unobserved time trend that has homogeneous impacts on all states. In addition to these terms, the interactive effects (or common factor components) specification $\gamma_i' f_t$ allows a $r \times 1$ time vector f_t to have heterogeneous impacts on states as the factor loading γ_i can be state specific. The interactive effects specification assumes that crime rates across states are affected by r common factors, which may generate cross-sectional and serial correlations. Statistically, the model assumes that after r principal components from the error matrix are removed, the remaining terms will be uncorrelated. The shape of the factors depends on data. If states with right-to-carry laws have different time-varying social economic characteristics such as different political preferences, a time fixed effect α_t will not be able to capture different time trends in crimes between states with and without right-to-carry laws. With interactive effects, a subset of states can be affected by their unique time trends. In the estimation, both the additive and interactive effects are treated as parameters so as to allow for correlations between the passage of right-to-carry laws and those effects. The interactive effects can include additive individual and time effects as special cases.[4] Nevertheless, directly controlling for possible additive effects along with interactive effects may improve estimation efficiency if additive effects are present.

Public safety is likely to have an external effect. To account for this, we construct an $N \times N$ matrix W_{1N} where its ijth element, $W_{1N,ij}$, is 1 if states i and j, $i \neq j$ are neighbors and 0 otherwise. The weight matrix is then row normalized, $W_{N,ij} = \frac{W_{1N,ij}}{\sum_{s=1}^{N} W_{1N,is}}$, so that $\sum_{j=1}^{N} W_{N,ij} \Delta y_{jt}$ measures the average effect of state i's neighbors on its security. In alternative specifications, we consider spatial weights matrices weighted by state population ($W_{2N,ij} = \frac{W_{N,ij} \times \text{stpop}_j}{\sum_{s=1}^{N} W_{N,is} \times \text{stpop}_s}$) where stpop_j is the average population of state j from 1977 to 2012. In (1), λ is the spatial multiplier. To capture dynamic effects, we include individual time lag $\Delta y_{i,t-1}$ and spatial time lag $\sum_{j=1}^{N} W_{N,ij} \Delta y_{j,t-1}$.

Stacking the observations in (1),

$$\mathscr{Y}_{Nt} = \lambda W_N \mathscr{Y}_{Nt} + \phi \mathscr{Y}_{N,t-1} + \rho W_N \mathscr{Y}_{N,t-1} + \mathscr{T}_{Nt} + \Gamma_N f_t + \mathbf{c}_N + \alpha_t \ell_N + \epsilon_{Nt}, \quad (3)$$

where $\mathscr{Y}_{Nt} = \left(\Delta y_{1t}, \cdots, \Delta y_{Nt}\right)'$, $\mathscr{T}_{Nt} = \left(t_{1t}, \cdots, t_{Nt}\right)'$, $\mathbf{c}_N = \left(c_1, \cdots, c_N\right)'$ and $\epsilon_{Nt} = \left(e_{1t}, \cdots, e_{Nt}\right)'$ are $N \times 1$, $\Gamma_N = \left(\gamma_1', \cdots, \gamma_N'\right)'$ is $N \times r$, and ℓ_N is an $N \times 1$ vector of ones.

Unlike in regression models, the interpretation of parameters in spatial interaction models is subtler. To see this, define $S_N(\lambda) = I_N - \lambda W_N$ and $D_N(\phi, \rho) = \phi I_N +$

[4] For example, $\tilde{\gamma}_i' \tilde{f}_t = \gamma_i + f_t$ if $\tilde{\gamma}_i = \left(\gamma_1 \;\; 1\right)'$ and $\tilde{f}_t = \left(1 \;\; f_t\right)'$.

ρW_N. Assuming that $S_N(\lambda)$ is invertible, the reduced form of (3) is

$$\begin{aligned}\mathscr{Y}_{Nt} &= S_N(\lambda)^{-1}\left(\mathscr{T}_{Nt} + \Gamma_N f_t + \mathbf{c}_N + \alpha_t \ell_N + \epsilon_{Nt}\right) + S_N(\lambda)^{-1} D_N(\phi,\rho)\,\mathscr{Y}_{N,t-1} \\ &= S_N(\lambda)^{-1}\left(\mathscr{T}_{Nt} + \Gamma_N f_t + \mathbf{c}_N + \alpha_t \ell_N + \epsilon_{Nt}\right) \qquad (4)\\ &\quad + S_N(\lambda)^{-1}\sum_{s=1}^{\infty}\left(D_N(\phi,\rho)\,S_N(\lambda)^{-1}\right)^s \\ &\quad \left(\mathscr{T}_{N,t-s} + \Gamma_N f_{t-s} + \mathbf{c}_N + \alpha_{t-s}\ell_N + \epsilon_{N,t-s}\right), \qquad (5)\end{aligned}$$

where (4) provides the contemporaneous effects of $\mathscr{T}_{Nt}$ on $\mathscr{Y}_{Nt}$, and (5) corresponds to its lagged effects on future $\mathscr{Y}_{Nt}$. The diagonal element of $\left[S_N(\lambda)^{-1}\right]_{ii}$ reflects the own effect (or direct effect) and feedback effect of t_{it} on Δy_{it}, and the off-diagonal element $\left[S_N(\lambda)^{-1}\right]_{is}$ corresponds to the indirect effect of Δy_{st} of state s on Δy_{it} of state i. The contemporaneous direct and indirect effects of right-to-carry laws also depend on λ and the spatial structure W_N, and in general are different from $\tau_1, \ldots, \tau_5$ due to spatial equilibrium adjustments. Right-to-carry laws can also have lagged effects, which also depend on ϕ and ρ.

Stacking (3) horizontally over time,

$$\begin{aligned}Y_{NT} = \lambda W_N Y_{NT} + \phi Y_{NT,-1} + \rho W_N Y_{NT,-1} + T_{NT} + \Gamma_N F_T' \\ + \mathbf{c}_N \ell_T' + \ell_N \mathbf{a}_T' + \epsilon_{NT},\end{aligned}$$

where $Y_{NT} = \left(\mathscr{Y}_{N1}, \cdots, \mathscr{Y}_{NT}\right)$, $T_{NT} = \left(\mathscr{T}_{N1}, \cdots, \mathscr{T}_{NT}\right)$, $F_T = \left(f_1, \cdots, f_T\right)'$, $\mathbf{a}_T = \left(\alpha_1, \cdots, \alpha_T\right)'$, and other terms are defined similarly. To estimate the model, we first eliminate time fixed effects $\mathbf{a}_T$. Define $M_N = I_N - \frac{1}{N}\ell_N \ell_N'$ and let $\left(G_{N,N-1}, \frac{1}{\sqrt{N}}\ell_N\right)$ be the orthornormal matrix of eigenvectors of M_N, where $\frac{1}{\sqrt{N}}\ell_N$ corresponds to the zero eigenvalue. As $G_{N,N-1}'\ell_N = 0$, $\mathbf{a}_T$ can be removed by pre-multiplying (3) by $G_{N,N-1}'$. The advantage of using $G_{N,N-1}'$ rather than M_N to remove fixed effects is that the errors after transformation are still uncorrelated. The estimation of panel data models with fixed effects by the transformation approach has been studied by, among others, Anderson and Hsiao (1981) and Arellano and Bover (1995), and for spatial panels, Lee and Yu (2010a, b). After the time fixed effects are eliminated, the model becomes

$$Y_{NT}^* = \lambda W_N^* Y_{NT}^* + \phi Y_{NT,-1}^* + \rho W_N^* Y_{NT,-1}^* + T_{NT}^* + \Gamma_N^* F_T{}' + \mathbf{c}_N^* \ell_T' + \epsilon_{NT}^*, \quad (6)$$

with $Y_{NT}^* = G_{N,N-1}' Y_{NT}$, $W_N^* = G_{N,N-1}' W_N G_{N,N-1}$, $Y_{NT,-1}^* = G_{N,N-1}' Y_{NT,-1}$, $T_{NT}^* = G_{N,N-1}' T_{NT}$, $\Gamma_N^* = G_{N,N-1}'\Gamma_N$, $\mathbf{c}_N^* = G_{N,N-1}'\mathbf{c}_N$ and $\epsilon_{NT}^* = G_{N,N-1}'\epsilon_{NT}$.

