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Do parole abolition and Truth-in-Sentencing deter violent crimes in Virginia?

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Abstract This paper examines the treatment effect of the justice reform enacted on January 1, 1995, in Virginia. Using FBI's Uniform Crime Report data about crime rates per 100,000 population from 1960 to 2010, we find that after the reform the reported aggregated violent crime rate declined significantly and is mainly driven down by the decrease in robbery. We also consider property crime and find that the reported property crime rate does not decline until 4 years later, indicating that the justice reform in Virginia also has lagged treatment effect on property crime.

Keywords Average treatment effect · Counterfactual analysis · Parole abolition · Truth-in-Sentencing

JEL Classification C31 · K14

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1 Introduction

This paper evaluates the treatment effect of the justice reform in Virginia. From January 1, 1995, Virginia abolished discretionary parole for all violent crimes, reformed its sentencing systems by establishing the Truth-in-Sentencing (TIS) structure, and extensively enhanced the sentences on all violent offenders. Table 1 demonstrates the details: Punishment becomes tougher on repeat violent offenders with prior conviction greater than or equal to 40 years. For example, the median serving time for a first-degree murder offender, who has prior conviction greater than or equal to 40 years, has increased to about 80 years during 1999–2001 compared to 14 years during 1988–1992. Even for offenders without prior crime records, the median serving years have almost doubled (for rape) or tripled (for first-/second-degree murder and robbery).

Even though there are voluminous literatures about the relationship between incarcerating time and crime rate, the conclusions are far from consensus. For example, Myers (1980) argues that tougher punishment does not necessarily lead to substantial rehabilitative effect. Marvell and Moody (1996) also do not find convincing evidence to support the argument that the determinate sentencing laws (DSL) and abolition of parole significantly suppress the growth of prison population, because the estimated impacts on commitments vary state by state, and there is little or no evidence that DSL affect crime rate. In a more related research, Sridharan et al. (2003) use time series intervention analysis on the violent crime rates in Virginia. Using ARIMA and structural time series models and controlling the serial dependence between adjacent error terms, they find evidence that the parole abolition and TIS laws only had deterring effects on rape and murder, while the deterring effect for property crimes and aggravated assaults is not statistical significant.

In contrast to the findings above, McPheters et al. (1984) examine the deterrent response of robbery with a firearm in Arizona as penalties became tougher for using firearms. They conclude that offenders reduce the number of robberies with a firearm, and the response is abrupt rather than gradual. Levitt (1996) argues that the elasticity is -0.4 for changes in violent crime with respect to changes in prison population, after controlling various covariates such as economics factors, percent changes in police staffing, racial composition, and the age distribution. Kuziemko (2013) focuses on

Table 1 Median years violent offenders served in Virginia

	FY 1988–FY 1992			FY 1999–FY 2001		
	No prior	Prior $<$ 40	Prior $>$ 40	No prior	Prior $<$ 40	Prior $>$ 40
First-Degree Murder	12.4	14.1	14.7	35.3	51.5	80.3
Second-Degree Murder	4.9	6.6	7.2	13.6	22.7	20.0
Rape	5.6	6.7	6.7	9.0	13.5	34.3
Robbery	1.4	2.2	2.3	3.7	6.2	7.3

Source: Virginia Criminal Sentencing Commission Annual Report 2001, pp 66–71. "No Prior" represents no previous violent crime records. "Prior $<$ 40" represents a previous conviction for a violent felony with a maximum penalty of less than 40 years. "Prior $>$ 40" represents a past conviction for a violent felony carrying a maximum penalty of 40 years or more microdata from Georgia and exploits the 1981 mass release in Georgia, a rare event, as a quasi-experiment to estimate how the elimination of discretionary prison release affects the social cost of crime. Similar to Levitt (1996) and Kuziemko (2013) finds that "longer prison terms decrease recidivism. The benefits of parole (the ability to ration prison resources based on recidivism risk and the creation of incentive) outweight the costs (lost incapacitation due to shorter prison terms)." She also argues that severely limiting the discretion of parole boards may leave some valuable information unused because parole boards "have access to information revealed after sentencing and therefore may be better than judges at forecasting inmates' expected recidivism risk." Shepherd (2002) checks the effect of TIS laws in deterring violent crimes using county-level data. The empirical results demonstrate that TIS laws could deter violent crimes through increasing both the probability of arrest and the maximum imposed prison sentences. She specifies that TIS laws decrease rates of murder by 16%, rape by 12%, robbery by 24%, aggravated assault by 12%, and larceny by 3%. She also finds that under the TIS framework offenders tend to substitute to commit more property crimes such as burglaries and auto thefts for less severe punishments.

In this paper, we contribute to the literature by measuring whether the justice reform deters violent crimes in Virginia. The reason why we concentrate on Virginia is because, compared with other states, its justice reform is more thorough: It abolishes parole for all types of violent offenders and requires 85% of sentenced terms. We use a panel data approach proposed by Hsiao et al. (2012, HCW hereafter) to conduct a counterfactual analysis. Unlike the popular difference-in-differences (DID) approach, HCW method does not suffer from sample selection bias problem since the method does not require the treatment unit and the control units follow parallel paths over time in the absence of treatment (Abadie 2005; Athey and Imbens 2006). HCW suggest using outcomes of control units which do not receive treatment to predict the counterfactual path for the treated unit. The idea behind HCW method is that some common factors (they may be unobservable) affecting outcomes of both treatment and control units and that cross-sectional correlation are stable over time in the absence of treatment.

The rest of the paper is organized as follows. In Sect. 2 we briefly review the history of parole system in the USA. In Sect. 3 we describe HCW's method for estimating average treatment effects. Sect. 4 reports the empirical findings and a series of robustness checks. We conclude the paper in Sect. 5. Formal definitions for violent/property crimes are included in "Appendix A". We collect more detailed estimation results in "Appendix B" which is available from the authors upon request.

2 Parole and Truth-in-Sentencing in USA

Parole was introduced into USA in the 1800s and was mainly used to efficiently manage the population in prisons and prepare inmates for release.[1] By 1942, both the state and the federal governments have established their parole systems run by parole

[1] For details, the first chapter of *Parole: Then & Now* by Texas Senate Research Center provides an excellent reference.