Individual fixed effects in (6) can be eliminated by transformations such as within transformation, first differencing and Helmert transformation, which can be seen as multiplying (6) from right by a proper transformation matrix L_T. The transformed model can be estimated by GMM (Arellano and Bond 1991; Lee and Yu 2014). The quasi-maximum likelihood (QML) approach may not be directly applicable for the transformed model because the transformed $Y_{NT,-1}^* L_T$ is not a time lag of $Y_{NT}^* L_T$

in the case of a within or Helmert transformation, and errors are serial correlated after first differencing. It is possible to eliminate interactive effects in (6). However, the transformation will depend on unknown parameters whose number increases with either N or T, see Ahn et al. (2013). For our estimation strategy, as T is not short, we do not eliminate individual effects but rather treat them as a time factor. This is so, because $c_i + \gamma_i' f_t = \left(c_i \ \gamma_i'\right)\begin{pmatrix}1\\ f_t\end{pmatrix}$, state fixed effects can be absorbed into interactive effects, such that $\Gamma_N^{\dagger} F_T^{\dagger\,\prime} = \Gamma_N^* F_T{}' + \mathbf{c}_N^* \ell_T'$. We then use the method in Shi and Lee (2017) to estimate the model parameters and interactive effects jointly by QML. The case with only individual fixed effects and no interactive effects is studied in Yu et al. (2008). Let $\theta = (\lambda, \phi, \rho, \tau_1, \ldots, \tau_5)$. The log likelihood function is

$$\begin{aligned}\mathcal{Q}_{NT}\left(\theta, \sigma^2, \Gamma_N^{\dagger}, F_T^{\dagger}\right) = {} & -\frac{1}{2}\log 2\pi - \frac{1}{2}\log\sigma^2 + \frac{1}{N-1}\log\left|I_N - \lambda W_N^*\right| \\ & - \frac{1}{2\sigma^2(N-1)T}\mathrm{tr} \\ & \left[\left(U_{NT}^*(\theta) - \Gamma_N^{\dagger} F_T^{\dagger\,\prime}\right)\left(U_{NT}^*(\theta) - \Gamma_N^{\dagger} F_T^{\dagger\,\prime}\right)'\right]\end{aligned}, \quad (7)$$

with $U_{NT}^*(\theta) = \left(I_N - \lambda W_N^*\right) Y_{NT}^* - \phi Y_{NT,-1}^* - \rho W_N^* Y_{NT,-1}^* - T_{NT}^*$. Maximizing (7) with respect to σ^2, $\Gamma_N^{\dagger}$ and $F_T^{\dagger}$, the concentrated log likelihood after dropping the constant term is

$$\begin{aligned}Q_{NT}(\theta) = {} & \frac{1}{N-1}\log\left|I_N - \lambda W_N^*\right| - \frac{1}{2}\log \\ & \left(\frac{1}{(N-1)T}\sum_{s=r+1}^{N-1}\mu_s\left(\mathrm{tr}\left(U_{NT}^*(\theta)\, U_{NT}^*(\theta)'\right)\right)\right),\end{aligned}$$

where $\mu_s(A)$ is the sth largest eigenvalue of matrix A. The QML estimator is obtained by maximizing $Q_{NT}(\theta)$.

For estimation, the number of factors needs to be determined. Several methods have been proposed in the literature, including Bai and Ng (2002)'s information criteria, Ahn and Horenstein (2013)'s eigenvalue ratio and growth ratio criteria, and Onatski (2010)'s criterion based on eigenvalue differences, to name a few. To estimate the number of factors, we first estimate the model using a large number of factors ($r_{max} = 7$) and assuming that it is not smaller than the true (unknown) number of factors, and obtain estimated coefficients $\breve{\lambda}$, $\breve{\phi}$, $\breve{\rho}$ and $\breve{\tau}_1, \ldots, \breve{\tau}_5$. Define the composite residual as $\breve{U}_{NT}^* = Y_{NT}^* - \breve{\lambda} W_N^* Y_{NT}^* - \breve{\phi} Y_{NT,-1}^* - \breve{\rho} W_N^* Y_{NT,-1}^* - \breve{T}_{NT}^*$. Bai and Ng (2002) propose the following 6 criteria.

$$\begin{aligned}PC_1(r) &= V(r) + r\cdot V(r_{max})\left(\frac{N+T-1}{(N-1)T}\right)\log\left(\frac{(N-1)T}{N+T-1}\right), \\ PC_2(r) &= V(r) + r\cdot V(r_{max})\left(\frac{N+T-1}{(N-1)T}\right)\log\left(c_{NT}\right),\end{aligned}$$

$$PC_3(r) = V(r) + r \cdot V(r_{max}) c_{NT}^{-1} \log(c_{NT}),$$
$$IC_1(r) = \log V(r) + r\left(\frac{N+T-1}{(N-1)T}\right)\log\left(\frac{(N-1)T}{N+T-1}\right),$$
$$IC_2(r) = \log V(r) + r\left(\frac{N+T-1}{(N-1)T}\right)\log(c_{NT}),$$
$$IC_3(r) = \log V(r) + r c_{NT}^{-1} \log(c_{NT}), \tag{8}$$

where $V(r) = \frac{1}{(N-1)T}\sum_{s=r+1}^{N-1}\mu_s\left(\mathrm{tr}\left(\breve{U}_{NT}^{*}\breve{U}_{NT}^{*}{}'\right)\right)$ and $c_{NT} = \min(N-1, T)$. With each criterion, the number of factors is selected by minimizing that criterion. For example, with IC_2 criterion, $\hat{r}_{IC_2} = \arg\min_{0\le r\le r_{\max}} IC_2(r)$. Bai and Ng (2002) show that the number of factors can be consistently estimated for a pure factor model using the criteria above, and Shi and Lee (2017) prove the consistency for the factor model with spatial interaction effects. For Ahn and Horenstein (2013)'s method, define the eigenvalue ratio statistic $ER(r) = \mu_r\left(\mathrm{tr}\left(\breve{U}_{NT}^{*}\breve{U}_{NT}^{*}{}'\right)\right)/\mu_{r+1}\left(\mathrm{tr}\left(\breve{U}_{NT}^{*}\breve{U}_{NT}^{*}{}'\right)\right)$ for $r > 0$ and $ER(0) = \frac{V(0)}{\frac{1}{(N-1)T}\mu_1\left(\mathrm{tr}\left(\breve{U}_{NT}^{*}\breve{U}_{NT}^{*}{}'\right)\right)\log(c_{NT})}$, and the eigenvalue growth ratio statistic $GR(r) = \log\left(\frac{V(r-1)}{V(r)}\right)/\log\left(\frac{V(r)}{V(r+1)}\right)$ for $r \ge 0$ with $V(-1) = V(0) + V(0)/\log(c_{NT})$. The number of factors is $\hat{r} = \arg\max_{0\le r\le r_{\max}} ER(r)$ or $\hat{r} = \arg\max_{0\le r\le r_{\max}} GR(r)$. In our empirical application, we find that these different criteria may give different estimates for the number of factors, and an estimated number of factors from a smaller sample can be larger than that from the full sample which suggests that the factor number selection might be less precise in finite samples and the proper selection of factors for this empirical application may remain an issue. To ensure robustness, we report estimated policy effects by varying the number of factors.

Bailey et al. (2016) provide a characterization on degrees of cross-sectional dependence in a panel. Strong dependence can be generated by common shocks or observations that influence a large number of other units, and weak dependence can arise from spatial dependence between neighboring observations. After interactive effects are controlled for, the residuals should not exhibit strong cross-sectional dependence. Kelejian and Prucha (2001) derive the asymptotic properties for Moran's *I* test for spatial dependence with a given weights matrix. Pesaran (2015) provides a test on the cross-sectional dependence (CD test) where the null hypothesis is that observations are weakly cross-sectionally dependent. Under the null of weak cross-sectional dependence, the CD test statistic is shown to have the limiting $N(0, 1)$ distribution as $N, T \to \infty$, $\frac{T}{N^{\epsilon}} \to \kappa$ for $\epsilon \in (0, 1]$ and some finite positive constant κ. As a diagnostic test, we report the CD statistic in the estimation results. Rejection of the null will indicate that there remains strong dependence in the errors and the model may be misspecified.[5]

[5] We appreciate a referee's suggestion on performing such a test for our model.