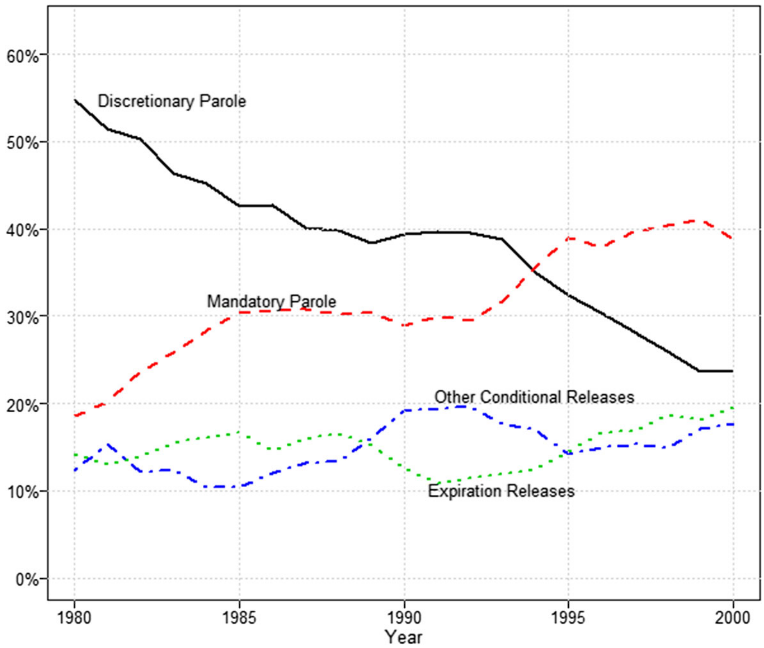

Fig. 1 Percentage of releases from state prison by method of release: 1980–2000. *Notes*: The "other" category includes those released due to over-crowding orders, those transferred to other states, those whose sentences were commuted or overturned, those who died during incarcerated, and those who escaped. *Source*: Reentry Trends in The US, Bureau of Justice Statistics

boards, whose discretionary power of releasing inmates was huge during the 1970s, a period in which judges only provided indeterminate sentences reflecting a range of minimum and maximum incarcerating years. Through structured decision-making process, a parole board might release an inmate as long as the offender served the minimum convicted sentence after evaluating his/her potential recidivism risk. For example, in Alaska, where the discretionary power of parole board still exists, the discretionary parole is defined as the following:

> *According to Alaska Stat. §33.16.900, "discretionary parole" means the release of a prisoner by the board before the expiration of a term, subject to conditions imposed by the board and subject to its custody and jurisdiction; "discretionary parole" does not include "special medical parole".*

The percentage of US prisoners released on parole reached a high level of 69% in 1977.[2] As shown in Fig. 1, even in the early 1980s, the discretionary paroles still account for more than half of all the prisoners released in the USA. To control the number of release on parole, in 1984, *The United States Federal Sentencing Guidelines* abolished parole for those committed federal crimes and limited early release from prison for good behavior on the federal level, as more and more states moved to determinate sentencing system and mandatory supervised release during the 1980s. This abolition narrowed the discrepancy between the sentenced years and the actual

[2] Trends in State parole, 1990–2000, Bureau of Justice Statistics, 2001.

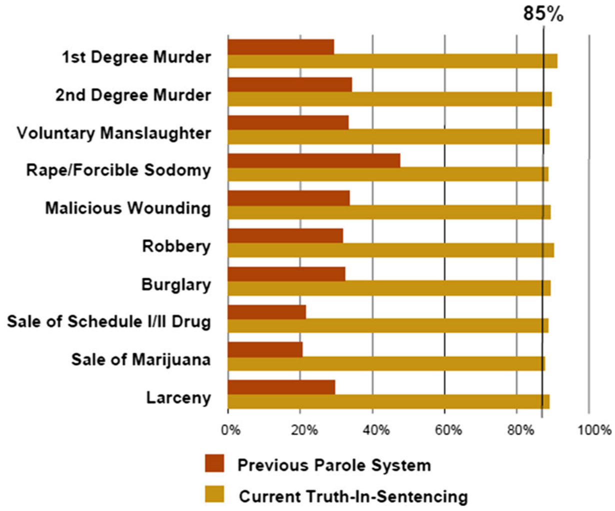

Fig. 2 Percentage of prison sentence served in Virginia. *Note*: The plot is adapted from *A Decade Truth-in-Sentencing in Virginia*, Virginia Sentencing Commission, 2004

years served. Consequently, as shown in Fig. 1, the percentage of discretionary parole continue to decline after 1980 while mandatory parole began to account for a larger fraction.[3] In 1990, the mean value of the maximum sentenced years for the most serious offense was 99 months and the actual mean serving time is 43.8% of the sentenced terms. By 1999, this percentage has increased to 55%. Also, in 1990, the percentages of served sentence for violent crimes (murder, manslaughter, rape, and robbery) are all less than 46%. But in 1999, all violent offenders have to serve more than 50% of their sentenced terms.[4]

To assure that felons serve a substantial portion of their sentences, the federal government launched Violent Offender Incarceration and Truth-in-Sentencing (VOI/TIS) Incentive Formula Grant Program. This program funds states to build or expand current correctional facilities for confinement of persons convicted of violent crimes, and the funded states should warrant violent felons to serve at least 85% of their sentenced terms. These measures led to a sharp decline in the percentage of discretionary paroles in the 1980s. The percentage of discretionary paroled prisoner declined to 38% in 1989 from 55% in 1980, while mandatory paroles increased from 19 to 30%.[5] The percentage of mandatory paroles surpassed that of discretionary paroles in 1994 and continued to increase. For Virginia, Fig. 2 compares the actual serving percentages of sentenced terms by various types of offenders before and after the justice reform. Before 1995, except for those who convicted of rape or forcible sodomy, offenders on

[3] According to Virginia Department of Correction, mandatory parole is "the automatic release of an offender six months before completion of his or her sentence." Unlike discretionary parole, parole board members might impose some special conditions for this type of parole but will not make the parole decision through voting.

[4] Table 5 of Trends in State Parole, 1990–2000, Bureau of Justice Statistics, 2001.

[5] Trends in State parole, 1990–2000, Bureau of Justice Statistics, 2001.

average served less than 40% of their sentences before being eligible for parole. After 1994, all offenders had to serve at least 85% of their sentences due to the TIS laws. We will then estimate whether enhancement in punishment and longer incarceration would effectively decrease violent crimes in Virginia.

3 Theoretical model

In this section, we briefly review the HCW estimation method. Following Hsiao et al. (2012), we assume that only the first unit, Virginia, receives the justice reform at time T_1, and all other units are not affected by the Virginia's policy intervention. Let y_{1t}^1 and y_{1t}^0 denote the violent crime rates of Virginia with and without the treatment, respectively. Given that there is an intervention at time T_1, we are interested in estimating the average treatment effects $\Delta_1 = E(y_{1t}^1 - y_{1t}^0)$. The difficulty is that we cannot observe y_{1t}^0 for $t \geq T_1 + 1$. Let $\hat{y}_{1t}^0$ be a generic estimator of y_{1t}^0. Then We estimate Δ_1 by

$$\hat{\Delta}_1 = \frac{1}{T_2} \sum_{t=T_1+1}^{T} \left(y_{1t} - \hat{y}_{1t}^0 \right). \tag{1}$$

We now discuss HCW's method for estimating the counterfactual outcome y_{1t}^0. In the absence of any treatments, Hsiao et al. (2012) consider the case that

$$y_t = a + Bf_t + u_t, \tag{2}$$

for $t = 1, \ldots, T_1$, where $y_t = (y_{1t}, \ldots, y_{Nt})'$, $a = (a_1, \ldots, a_N)'$, f_t is a $K \times 1$ vector of common factors (they may be unobservable) that affect crime rates, B is a $N \times K$ matrix of factor loading, $u_t = (u_{1t}, \ldots, u_{Nt})'$ is a vector of idiosyncratic error. Hsiao et al. (2012) suggest using control group's y_{jt}, $j \geq 2$, to estimate y_{1t}^0. This can be done by replacing f_t by $\tilde{y}_t = (y_{2t}, \ldots, y_{Nt})'$ in the first unit's equation $y_{1t} = a_1 + b_1' f_t + u_{1t}$ to obtain

$$y_{1t} = \gamma_1 + \tilde{y}_t' \gamma + v_{1t} \tag{3}$$

for $t = 1, \ldots, T_1$, where v_{1t} satisfies $E(v_{1t}) = 0$ and $E(v_{1t}\tilde{y}_t) = 0$. Let $\hat{\gamma}_1$ and $\hat{\gamma}$ denote the least square estimators of γ_1 and γ based on (3) using the pre-treatment period data, then we estimate the counterfactual outcome of y_{1t}^0 by

$$\hat{y}_{1t}^0 = \hat{\gamma}_1 + \tilde{y}_t' \hat{\gamma} \tag{4}$$

for $t = T_1 + 1, \ldots, T$. The average treatment effect is estimated by

$$\hat{\Delta}_1 = \frac{1}{T_2} \sum_{t=T_1+1}^{T} \left(y_{1t} - \hat{y}_{1t}^0 \right), \tag{5}$$

where $T_2 = T - T_1$.

Under quite mild conditions including that $rank(B) = K$ and that the data is a weakly dependent stationary process, Li and Bell (2017) derive the asymptotic distribution of $\hat{\Delta}_1$ as follows:

$$\sqrt{T_2}(\hat{\Delta}_1 - \Delta_1) \xrightarrow{d} N(0, \Sigma),$$

where $\Sigma = \Sigma_1 + \Sigma_2$. $\Sigma_1 = \eta E(x_t)' V E(x_t)$, $\eta = \lim_{T_1, T_2 \to \infty} T_2/T_1$, V is the asymptotic variance of $\sqrt{T_2}(\hat{\beta} - \beta)$, $\beta = (\gamma_1, \gamma')'$, $\hat{\beta} = (\hat{\gamma}_1, \hat{\gamma}')'$, Σ_2 is the asymptotic variance of $T_2^{-1/2} \sum_{t=T_1+1}^{T} (\Delta_{1t} - E(\Delta_{1t}) + v_{1t})$.

In this paper, we will use the above method to estimate how the justice reform affected the violent crimes in Virginia after 1995. However, since our data shows non-stationary behavior, we cannot use Li and Bell's (2017) asymptotic result to do inference. We will use placebo method to conduct inference. As we mentioned earlier, using HCW's method to estimate average treatment effects has the advantage that it does not require outcomes of treated units and the control units to follow parallel paths in the absence of treatments.

4 Empirical results

4.1 Data

For a state to be included in the control group, we require that the crime rate in this state is not affected by Virginia's justice reform (the treatment), and no similar treatment occurs during the whole sample period in that state. Sabol et al. (2002) summarize different states' policies for parole and early release.[6] Based on Sabol et al. (2002), we select 17 states as potential control units which still keep parole system for certain offenders and have not established stringent TIS laws.[7] They are Alabama, Arkansas, Colorado, Hawaii, Maryland, Massachusetts, Nebraska, Nevada, New Hampshire, New Mexico, South Dakota, Rhode Island, Texas, Utah, Vermont, West Virginia, and Wyoming. Some of them require certain minimum incarcerating periods. For example, Texas and Maryland demand all felons to serve at least 50% of their sentences while Arkansas requires certain offenders to serve 70%. Colorado separates violent offenders by the number of time for prior violent convictions: felons with two prior violent convictions to serve 75% and with one prior violent conviction, 56%. Some even still keep discretionary power of correctional committee for parole. For example, Rhode Island still grants discretionary parole on inmates who have been imprisoned for more than 6 months and who have served no less than one-third of sentenced terms (Table 2).

We collect both violent and property crime rates per 100,000 population from FBI's Uniform Crime Report. The data cover the period between 1960 and 2010. Table 3

[6] For details, see page 20, Chapter 2 of *The Influences of Truth-in-Sentencing Reforms on Changes in States' Sentencing Practices and Prison Populations*.

[7] Utah does not have Truth-in-Sentencing statutes but received federal grant funding on the basis of its Truth-in-Sentencing practices.