4 Estimation results

4.1 Main results

4.1.1 Violent and property crimes

Existing literature on effects of right-to-carry laws often ignores spatial dependence in crime rates and uses a panel regression model with additive individual and time fixed effects. Here we contrast the estimates from our model with those where spatial interactions are ignored and the heterogeneity is assumed to be additive, $\gamma_i' f_t = \gamma_i + f_t$. Many papers on this subject use a data set that ends in the late 1990s, so we also report estimation results for a smaller sample from 1977 to 1999. The dependent variable is the change in the log crime rates, and coefficients of regressors can be approximately interpreted as the marginal effect on $\frac{c_{it} - c_{it-1}}{c_{it-1}}$.

We firstly examine effects of right-to-carry laws and spatial dependence in violent and property crimes. Table 2 reports estimation results where models in columns 2, 4, 6, and 8 are panel regressions with state and year fixed effects. Many empirical models in this literature ignore spatial interactions and restrict interactive effects to be additive so that a two-way fixed effects regression panel is estimated. For models in columns 1, 3, 5, and 7, we also consider spatial interactions in crimes and the possibility of factors in errors where the number of factors is selected using the IC_2 criterion in (8). We will also report estimates with a different number of factors. In case that the estimated number of factors is zero, a dynamic spatial panel model with state and year fixed effects is estimated, so that the only difference with the two-way fixed effects model is from the spatial interaction and dynamic terms. As discussed in Sect. 1, the spatial interaction parameters λ and ρ measure net spillover effects of crime rates from neighboring states, as both positive and negative spillovers may be present. The results show that positive spillovers dominate and spatial effects are stronger for property crimes than violent crimes, which are consistent with Glaeser et al. (1996). The spatial dependence in property crime rates is stronger than that in violent crime rates, as the property crime rate in a state can be reduced by 0.248% for an average contemporaneous 1% reduction in the crime rates in its neighbors, compared with a 0.137% reduction for violent crimes. The time lagged spatial spillover effect is captured by ρ, and its estimate indicates that an average 1% reduction in crime rates in the neighboring states will decrease the property crime rate by 0.112% in the following year, compared with a decrease of 0.074% for the violent crime rate. As the dependent variable is the change in log crime rates, the positive coefficient of $\mathscr{Y}_{N,t-1}$ indicates a small positive lag effect for violent crimes, while it is not significant for property crimes.

We cannot conclude that right-to-carry laws have robust effects on violent or property crimes overall. The results for the additive fixed effects model show that right-to-carry laws have positive effects (6.2% increase) on violent crimes after 7 years since the passage of the law (column 4) for the sample from 1977 to 1999. However, the significance generally disappears as we have more observations and longer histories on states with right-to-carry laws, and this indicates that results from the restricted model are likely not reliable. The IC_2 criterion selects 1 factor in the error terms and

Table 2 Dynamic effects of right-to-carry laws on violent and property crime rates

	Violent crime				Property crime			
	1977–2012		1977–1999		1977–2012		1977–1999	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
First 2 years	0.010	0.011	−0.000	0.013	−0.002	0.005	0.002	0.011
	(0.011)	(0.011)	(0.012)	(0.013)	(0.005)	(0.006)	(0.006)	(0.008)
3–4 years	−0.011	−0.011	0.006	0.003	0.000	−0.008	0.001	0.000
	(0.011)	(0.011)	(0.013)	(0.014)	(0.005)	(0.007)	(0.006)	(0.008)
5–6 years	−0.004	−0.007	0.002	0.007	0.001	−0.012*	0.005	−0.011
	(0.011)	(0.011)	(0.016)	(0.018)	(0.005)	(0.007)	(0.008)	(0.011)
7–8 years	0.021*	0.021*	0.014	0.062***	0.010*	0.003	−0.002	−0.010
	(0.012)	(0.012)	(0.017)	(0.020)	(0.006)	(0.007)	(0.009)	(0.012)
9 years +	−0.005	−0.006	0.022	0.059***	−0.002	−0.015***	0.010	0.009
	(0.009)	(0.008)	(0.017)	(0.020)	(0.003)	(0.005)	(0.008)	(0.012)
$W_N y_{Nt}$	0.137***		0.110**		0.248***		0.267***	
	(0.035)		(0.044)		(0.033)		(0.041)	
$y_{N,t-1}$	0.042*		0.077**		0.028		0.107***	
	(0.025)		(0.032)		(0.025)		(0.031)	
$W_N y_{N,t-1}$	0.074*		0.043		0.112***		0.139***	
	(0.043)		(0.054)		(0.039)		(0.050)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes		Yes		Yes		Yes
No. of factors	0	–	1	–	2	–	3	–
R-squared	0.269	0.242	0.414	0.278	0.576	0.374	0.712	0.449
CD statistic	−1.084	−1.817	−1.270	−2.256	−0.218	−3.483	−0.482	−2.625

Standard errors are in parentheses. Significance levels: *10%, **5%, ***1%. The dependent variable is $\log c_{it} - \log c_{it-1}$ where c_{it} is the number of reported crimes in a respective category per 100,000 population. The number of factors in models (1), (3), (5), and (7) is selected using the IC_2 criterion in (8). The CD statistic for testing the hypothesis of weakly cross-sectionally dependent errors (Pesaran 2015) is reported

the effects become much less significant with the factor controlled for (column 3). For property crimes, the long-term impact is significant from the two-way fixed effects model using the full sample (1977–2012). After 9 years since the passage of the law, property crime is decreased by 1.5% annually. However, the effect is not significant in the shorter sample, and it becomes insignificant if the model includes 2 factors.

Besides the IC_2 criterion (8), we also estimate the number of factors using other factor number selection criteria (such as Bai and Ng 2002; Ahn and Horenstein 2013) and find that they can be different in our sample, with Ahn and Horenstein (2013) usually giving a smaller estimated number of factors and the $PC_1 - PC_3$ criteria giving larger factor number estimates. The factor number estimate based on the IC_2 criterion is oftentimes between the other estimates. It is also noted in Table 2 that the estimated number of factors is 3 for property crime rates in the subsample from 1977 to 1999, which is larger than 2 from the full sample. These might indicate that

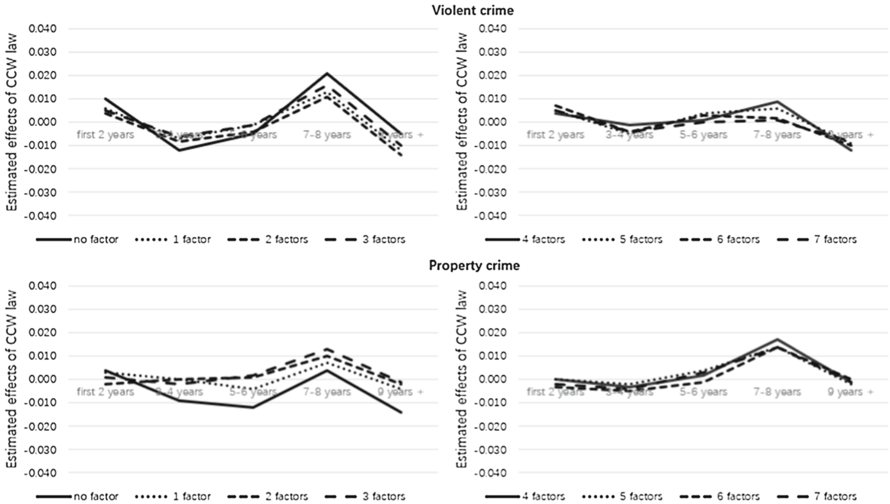

Fig. 4 Dynamic effects of right-to-carry laws for different number of factors, dependent variable: violent crime, property crime

factor number estimation might not be precise given our sample size. In order to ensure that our estimates are robust to the number of factors, we report estimates on effects of right-to-carry laws using different numbers of factors, from 0 to 7 in Fig. 4. Controlling for additional factors tends to decrease the estimated effects of right-to-carry laws, although the differences are small for violent crimes which is not surprising as the estimated number of factors is zero. On the other hand, for property crimes, the spatial model with no factors shows a large long-term effect of the law (− 1.4% annual decline 9 years after the passage of the law, Fig. 4 “no factor” line) which is similar to the estimate from the model without spatial interaction terms (− 1.5% from model 6 of Table 2). According to the IC_2 criterion, there are 2 common unobserved factors in errors. When only one factor is controlled for, the effect diminishes, which suggests that the earlier estimate may be driven by some unobserved common shocks. Except for the model with no factors, estimates with a different number of factors are generally close and their magnitudes are small. Overall the results are consistent with the findings in Table 2 that right-to-carry laws have no robust effects on violent or property crime rates in this sample, as the significant estimates are not robust to a different sample ending point or common unobserved factors.