Table 2 Median years property offenders served in Virginia

	FY 1993	FY 1999–FY 2001		
	Prior > 40	No prior	Prior < 40	Prior > 40
Burglary	2.2	1.8	3.6	5.4
Larceny	1.3	1.1	1.8	2.3
Motor Vehicle Theft	1.3	1.3	1.8	2.7

Source: Virginia Criminal Sentencing Commission Annual Report 2001, pp 66 - 71. "No Prior" represents no previous violent crime records. "Prior < 40" represents a previous conviction for a violent felony with a maximum penalty of less than 40 years. "Prior > 40" represents a past conviction for a violent felony carrying a maximum penalty of 40 years or more. The medians for burglary, larceny, and motor vehicle theft during 1993 are from Sridharan et al. (2003). Their calculations are based on unpublished data maintained by the Sentencing Commission

Table 3 Descriptive statistics: violent/property crime rates (1960–2010)

	Aggregated violent crime rates				Aggregated property crime rates			
	Min	Max	Mean	SD	Min	Max	Mean	SD
US	72.48	324.81	209.60	70.08	1726.3	5353.3	3896.61	1028.98
Virginia	42.47	182.38	126.66	37.70	1469.20	4349.10	3144.75	792.60
Alabama	38.36	228.66	141.82	55.23	985.50	4521.40	3276.42	1117.66
Arkansas	36.64	191.36	114.34	42.37	926.40	4581.70	3113.49	1079.20
Colorado	74.15	223.01	149.06	41.47	2035.10	6821.40	4601.98	1438.82
Hawaii	15.85	233.65	117.77	55.32	2276.50	7182.80	5010.10	1282.62
Maryland	49.99	491.42	316.29	117.07	1518.80	5777.70	4143.38	1085.49
Massachusetts	26.71	301.96	161.75	70.42	1170.30	5635.30	3460.32	1217.11
Nebraska	22.01	113.91	80.24	26.21	1177.80	4162.50	3151.82	882.39
Nevada	95.35	547.79	305.23	111.14	2774.70	7996.00	5142.41	1419.44
New Hampshire	7.09	80.68	46.42	22.15	676.40	4499.80	2452.77	986.28
New Mexico	55.60	237.42	157.98	51.66	2062.40	6053.20	4593.11	1174.94
Rhode Island	15.38	157.47	103.74	40.59	1833.30	5524.10	3778.88	1091.71
South Dakota	14.97	91.11	45.63	19.78	1065.20	3116.30	2179.93	572.42
Texas	48.03	355.17	199.60	77.65	1970.50	7365.10	4596.80	1426.20
Utah	28.63	118.26	84.27	25.07	2047.90	5762.00	4297.66	1013.13
Vermont	4.87	72.19	35.60	15.50	796.70	5115.00	2793.35	1169.34
West Virginia	20.49	80.67	55.95	17.02	599.20	2639.90	1914.02	642.95
Wyoming	24.93	82.72	50.54	13.80	1564.70	4701.80	3285.44	822.14

Source: FBI's Uniform Crime Report (UCR). The violent crimes include murder, rape, and robbery. The property crimes include burglary, larceny, and motor vehicle theft

displays descriptive statistics for both national- and state-level violent and property crime rates. We find that, even though lower than the national average level, the average violent crime rate in Virginia is only lower than that in Colorado, Maryland, Nevada, New Mexico, and Texas, but higher than that of other states in the control group. For property crime, Virginia is not significantly lower than the other states in the control

Table 4 Descriptive statistics: three types of violent crimes rates (1960–2010)

	Murder/Manslaughter				Forcible rape				Robbery			
	Min	Max	Mean	SD	Min	Max	Mean	SD	Min	Max	Mean	SD
US	4.6	10.2	7.34	1.82	9.4	42.8	28.86	9.82	58.30	272.70	173.40	60.04
Virginia	4.6	12.3	7.87	1.92	8.0	32.1	22.53	6.22	25.6	142.0	96.27	31.78
Alabama	5.6	16.0	10.64	2.57	5.7	41.2	25.69	9.64	19.1	185.8	105.50	46.88
Arkansas	4.7	12.0	8.3	1.7	6.1	49.7	28.8	12.5	21.9	135.6	77.2	30.6
Colorado	2.4	8.3	5.20	1.42	12.9	53.1	37.98	10.81	53.8	174.1	105.88	34.76
Hawaii	1.6	8.7	3.89	1.86	0.8	44.6	24.34	11.17	10.7	190.2	89.54	45.22
Massachusetts	1.4	4.4	3.09	0.81	4.50	36.10	22.25	9.44	20.4	270.9	136.42	63.28
Maryland	4.5	12.7	9.3	1.9	7.2	46.4	29.3	10.7	37.3	434.7	277.7	105.7
New Hampshire	0.6	3.6	1.93	0.78	2.10	44.40	20.85	13.28	3.0	42.0	23.65	10.57
New Mexico	5.4	13.3	9.05	2.17	12.2	63.5	41.23	15.36	37.8	171.4	107.71	36.78
Nebraska	1.5	4.4	3.01	0.69	3.30	36.80	20.97	9.34	15.5	91.0	56.26	18.72
Nevada	5.5	20.0	10.48	3.19	8.00	73.80	44.23	18.29	74.0	460.6	250.51	92.63
Rhode Island	0.8	4.9	2.99	1.01	1.7	46.9	19.81	12.65	12.5	132.0	80.93	32.29
South Dakota	0.6	4.6	2.19	1.03	5.3	69.9	27.16	18.20	8.3	31	16.29	4.64
Texas	5.0	16.9	10.29	3.55	9.30	55.00	34.32	13.90	31.1	286.5	154.99	62.64
Utah	1	4.8	2.70	0.79	6.8	47.5	26.87	12.39	20.3	86.5	54.39	16.25
Vermont	0.3	5.5	2.07	1.02	2.3	40.8	19.54	9.20	1.9	38.9	13.98	7.79
West Virginia	2.2	7.4	5.00	1.24	4.2	25.8	14.64	6.66	11.7	50.2	36.31	11.12
Wyoming	1.4	10.3	4.42	2.13	3.6	35.4	22.82	9.15	12.4	53.3	23.31	11.21

Source: FBI's Uniform Crime Report (UCR). Formal definition for each violent crime is included in "Appendix A"

group. Table 4 exhibits rates of three types of violent crime: murder, rape and robbery. It shows that Virginia's average murder rate over the 51 years is higher than the national level, and the fluctuation is also relatively larger. Rates for rape and robbery are both lower than the national average levels.