4.1.2 Types of violent and property crimes

We next examine effects of right-to-carry laws on different types of violent and property crimes. Tables 3, 4, and 5 show results where the dependent variable is the rate of murder, rape, robbery or aggravated assault among violent crimes, and the rate of burglary, larceny or motor vehicle theft among property crimes. For each type of crimes, we report estimates of the dynamic spatial panel model with interactive effects, and a restricted model where spatial interaction terms are dropped and state and year

Table 3 Dynamic effects of right-to-carry laws on murder and rape

	Murder and non-negligent manslaughter				Rape			
	1977–2012		1977–1999		1977–2012		1977–1999	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
First 2 years	−0.002	0.005	−0.020	−0.011	0.010	0.011	−0.003	0.007
	(0.017)	(0.033)	(0.017)	(0.042)	(0.014)	(0.015)	(0.012)	(0.019)
3–4 years	0.023	0.009	0.007	0.003	−0.023	−0.022	−0.024*	−0.030
	(0.017)	(0.034)	(0.018)	(0.044)	(0.014)	(0.015)	(0.013)	(0.020)
5–6 years	−0.043**	−0.030	−0.053**	−0.108*	0.001	0.004	−0.010	0.000
	(0.018)	(0.034)	(0.023)	(0.057)	(0.015)	(0.015)	(0.016)	(0.025)
7–8 years	−0.004	−0.014	−0.015	−0.033	0.018	0.017	0.047***	0.057**
	(0.018)	(0.036)	(0.024)	(0.063)	(0.015)	(0.016)	(0.018)	(0.028)
9 years +	−0.011	−0.013	0.017	−0.030	0.008	0.005	0.013	0.018
	(0.010)	(0.026)	(0.024)	(0.065)	(0.011)	(0.012)	(0.017)	(0.029)
$W_N y_{Nt}$	0.170***		0.278***		0.089**		0.107**	
	(0.033)		(0.040)		(0.035)		(0.044)	
$y_{N,t-1}$	−0.395***		−0.333***		−0.223***		−0.108***	
	(0.025)		(0.037)		(0.024)		(0.033)	
$W_N y_{N,t-1}$	0.150***		0.188***		0.037		0.076	
	(0.031)		(0.035)		(0.047)		(0.050)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects		Yes		Yes	Yes	Yes		Yes
No. of factors	6	–	7	–	0	–	4	–
R-squared	0.786	0.055	0.879	0.055	0.218	0.162	0.645	0.185
CD statistic	1.731	3.498	−0.288	4.244	1.733	−1.714	−0.155	−1.401

Standard errors are in parentheses. Significance levels: *10%, **5%, ***1%. The dependent variable is $\log c_{it} - \log c_{it-1}$ where c_{it} is the number of reported crimes in a respective category per 100,000 population. The number of factors in models (1), (3), (5), and (7) is selected using the IC_2 criterion in (8). The CD statistic for testing the hypothesis of weakly cross-sectionally dependent errors (Pesaran 2015) is reported

Table 4 Dynamic effects of right-to-carry laws on robbery and assault

	Robbery				Aggravated assault			
	1977–2012		1977–1999		1977–2012		1977–1999	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
First 2 years	0.034***	0.035**	0.027*	0.043**	0.008	0.009	−0.009	0.013
	(0.012)	(0.016)	(0.014)	(0.020)	(0.014)	(0.014)	(0.014)	(0.017)
3–4 years	−0.017	−0.016	−0.024	−0.018	−0.011	−0.009	0.006	0.013
	(0.012)	(0.016)	(0.015)	(0.021)	(0.014)	(0.014)	(0.015)	(0.018)
5–6 years	−0.016	−0.026	−0.014	−0.025	0.000	−0.004	0.010	0.025
	(0.012)	(0.016)	(0.019)	(0.027)	(0.014)	(0.014)	(0.019)	(0.023)
7–8 years	0.013	0.022	−0.001	0.041	0.023	0.023	0.026	0.075***
	(0.013)	(0.017)	(0.021)	(0.030)	(0.015)	(0.015)	(0.021)	(0.026)
9 years +	−0.009	−0.015	0.011	0.011	−0.005	−0.004	0.019	0.083***
	(0.007)	(0.012)	(0.020)	(0.031)	(0.011)	(0.011)	(0.022)	(0.026)
$W_N y_{Nt}$	0.110***		−0.017		0.069*		0.091**	
	(0.035)		(0.045)		(0.036)		(0.044)	
$y_{N,t-1}$	−0.074***		−0.040		0.078***		0.030	
	(0.025)		(0.032)		(0.025)		(0.032)	
$W_N y_{N,t-1}$	0.171***		0.259***		0.113***		0.096*	
	(0.040)		(0.051)		(0.043)		(0.051)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects		Yes		Yes	Yes	Yes		Yes
No. of factors	2	–	3	–	0	–	2	–
R-squared	0.500	0.211	0.609	0.222	0.182	0.162	0.445	0.186
CD statistic	−1.050	−2.338	−0.362	−2.231	−1.209	−1.824	−0.740	−2.468

Standard errors are in parentheses. Significance levels: *10%, **5%, ***1%. The dependent variable is $\log c_{it} - \log c_{it-1}$ where c_{it} is the number of reported crimes in a respective category per 100,000 population. The number of factors in models (1), (3), (5), and (7) is selected using the IC_2 criterion in (8). The CD statistic for testing the hypothesis of weakly cross-sectionally dependent errors (Pesaran 2015) is reported

Table 5 Dynamic effects of right-to-carry laws on types of property crimes

	Burglary				Larceny				Motor vehicle theft			
	1977–2012		1977–1999		1977–2012		1977–1999		1977–2012		1977–1999	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
First 2 years	0.009	0.010	0.008	0.012	−0.004	0.002	0.002	0.009	0.009	0.012	0.023*	0.017
	(0.008)	(0.009)	(0.010)	(0.011)	(0.005)	(0.007)	(0.006)	(0.008)	(0.013)	(0.013)	(0.014)	(0.016)
3–4 years	−0.014	−0.012	−0.010	−0.006	−0.000	−0.008	0.003	0.001	−0.010	−0.009	−0.006	0.000
	(0.009)	(0.009)	(0.011)	(0.011)	(0.005)	(0.007)	(0.006)	(0.009)	(0.013)	(0.013)	(0.015)	(0.016)
5–6 years	−0.011	−0.009	−0.010	−0.005	0.002	−0.012*	0.001	−0.011	−0.026**	−0.029**	−0.033*	−0.038*
	(0.009)	(0.009)	(0.014)	(0.014)	(0.005)	(0.007)	(0.008)	(0.011)	(0.013)	(0.013)	(0.019)	(0.021)
7–8 years	0.007	0.004	0.005	0.002	0.010*	0.004	0.002	−0.014	−0.006	−0.007	−0.005	−0.005
	(0.009)	(0.010)	(0.015)	(0.016)	(0.006)	(0.007)	(0.009)	(0.012)	(0.014)	(0.014)	(0.020)	(0.023)
9 years +	−0.018***	−0.017**	0.007	0.009	-0.002	−0.013***	0.006	0.005	−0.018*	−0.020**	0.040**	0.027
	(0.007)	(0.007)	(0.016)	(0.016)	(0.003)	(0.005)	(0.009)	(0.013)	(0.010)	(0.010)	(0.020)	(0.024)
$W_N y_{Nt}$	0.263***		0.301***		0.287***		0.295***		0.181***		0.205***	
	(0.032)		(0.040)		(0.032)		(0.040)		(0.034)		(0.042)	
$y_{N,t-1}$	−0.133***		−0.097***		0.029		0.090***		0.004		0.067**	
	(0.024)		(0.031)		(0.025)		(0.032)		(0.025)		(0.032)	
$W_N y_{N,t-1}$	0.151***		0.143***		0.087**		0.112**		0.137***		0.057	
	(0.042)		(0.056)		(0.038)		(0.049)		(0.044)		(0.054)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes		Yes		Yes	Yes	Yes		Yes
No. of factors	0	–	0	–	2	–	3	–	0	–	1	–

Table 5 continued

	Burglary				Larceny				Motor vehicle theft			
	1977–2012		1977–1999		1977–2012		1977–1999		1977–2012		1977–1999	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
R-squared	0.398	0.323	0.489	0.401	0.577	0.368	0.709	0.433	0.347	0.306	0.403	0.270
CD statistic	0.313	−3.463	−0.153	−2.801	−0.509	−3.514	−0.138	−2.688	−0.083	−3.611	−1.552	−2.767

Standard errors are in parentheses. Significance levels: $^{*}10\%$, $^{**}5\%$, $^{***}1\%$. The dependent variable is $\log c_{it} - \log c_{it-1}$ where c_{it} is the number of reported crimes in a respective category per 100,000 population. The number of factors in models (1), (3), (5), (7), (9), and (11) is selected using the IC_2 criterion in (8). The CD statistic for testing the hypothesis of weakly cross-sectionally dependent errors (Pesaran 2015) is reported

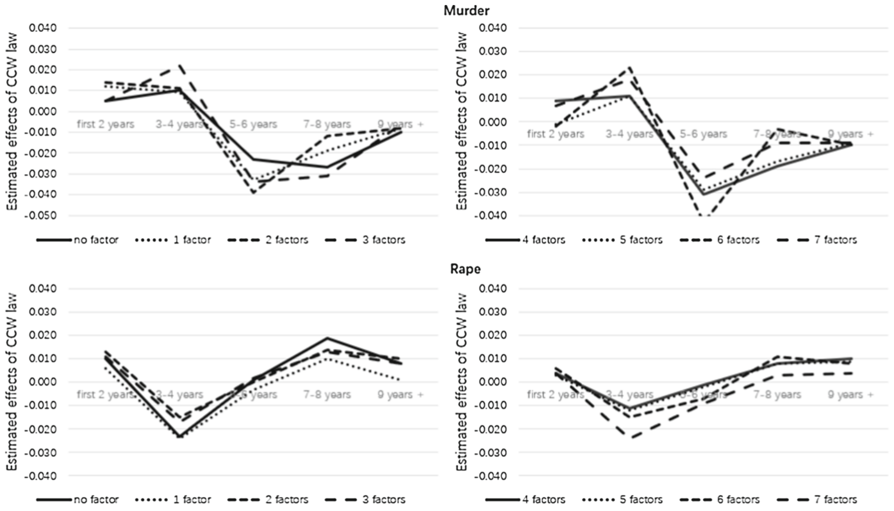

Fig. 5 Dynamic effects of right-to-carry laws for different number of factors, dependent variable: murder, rape

effects are additive. For the model with interactive effects, the number of factors is selected using the IC_2 criterion in (8). As in the previous section, if the estimated number of factors is zero, a dynamic spatial panel with state and year fixed effects is estimated so that the only difference with the restricted model comes from spatial interaction and dynamic terms. If the estimated number of factors is positive, the methods in Sect. 3 will be used and state fixed effects will be absorbed into factor components. Estimation results are reported for the full sample (1977–2012) and a shorter sample (1977–1999) separately. Figures 5, 6, and 7 show how the estimated effects of right-to-carry laws change as a different number of factors is assumed.

We find that right-to-carry laws lead to a 4.3% reduction in murder rates (Table 3 column 1) and 2.6% reduction in motor vehicle theft rates (Table 5 column 9) annually 5–6 years after the passage of the law, a 3.4% increase in robbery rates during the first 2 years (Table 4 column 1), and an annual 1.8% decline in burglary rates 9 years after the passage of the law (Table 5 column 1). The negative effects on murder and motor vehicle theft rates and the positive effects on robbery rates are similar using the smaller sample from 1977 to 1999 and a different number of factors (Figs. 5, 6, 7). In particular, Fig. 6 shows that the estimated short-term increase in the robbery rate is almost identical as more factors are assumed, which suggests that the result is likely quite robust to unobserved heterogeneities. The opposite effects on murder and robbery may explain the insignificant overall effect on (aggregated) violent crimes. On the other hand, Fig. 7 shows that the negative long-term effect on burglary is not robust to additional factors, as the estimate becomes close to zero when factors are controlled for in the error terms. The small long-term effect is also observed for other types of property crimes (larceny and motor vehicle theft) when factors are controlled for. Overall, right-to-carry laws in general do not have significant effects on property crimes, except for auto theft where the effect is negative 5–6 years after the passage

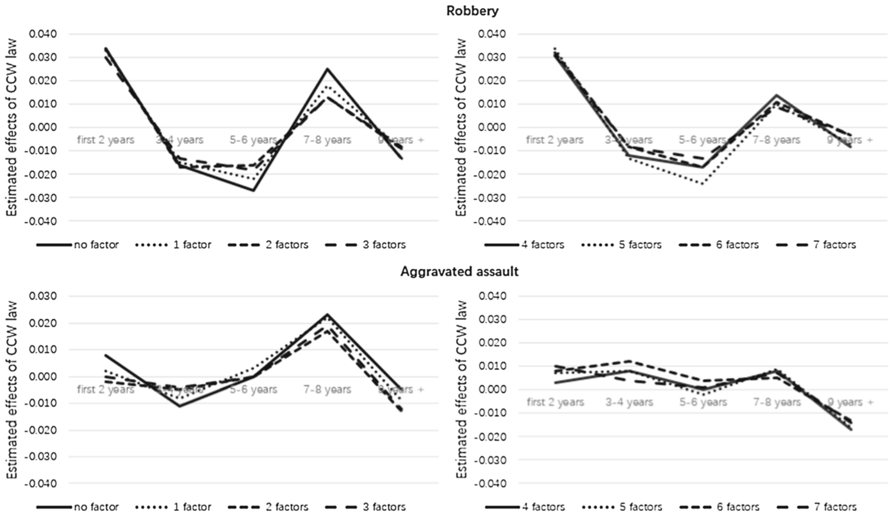

Fig. 6 Dynamic effects of right-to-carry laws for different number of factors, dependent variable: robbery, assault

of the law (Table 5), perhaps because people take more precautions as they carry weapons in their vehicles. Among all the specifications, we find robust evidence that right-to-carry laws lead to short-term increases in the robbery rate, and there is also evidence of medium-term negative effect on murder and motor vehicle theft rates.

Right-to-carry laws have external effects on neighboring states due to spatial dependence in crime rates. Consistent with findings using violent and property crime rates, spatial interaction effects in crimes are positive, and they are stronger for property crimes such as burglary, larceny, and motor vehicle theft than violent crimes. The spatial interaction effect for assault and rape is smaller than other types of crimes. Our finding using the spatial autoregressive model is also consistent with an earlier work by Glaeser et al. (1996) which analyze the covariance between city-level crime rates and conclude that the degree of social interaction is strongest for petty crimes and smallest for murder and rape. Based on the full sample, an average 1% reduction in the burglary, larceny or motor vehicle theft rate in a state's neighbors can reduce the corresponding crime rate by 0.181–0.287% in the same year, compared with 0.069–0.170% reductions for types of violent crimes. With the exception of robbery, crime rates appear to become less spatially dependent over time as the estimated spatial interaction coefficients based on the earlier sample (1977–1999) are larger than those of the later sample. In terms of dynamics, we find partial mean reversion for murder, rape, robbery, and burglary as the negative coefficient on $y_{N,t-1}$ indicates that increases in crime rates in the previous year are partially offset in the current year. Based on the full sample, the coefficient on $W_N y_{N,t-1}$ indicates that an average 1% reduction in a crime rate in a state's neighbors can reduce the crime rate by 0.037–0.171% the next year depending on crime categories.