4.2 Aggregated violent crime

We check the treatment effect on the aggregated violent crime rate first. Implementing the HCW method, we regress the violent crime rate in Virginia before 1995 on violent crime rates of various combinations of the 17 states in the control group. For the 17 states, there are $2^{17} - 1 = 131071$ different combinations. We categorize these combinations into 17 groups by the number of the control states selected, and then pick up one "optimal" model from each of the 17 groups by comparing their adjusted R^2. For the 17 selected models, we finally choose the one gives the smallest AICC value.[8]

[8] We have also used the AIC standard for state selections and actually get quite similar results. AICC is a more conservative model selection standard and prefers more parsimonious model. Thus, in the remaining part of the paper, the optimal models are all selected under AICC.

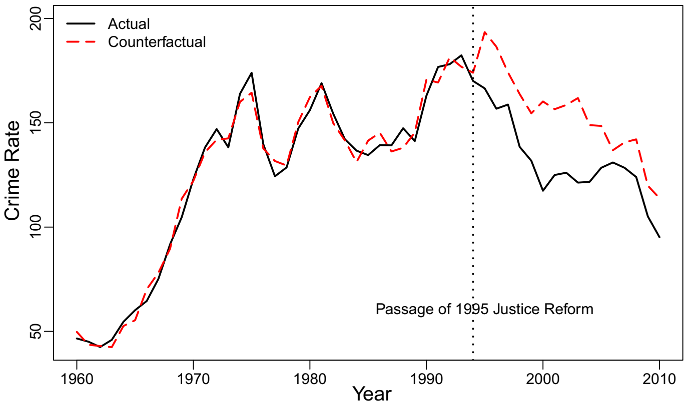

Fig. 3 HCW method: actual and counterfactual paths of violent crime in VA. *Note*: The selected control states are: Alabama, Arkansas, Colorado, Maryland, Nevada, Rhode Island, Utah and West Virginia. The estimated average treatment effect is −23.83

Based on HCW approach, the AICC method selects eight states from the control group: Alabama, Arkansas, Colorado, Maryland, Nevada, Rhode Island, Utah, and West Virginia. Using the violent crime rates in these eight states as the covariates, the estimated ATE on violent crime in Virginia is −23.83 per 100,000 population, or a 16% drop after the TIS reform. These states are geographically scattered. Figure 3 shows that a linear combination of the selected eight states fits the actual path of the violent crime rate in Virginia quite well for data between 1960 and 1995 (in-sample fit). The actual path (solid line) and the counterfactual path (dashed line) begin to diverge abruptly after the passage of the TIS laws. Figure 3 shows that counterfactual path jumps at 1995 and is above the actual path from then on.

Detailed regression results are documented in Table B.1 of the supplementary "Appendix B". However, the severities of the three crimes included in the aggregated violent crime are quite different as documented in Table 4; the aggregated violent crime is mainly comprised by robbery, while murder and rape are relatively rare.[9] Therefore, it is necessary to check the heterogeneity of the treatment effect on the three types of crime.

4.3 Treatment effect on three types of violent crime

In this section, we check the treatment effect on rape, robbery and murder separately. The formal definitions for them are included in "Appendix A". Figure 4a implies that the pre-treatment paths are well fitted for all the three types of violent crime and we could observe clear divergence between the actual and the counterfactual paths for robbery and rape after 1995. For rape, five states—Alabama, Hawaii, Maryland, Nebraska, and West Virginia—are selected. The average of the estimated rape rate per 100,000 population from 1995 to 2010 is 25.88, while the average of the actual rape

[9] We thank one referee for pointing this out.

rate per 100,000 population is 24.14. The estimated average treatment effect is −1.74 or a −7% drop. However, the deterrence on rape is not significant immediately after the TIS: The actual and the counterfactual paths begin to diverge after about 2000, indicating the lagging impact of TIS on rape.

For robbery, six states are included: Alabama, Maryland, Nevada, New Mexico, Utah, and Vermont. The adjusted R^2 coefficient is 0.98, implying a good fit to the actual path. The average treatment effects are −13.01 or −12% lower after the TIS reform, as shown in Fig. 4b. According to Fig. 4c, the treatment effect on murder rate is also very obvious, as the counterfactual path is well above the actual path between 1995 and 2010. The mean value of estimated treatment effect is −1.69 or −23%. However, the adjusted R^2 is only 0.59, which is significantly smaller than that in rape and robbery. Figure 4c displays that the main variation is during the 1960s. During this period, the unobserved common factors should impact the five states in the control group and Virginia in quite different manners. But after the 1960s, the fitted path generally follows the actual path of murder rate and obvious deviations become fewer. The detailed regression results for all the three types of violent crime are included in the supplementary "Appendix B." Thus, as can be seen, the −23.83 or 16% decline in the aggregated violent crime rate is mainly driven by the decrease in robbery, which involves violent threats such as firearms, knifes, or fists according to the definition of UCR. In the long run, the justice reform in Virginia mainly deterred robbery, which exhibits relatively less severity than murder and rape do.

4.4 Robustness checks

To check the robustness of the treatment effect, we conduct several robustness checks. First, we assume the treatment had happened earlier than 1995 to check whether the detected treatment effect is driven by statistical coincidence. Second, following Bai et al. (2014), we explicitly introduce a time trend variable on the right-hand side of Eq. (3) to reduce the near multicollinearity concern and examine whether a time trend would affect our conclusion. We then examine the robustness of our estimations by conducting placebo tests and incorporating several economic and demographic covariates which may impact crime.[10]

4.4.1 Out-of-sample prediction

We assume that the justice reform had happened 10 years earlier and rerun the regression model specified in Eq. (3) using the new pre-treatment data. If the results are robust, we should not observe significant treatment effect between 1985 and 1994. Figure 5 indicates that a remarkable deviation between the actual and the counterfactual paths still emerges only after 1994, even though we assume that the treatment had happened 10 years earlier. According to Fig. 5, we still obtain a good fit during the pre-1985 period. During the 1985–1994 period, the counterfactual path follows the pattern of the actual path, and the mean value of the estimating errors during this

[10] We thank a referee for the suggestion of conducting the add-covariates robustness check.