Next we examine the model on murder and robbery rates in more detail. As from (4) and (5), total effects of right-to-carry laws also depend on the spatial interaction

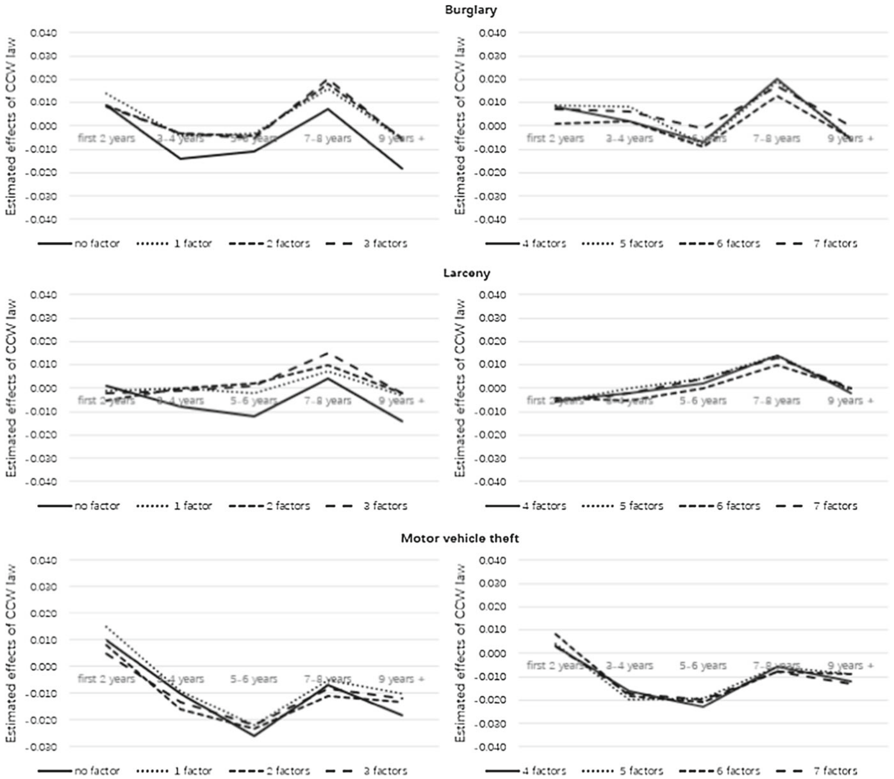

Fig. 7 Dynamic effects of right-to-carry laws for different number of factors, dependent variable: burglary, larceny, motor vehicle theft

parameters (λ, ϕ, ρ) and the spatial structure W_N. Effects of right-to-carry laws may also vary over time as captured by their lagged terms in the model. In models with a spatial autoregressive lag, a state can also be affected by right-to-carry laws in its neighboring states. LeSage and Pace (2009) (Section 2.7) provide discussions on direct and indirect effects in spatial models and methods to calculate their dispersions. From (4) and (5), the average direct effect in year t is $\frac{1}{N}\text{tr}\left(M_{N,t}\right)$ with $M_{N,t} = S_N\left(\hat{\lambda}\right)^{-1}\left(D_N\left(\hat{\phi}, \hat{\rho}\right)S_N\left(\hat{\lambda}\right)^{-1}\right)^{t-1}$, and the average indirect effect is $\frac{1}{N}\left(\ell_N' M_{N,t}\ell_N - \text{tr}\left(M_{N,t}\right)\right)$. The 95% confidence bands are constructed using the empirical distribution of the estimated model parameters based on 10,000 simulation draws from a multivariate normal distribution according to the QML estimates and their asymptotic variance matrix. Figure 8 shows the average direct effects and the average indirect effects over time for murder and robbery rates. For example, right-to-carry legislation increases the robbery rate by 3.4% in the first year since the law's passage (Table 4 column 1, $\tau_1 = 0.034$). From Fig. 8, due to spatial effects, this one year's effect generates on average an approximately 3.4% increase in the own state's robbery rate, and 0.034% (10% spillovers) total increase in the state's neigh-

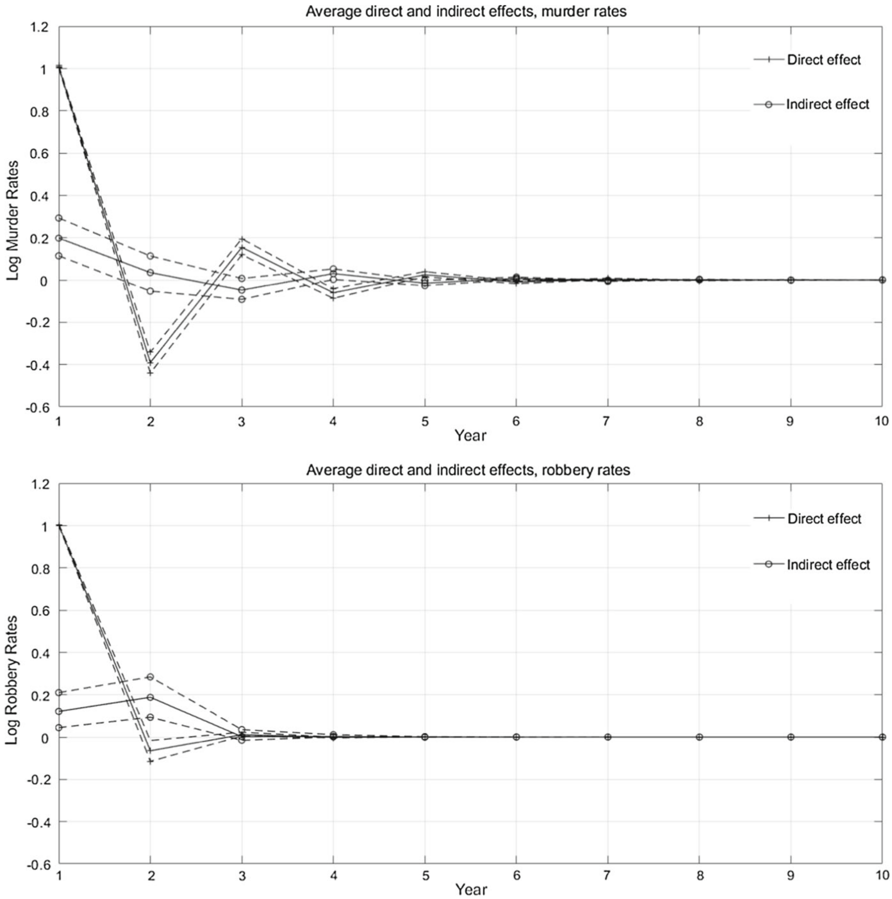

Fig. 8 Average estimated direct and indirect effects with pointwise 95% confidence bands

bors. The spillover effect remains significant in the following year. The lagged effects for murder rates are stronger. Positive spillover effects imply that the passage of right-to-carry laws decreases murder rates but increases robbery rates in neighboring states including states that have not passed right-to-carry laws. If the spatial spillover effects are ignored, a direct comparison of states with and without right-to-carry laws may underestimate the magnitude of the effects.

For the model on the robbery rate using the full sample, there are two estimated factors. The upper panel in Fig. 9 plots the estimated time factors, and the lower panel shows their loadings. Because estimated factors are rotations of true factors (Bai 2003), their values are not directly interpretable. With this caveat, the figure shows that the two estimated time factors have more similar patterns after early 1990s while they capture more different time trends prior to 1990. In the lower panel of Fig. 9, the horizontal axis corresponds to the value of the loadings for the first factor and the vertical axis corresponds to the second factor. The figure shows that many states have wildly different loadings, and as a result, they are affected differently

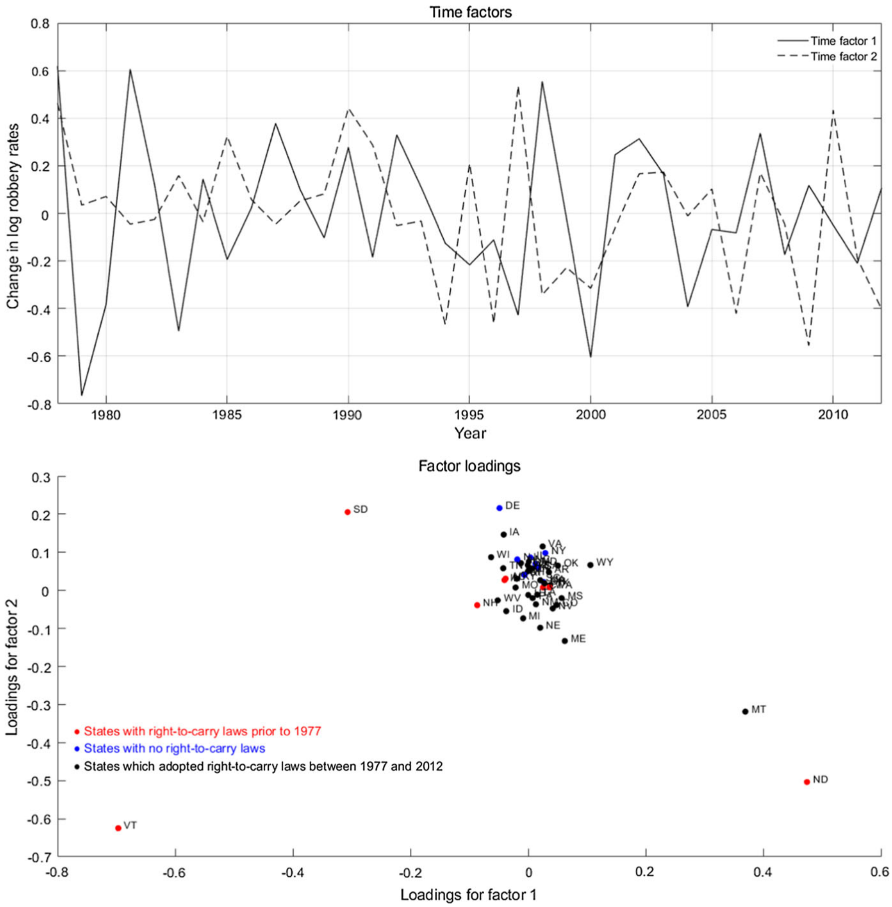

Fig. 9 Estimated time factors and factor loadings, dependent variable: robbery

by the time factors and the two-way fixed effects specification where time effects have homogeneous impacts on states may not be appropriate in this case. Eight states adopted right-to-carry laws prior to 1977 and they are colored in red in the figure, and nine states have not adopted right-to-carry laws by 2012 and they are colored in blue. Other states adopted right-to-carry laws between 1977 and 2012. With the synthetic control method, the counterfactual of the treated is constructed as a weighted average of control units. If the DGP has a factor structure, the assumption is that factor loadings of the treated should be in the convex hull of factor loadings of control units (Gobillon and Magnac 2016). However, as the figure shows, if Delaware is excluded from control units, for both factors, their loadings of many treated units are outside the convex hull of those of control units,[6] which suggest that the interactive effects model may be preferable to the synthetic control method in this application.

6 With a rotation, the loadings of treated units may be inside the convex hull of those of control units.

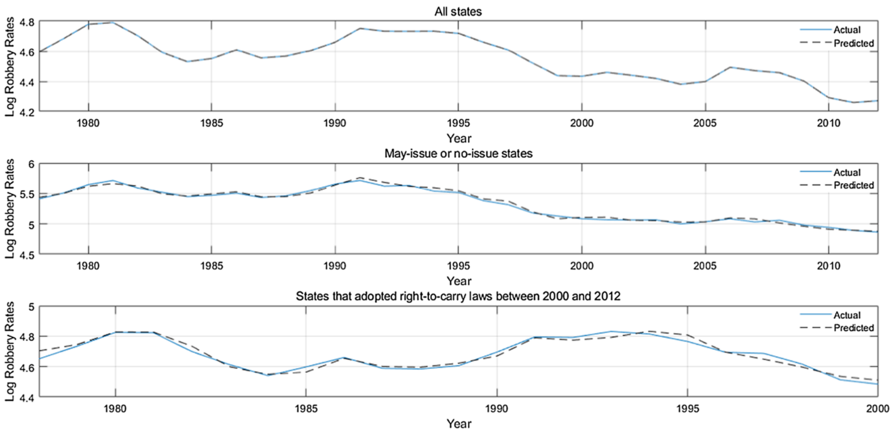

Fig. 10 Average observed and predicted log robbery rates for all states, states with no right-to-carry laws, and states that adopted right-to-carry laws between 2000 and 2012

4.2 Robustness checks

4.2.1 Model fit

The CD test statistics of Pesaran (2015) are reported in all tables on estimated results. The limiting distribution of the test statistic under the null of weak cross-sectional dependence is standard normal. The results show that while the CD test is often rejected in the additive fixed effects model, they pass in the model with added interactive fixed effects, that gives reassurance that strong cross-sectional dependence in errors is well captured by included interactive effects.

As a further robustness check, in Fig. 10 we compare the actual and predicted log robbery rates for all states, for states that have not passed right-to-carry laws by 2012, which can be viewed as the control states, and for states that adopted right-to-carry laws between 2000 and 2012 (see Footnote 1 for its list). States in the control group have on average slightly higher robbery rates, but their average robbery rates generally have similar time patterns with national averages. Our model can capture these time patterns quite well. The bottom panel of the figure shows that for the states before their right-to-carry laws were adopted, the predicted values from our model match the actual values closely, which indicates that the counterfactuals of no right-to-carry laws after the adoption of the law are likely valid.

4.2.2 Spatial Durbin model specification

We further examine mechanisms behind spatial dependence in crimes by analyzing both spatial effects in the dependent variable (endogenous interaction effect) and spatial effects in the explanatory variables (exogenous interaction effect) using a spatial Durbin model (Anselin 1988). Accounting for both effects enriches the model. The exogenous interaction effect is captured by a spatial lag of the explanatory variables, which reflects local spillovers from explanatory variables as the dependent variable is affected by its own and direct neighbors' explanatory variables. The endogenous interaction effect is still modeled by a spatial lag of the dependent variable, which has a spatial equilibrium interpretation because it also allows spillover and feedback from higher-order neighbors. The ratio between indirect and direct effects can be different between explanatory variables in spatial Durbin models, which is discussed in Vega and Elhorst (2015), and in our application, this means that the ratio between indirect and direct effects can be different for different number of years since right-to-carry legislation.

From theoretical perspectives, those spillover effects have ambiguous signs. For the exogenous interaction effect, right-to-carry legislation in a neighboring state can decrease crime if the legislation increases the level of vigilance which deters crime. On the other hand, the crime rate can become higher if right-to-carry legislation in the neighboring state makes weapons more widely available which may facilitate crime. Using county-level annual crime data from 1977 to 1992, Bronars and Lott (1998) find that for many crime categories, right-to-carry legislation in neighboring states generally increases crimes and this is mitigated if own state also has enacted right-to-carry laws. As discussed earlier, in theory the endogenous interaction effect also has an ambiguous sign.

These hypotheses are examined using a spatial Durbin model and an interaction term between average neighboring states' and own state's right-to-carry status. Table 6 reports the estimation results where the dependent variable is murder or robbery rate. The coefficient of the endogenous spatial interaction term ($W_N y_{Nt}$) is similar across those model specifications for each type of crimes. Columns (2) and (5) show that most of the spatial lags of the explanatory variables have insignificant and quantitatively small coefficients. In addition, we find that spillover effects from neighbor's right-to-carry legislation are mediated by whether own state has right-to-carry laws. For example, comparing columns (6) and (5), right-to-carry legislation in neighboring states on average decreases robbery rates by 8.9% seven to eight years since the passage of the law, which may be due to increased level of vigilance. If own state also has right-to-carry laws, the crime rate is increased by 8.1% which means that the net effect is close to zero. The negative effect on robbery rates from right-to-carry legislation in neighboring states occurs only in states that have not enacted right-to-carry laws. The estimates are consistent with right-to-carry laws increasing both the availability of weapons and the level of vigilance among the public. The net direct effect is positive, as right-to-carry legislation increases robbery rates within two years of the law's passage. If own state does not have right-to-carry laws, and neighboring states pass the law, the increase in the level of vigilance dominates and the spillover effect from neighboring states' laws is negative. However, the hypothesis that the coefficients of the spatial lags of the explanatory variables are all zero cannot be rejected at conventional significance levels using likelihood ratio tests,[7] which means that the more complex model does not increase the explanatory power of the model much. Therefore, the endogenous interaction effect is the dominant spatial effect.

[7] The conclusions for other types of crimes are similar, and the results are available upon request.