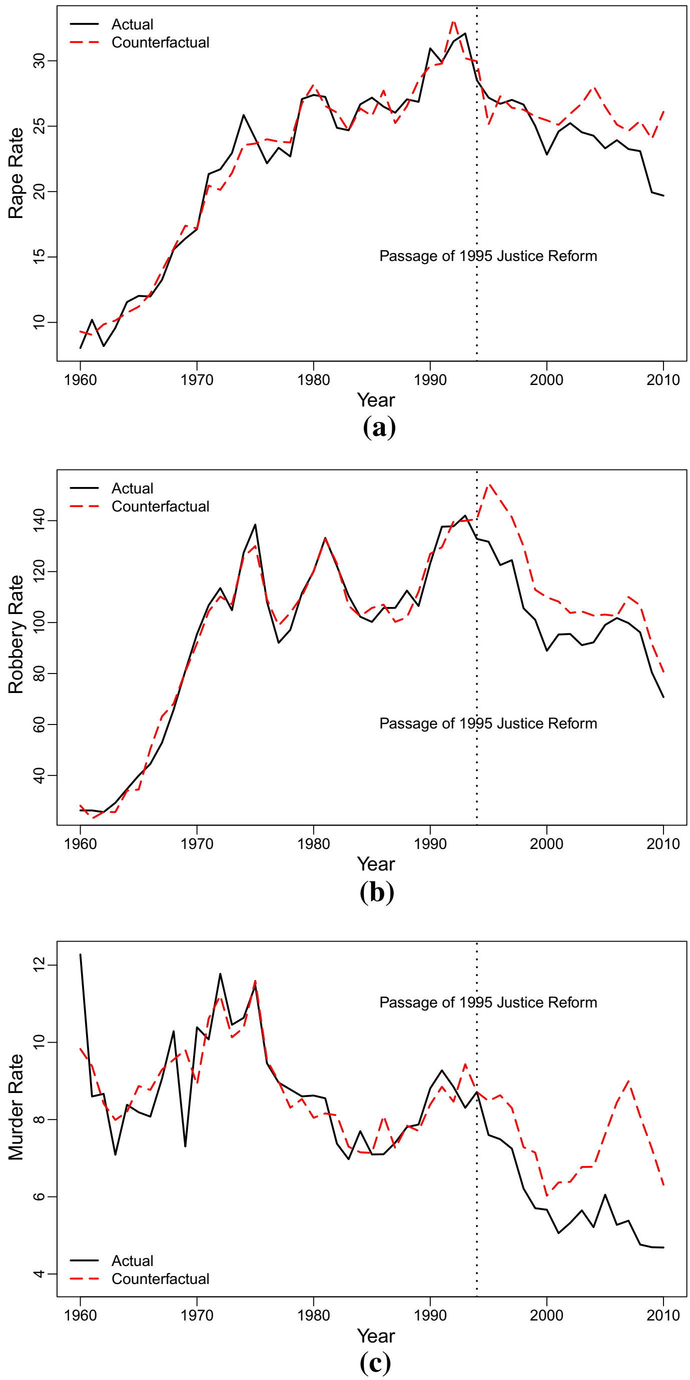

Fig. 4 HCW method: actual and counterfactual paths of rape, robbery and murder in VA. *Note*: The estimated average treatment effect on the reported violent crime rates for murder, rape, and robbery are −1.74, −12.46, and −1.48, respectively

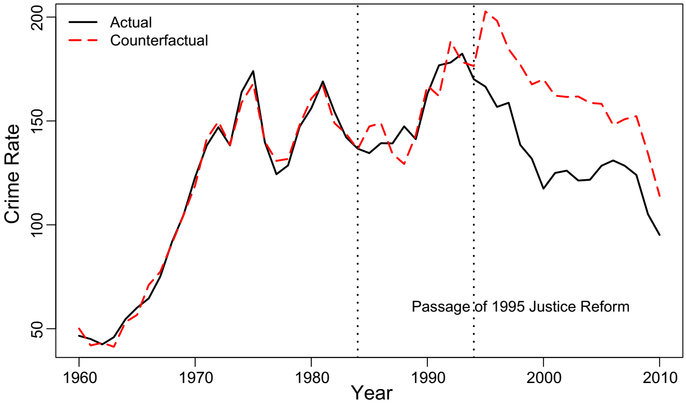

Fig. 5 HCW method: actual and counterfactual paths of violent crime in VA assuming the TIS was passed in 1985. *Note*: The pre-treatment observations are from 1960 to 1984. The selected control states are: Alabama, Arkansas, Colorado, Maryland, New Mexico, Nevada and Utah. The average predicting error between 1985 and 1994 is −0.29. The estimated average treatment effect during 1995 and 2010 is −32.92, respectively

period is −0.29. After 1994, the counterfactual path exhibits an abrupt jump and is above the actual path during the post-treatment period, with the mean of the estimated 16 years' treatment effect equals to −32.92. Again this result supports that the decline of violent crime in Virginia is not a statistical coincidence. The detailed regression results and the specific treatment effect information are given by Table B.9 and Table B.10 in the supplementary "Appendix B."

4.4.2 Time trend

Bai et al. (2014) extend Hsiao et al. (2012) to $I(1)$ process and prove that HCW's approach still gives consistent estimate of the average treatment effects. If both y_{1t} and $\tilde{y}_t$ in Eq. (3) are $I(1)$ process and v_{1t} is $I(0)$, y_{1t} and $\tilde{y}_t$ are cointegrated. However, since some or all components in $\tilde{y}_t$ may contain drift terms, these series will be dominated by their non-zero drift terms. Table 5 shows the results of unit root tests on the series of crime in VA before 1995.[11] It shows that all the series of crime are $I(1)$ processes. The unit root test further excludes the existence of unit root in the residuals of the regression of y_{1t} on $\tilde{y}_t$. Therefore, y_{1t} and $\tilde{y}_t$ are cointegrated. One way to estimate the cointegrated model is to add a time trend regressor to capture the time trend components of the $I(1)$ regressors. After adding the time trend, the estimated ATE on violent crime is −20.68, which is also quite close to the main finding (−23.83) in Sect. 4.2. The detailed regression results are displayed in Table B.1 and Table B.2 in the supplementary "Appendix B."