Table 6 Dynamic effects of right-to-carry laws on murder and robbery, spatial Durbin model

	Murder			Robbery		
	(1)	(2)	(3)	(4)	(5)	(6)
First 2 years	− 0.002	− 0.002	0.004	0.034***	0.031***	0.029**
	(0.017)	(0.017)	(0.018)	(0.012)	(0.012)	(0.013)
3–4 years	0.023	0.025	0.023	− 0.017	− 0.018	− 0.023*
	(0.017)	(0.018)	(0.019)	(0.012)	(0.013)	(0.013)
5–6 years	− 0.043**	− 0.041**	− 0.047**	− 0.016	− 0.017	− 0.022*
	(0.018)	(0.018)	(0.019)	(0.012)	(0.013)	(0.013)
7–8 years	− 0.004	− 0.002	− 0.002	0.013	0.013	0.007
	(0.018)	(0.018)	(0.019)	(0.013)	(0.013)	(0.014)
9 years +	− 0.011	− 0.009	− 0.009	− 0.009	− 0.008	− 0.012
	(0.010)	(0.011)	(0.012)	(0.007)	(0.008)	(0.009)
$W_N \times$First 2 years		0.021	0.084*		0.015	0.028
		(0.031)	(0.044)		(0.023)	(0.032)
$W_N \times$3–4 years		− 0.031	− 0.062		0.022	0.011
		(0.033)	(0.049)		(0.023)	(0.035)
$W_N \times$5–6 years		− 0.017	− 0.078		0.026	0.010
		(0.034)	(0.052)		(0.023)	(0.037)
$W_N \times$7–8 years		0.003	− 0.022		− 0.033	− 0.089**
		(0.033)	(0.053)		(0.024)	(0.039)
$W_N \times$9 years +		− 0.018	− 0.005		− 0.003	− 0.006
		(0.016)	(0.025)		(0.012)	(0.020)
$W_N \times$First 2 years$\times$ own right-to-carry dummy$_t$			− 0.112**			− 0.022
			(0.057)			(0.040)
$W_N \times$3–4 years$\times$ own right-to-carry dummy$_t$			0.047			0.019
			(0.061)			(0.042)
$W_N \times$5–6 years$\times$ own right-to-carry dummy$_t$			0.096			0.026
			(0.062)			(0.043)
$W_N \times$7–8 years$\times$ own right-to-carry dummy$_t$			0.046			0.081*
			(0.062)			(0.045)
$W_N \times$9 years +$\times$ own right-to-carry dummy$_t$			− 0.019			0.003
			(0.026)			(0.020)
$W_N y_{Nt}$	0.170***	0.167***	0.160***	0.110***	0.107***	0.108***
	(0.033)	(0.034)	(0.034)	(0.035)	(0.035)	(0.035)

Table 6 continued

	Murder			Robbery		
	(1)	(2)	(3)	(4)	(5)	(6)
$y_{N,t-1}$	-0.395^{***}	-0.396^{***}	-0.399^{***}	-0.074^{***}	-0.076^{***}	-0.077^{***}
	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
$W_N y_{N,t-1}$	0.150^{***}	0.148^{***}	0.146^{***}	0.171^{***}	0.170^{***}	0.170^{***}
	(0.031)	(0.031)	(0.031)	(0.040)	(0.040)	(0.040)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects						
No. of factors	6	6	6	2	2	2
CD statistic	1.731	1.750	2.010	-1.050	-1.050	-0.954
log likelihood	3649.610	3650.836	3654.530	4026.005	4028.243	4030.318

The crime rates are from 1977 to 2012. Standard errors are in parentheses. Significance levels: $^{*}10\%$, $^{**}5\%$, $^{***}1\%$. The dependent variable is $\log c_{it} - \log c_{it-1}$ where c_{it} is the number of reported crimes in a respective category per 100,000 population. The number of factors in models is selected using the IC_2 criterion in (8). The CD statistic for testing the hypothesis of weakly cross-sectionally dependent errors (Pesaran 2015) is reported

4.2.3 Alternative spatial weights matrix

The choice of a spatial weights matrix is sometimes subject to debate. We now consider a row-normalized contiguity matrix weighted by average state population from 1977 to 2012. The estimation results are reported in Table 7. Using the full sample, right-to-carry laws still have negative and significant effects on murder and motor vehicle theft rates 5–6 years after the passage of the law, and positive and significant effects on the robbery rate within 2 years of the right-to-carry legislation. The spatial interaction effect is stronger in property crimes than violent crimes, which is also consistent with earlier results.

5 Conclusion

Right-to-carry laws are controversial in part due to conflicting estimates of their effects on crimes reported in the literature. The main challenge is to construct counterfactual crime rates had right-to-carry laws not been adopted. As with other program evaluations using non-experimental data, it is often a concern that there are systematic differences between the states that have adopted right-to-carry laws and states that have not. Many papers in this literature often assume that heterogeneities consist of a state-specific time invariant component and a (homogeneous) time effect that is common to all states, and use a set of explanatory variables to control for confounding factors that might correlate with both the passage of right-to-carry laws and crime. This paper relaxes the assumption that all time factors have homogeneous effects on states; therefore, different states may follow different time trends. Heterogeneous time trends are modeled by a factor structure where time factors represent time-varying

Table 7 Dynamic effects of right-to-carry laws on murder, alternative spatial weights matrix

	Violent crime	Murder	Rape	Robbery	Assault	Property crime	Burglary	Larceny	Auto theft
First 2 years	0.010	− 0.002	0.010	0.034***	0.008	− 0.001	0.010	− 0.003	0.010
	(0.011)	(0.017)	(0.014)	(0.012)	(0.014)	(0.005)	(0.008)	(0.005)	(0.013)
3–4 years	− 0.011	0.024	− 0.023	− 0.017	− 0.010	− 0.000	− 0.015*	− 0.000	− 0.011
	(0.011)	(0.017)	(0.014)	(0.012)	(0.014)	(0.005)	(0.009)	(0.005)	(0.013)
5–6 years	− 0.005	− 0.044**	0.002	− 0.018	− 0.001	0.000	− 0.012	0.001	− 0.028**
	(0.011)	(0.018)	(0.015)	(0.012)	(0.014)	(0.005)	(0.009)	(0.005)	(0.013)
7–8 years	0.021*	− 0.004	0.019	0.013	0.022	0.010*	0.006	0.009*	− 0.008
	(0.012)	(0.018)	(0.015)	(0.013)	(0.015)	(0.006)	(0.009)	(0.006)	(0.014)
9 years +	− 0.006	− 0.012	0.008	− 0.010	− 0.006	− 0.002	− 0.019***	− 0.002	− 0.020*
	(0.009)	(0.010)	(0.011)	(0.007)	(0.011)	(0.003)	(0.007)	(0.003)	(0.010)
$W_N y_{Nt}$	0.132***	0.148***	0.088***	0.128***	0.074**	0.257***	0.287***	0.272***	0.189***
	(0.032)	(0.031)	(0.032)	(0.032)	(0.033)	(0.030)	(0.030)	(0.030)	(0.031)
$y_{N,t-1}$	0.044*	− 0.391***	− 0.222***	− 0.076***	0.080***	0.032	− 0.136***	0.036	0.004
	(0.025)	(0.025)	(0.024)	(0.025)	(0.025)	(0.025)	(0.024)	(0.025)	(0.025)
$W_N y_{N,t-1}$	0.077	0.205***	0.019	0.157***	0.108**	0.082**	0.139***	0.069*	0.111**
	(0.047)	(0.036)	(0.052)	(0.043)	(0.048)	(0.038)	(0.042)	(0.039)	(0.044)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes		Yes		Yes		Yes		Yes
No. of factors	0	6	0	2	0	2	0	2	0
R-squared	0.267	0.786	0.217	0.500	0.181	0.578	0.405	0.576	0.348
CD statistic	0.279	1.793	1.828	− 1.069	− 1.251	− 0.483	0.235	− 0.659	0.001

The crime rates are from 1977 to 2012. Standard errors are in parentheses. Significance levels: *10%, **5%, ***1%. The dependent variable is $\log c_{it} - \log c_{it-1}$ where c_{it} is the number of reported crimes in a respective category per 100,000 population. The spatial weights matrix is the row-normalized contiguity matrix weighted by average state population from 1977 to 2012. The number of factors in models is selected using the IC_2 criterion in (8). The CD statistic for testing the hypothesis of weakly cross-sectionally dependent errors (Pesaran 2015) is reported unobservables that affect all states, and factor loadings account for possible heterogeneous impacts that time factors may have on different states. In addition to a specific additive time effect, the factor structure is also treated as fixed parameters so as to allow for correlations between the regressors and the factors.

Crime statistics exhibit spatial dependence, and a state's adoption of right-to-carry law may have spillover effects to its neighbors. Using a dynamic spatial panel model with interactive effects, we find positive spatial spillovers in crime rates. Depending on a crime category, an average 1% reduction in crime rates in neighboring states can decrease crime rates by 0.069–0.287%, with property crimes exhibiting higher degrees of spatial dependence. We find that the passage of right-to-carry laws has no significant effects on the overall violent crime or property crime rates. However, with disaggregate violent crimes, for murder and non-negligent manslaughter, right-to-carry laws are associated with an annual 4.3% reduction in their rates 5–6 years after the passage of the law. On the other hand, right-to-carry laws are also associated with short-term increases in robbery rate. In the first two years, robbery rate is increased by about 3.4% annually. The empirical results hold for different numbers of factors and in subsample from 1977 to 1999 versus the full sample periods.

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