[11] We thank one referee for pointing this out.

Table 5 Unit root test

	Violent crime	Rape	Robbery	Murder	Property crime
A: Unit root test results for series of crime rates pre-1995					
None	0.5178	1.3292	0.3899	−0.4577	0.4292
p value	(0.608)	(0.193)	(0.699)	(0.650)	(0.671)
Drift	−1.9003*	−1.4867	−1.9455*	−1.916*	−2.0988**
p value	(0.067)	(0.148)	(0.061)	(0.065)	(0.044)
Trend	−2.087**	−1.4688	−2.1934**	−1.9018*	−1.6895
p value	(0.046)	(0.153)	(0.036)	(0.067)	(0.102)
B: Unit root test results for series of HCW regressions					
ADF statistic	−5.534***	−3.8777***	−5.169***	−4.7215***	−5.8778***
p value	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)

*** Significant at 1%, ** significant at 5%, * significant at 10%. The null hypothesis of the unit root test is that the tested series is $I(1)$. "none" indicates that neither an intercept nor a trend is included in the test regression. "drift" indicates that an intercept is added. "trend" indicates that both an intercept and a trend is added. The p values are based on Hamilton (1994) and Dickey and Fuller (1981)

4.4.3 Placebo test

As shown in Table 5, all the series of crime are $I(1)$ processes. Our data sample size is not large enough to ensure the reliable use the asymptotic theory (in our case, $T_1 = 35$ and $T_2 = 16$). Therefore, to check that whether our findings are driven by statistical coincidence, we conduct a series of placebo tests, which are similar to Abadie et al. (2010). Specifically, we shift Virginia to the control unit and then artificially assume that each state in the control group passed the TIS laws in 1995. We carry out the HCW method repeatedly and obtain 18 synthetic paths. The calculated pre-treatment MSE for violent crime in Virginia is 24.59. However, we also find that some states have poor pre-treatment fits, which make the interpretation to the corresponding post-treatment prediction less convincing. For example, for the aggregated violent crime, the pre-treatment MSE in Maryland is about 788 (as opposed to 24.59 in Virginia). The largest MSE for Maryland does not come as a surprise. According to Table 3, Maryland has the highest average violent crime rate, indicating that there is no combination of states in the control group that can reproduce the path of violent crime in Maryland prior to 1995. Similar results arise for other states with extreme violent crime rates before the passage of the TIS laws.

Therefore, we exclude states with pre-treatment MSEs of more than 2 times the MSE of Virginia, a strategy which is similar to Abadie et al. (2010, p 509). By doing this, we exclude 6 states and then draw the difference (gap) between the actual and the synthetic paths. Figure 6 displays the gap obtained from the placebo studies. In Fig. 6, the gray lines represent the gaps of aggregated violent crime in other 11 control states, while the black line denotes the gap of aggregated violent crime in Virginia. Figure 6 shows that the deterrent effect on violent crime in Virginia becomes the lowest for the aggregated violent crime. This result is consistent with the main finding in Sect. 4.2.

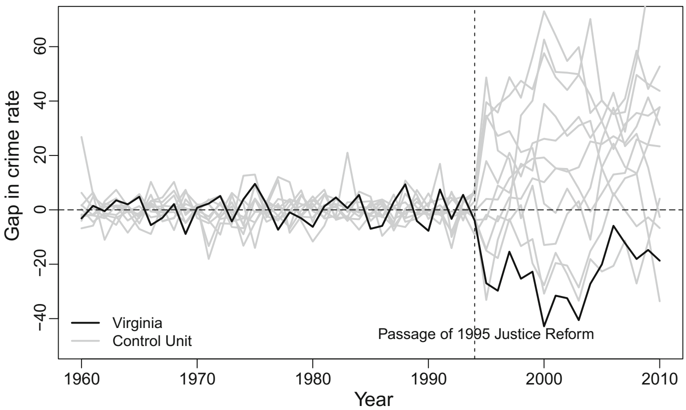

Fig. 6 HCW method: violent crime gaps in VA and placebo gaps in control states. *Note*: states with pre-treatment period's MSE 2 times higher than Virginia's are excluded

4.4.4 State specific covariates

Hsiao et al.'s (2012) ATE estimation method was recently extended to the case of incorporating additional covariates by Li and Lin (2017). To further check the robustness of our main findings, we collect covariates which may also impact the crime rate.[12] Specifically, we collect the following control variables to represent the deterrence measures, economic situation and demographic impact on crime: the number of police officer per 1000 population (Pol), median of household's real income ($HHinc$), unemployment rate ($Urate$), population (Pop), percentage of white and percentage of African-American ($White$ and $Black$).[13]

To check the robustness of the main findings, we add these six covariates to Eq. (3) and estimate the following model using the pre-treatment period data

$$y_{1t} = \gamma_1 + \tilde{y}_t'\gamma + x_{1t}'\beta + v_{1t} \tag{6}$$

for $t = 1, 2, \ldots, T_1$, where $x_{1t} = (Pol_{1t}, HHinc_{1t}, Urate_{1t}, Pop_{1t}, White_{1t}, Black_{1t})'$, and $\beta = (\beta_1, \ldots, \beta_6)'$ are the coefficients associated with each of the control variable (population and income are in logarithm). We assume that these added covariables are exogenous to the treatment, i.e., TIS in Virginia did not *significantly* affect these additional covariates. Then we can predict the counterfactual crime rate for Virginia for the post-treatment period in the absence of TIS by

$$\hat{y}_{1t}^0 = \hat{\gamma}_1 + \tilde{y}_t'\hat{\gamma} + x_{1t}'\hat{\beta} \tag{7}$$

for $t = T_1 + 1, \ldots, T$, where $\hat{\gamma}_1$, $\hat{\gamma}$ and $\hat{\beta}$ are the least squares estimators of γ_1, γ and β based on equation (Add covariates 1).

[12] We thank one referee for pointing this out.

[13] The sources of those data sets are FBI, Bureau of Labor Statistics and Census Bureau.

However, it is arguable that the number of police officer per 1000 population may not be exogenous to TIS because government could use the acquired federal funding to recruit more police officers. To address this concern, we further regress the number of police officer per 1000 population in Virginia to those in the control states:

$$Pol_{1t} = \delta_1 + \widetilde{Pol}'_t \delta + e_{1t}, \quad t = 1, \ldots, T_1. \tag{8}$$

Let $\hat{\delta}_1$ and $\hat{\delta}$ be the OLS estimates of δ_1 and δ from the above equation, we then replace Pol_{1t} by $\hat{\delta}_1 + \widetilde{Pol}'_t \hat{\delta}$ in Eq. (6). And the predicted counterfactual Virginia crime rate is $\hat{y}^0_{1t} = \hat{\gamma}_1 + \tilde{y}'_t \hat{\gamma} + \hat{x}'_{1t} \hat{\beta}$ for $t = T_1 + 1, \ldots, T$, where $\hat{x}_{1t}$ is the same as x_{1t} except that Pol_{1t} is replaced by $\hat{\delta}_1 + \widetilde{Pol}'_t \hat{\delta}$.

After adding these covariates, the estimated ATE on violent crime becomes −23.96, which is very close to the main finding of −23.83 in Sect. 4.2. Detailed regression result is documented in Table B.13 in the supplementary "Appendix B."

4.4.5 Observable common factors

We further check whether some other observable factors which are common to all these states affect the crime rates.[14] Specifically, we collect three variables: annual GDP growth rate in USA (GDP), US real disposable income per capita ($Dinc$, in 2009 dollars), and unemployment rate in USA ($Unemploy$). Since all the three variables represent the macroeconomic situation for the whole country, it is reasonable to assume that they should be exogenous to the TIS laws in Virginia.

Since these factors are common to both the treatment and the control states, adding them into our model means that we could rewrite the Eq. (3) as

$$y_{1t} = \gamma_1 + \tilde{y}'_t \gamma + x'_t \theta + v_{1t}, \tag{9}$$

for $t = 1, 2, \ldots, T_1$, and $x_t = (GDP_t, Dinc_t, Unemploy_t)'$ and $\theta = (\theta_1, \theta_2, \theta_3)$. Let $\hat{\gamma}_1, \hat{\gamma}$ and $\hat{\theta}$ be the OLS estimates of γ_1, γ and θ. Then the predicted counterfactual crime rates in Virginia in the absence of TIS could be estimated by

$$\hat{y}^0_{1t} = \hat{\gamma}_1 + \tilde{y}'_t \hat{\gamma} + x'_t \hat{\theta}, \tag{10}$$

for $t = T_1 + 1, \ldots, T$. After adding these three common factors, the estimated ATE becomes −21.44 using (5) with $\hat{y}^0_{1t}$ given in (10). The result is, once again, close to our main finding of −23.83 in Sect. 4.2. This indicates that $\tilde{y}_t$ incorporates most of the common factors which impact the treatment unit. Detailed regression results are documented in Table B.18 of the supplementary "Appendix B."

[14] We thank one referee for pointing this out.

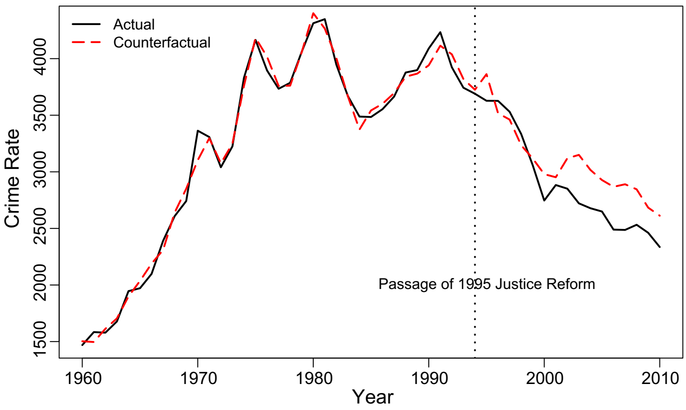

Fig. 7 HCW method: property crime

4.5 Property crime

Even though the justice reform in Virginia targets on violent offenders, it is also meaningful to examine the response of non-violent property crime offenders. Levitt (1998) decomposes the reduction in crime rate into two channels. The first one is incapacitation: Tougher punishment leads to fewer crimes due to longer imprisonment. We have observed such an incapacitation effect in violent crimes as all murder, rape, and robbery rate declined abruptly after 1994. The second one is deterrence: Severe punishment on one kind of crime will lead to a rise in another crime as offenders substitute away from the former. Table 2 compares the medians of actual serving time for burglary, larceny, and motor vehicle theft in the year 1993 and in the period of 1999–2001. Changes in punishment for property crimes are relatively small compared with that for violent crimes.

Literatures have some evidence about the substitution from severely punished violent crimes to less stringently penalized property crimes. Using data on all counties in the USA, Shepherd (2002) finds that burglaries and auto thefts increase by 20 and 15% respectively after the enactment of TIS laws. As a matter of fact, sociological evidence (see Shafer 1999) shows that for those who have experienced the sentencing system, many have learned from the path and thus realized that the less severe punishments on property crimes would not deter them from committing the same crimes in the future. However, when we impose more severe punishments on felons who also plan to engage in property crimes, we might also see a decline in property crimes as a by-product. Thus, the overall anticipated effect of the justice reform on property crimes is not clear in literatures.

Figure 7 demonstrates that the average property crime rate dropped after the introduction of the justice reform. However, the story is not that straightforward: At least during the four years between 1995 and 1998 after the passage of the justice reform, the actual property crime rate in Virginia was not significantly different from its counterfactual counterpart. Specifically, during 1996–1998, the property crime rate per 100,000 population increased by about 130 on average. It is from 1999 that the actual and the counterfactual path for property crime rate began to diverge with each other. This indicates that the actual property crime rate in Virginia declined four years after the justice reform. This finding is consistent with the theory of substitution to less severely penalized property crime as documented in Levitt (1998) and Shepherd (2002). However, even though non-violent offenders did not change their behaviors drastically after the reform in 1995, as time goes by, felons who plan to commit property crime are incapacitated to do so due to the longer serving time in jails. This indicates some lagged treatment effect of the justice reform on property crime, so that the actual property crime rate became lower than it would have been in the absence of the justice reform.

The overall ATE on property crime during 1995–2010 is -194.74, indicating that after the initial substitution from violent crime to property crime in the first few years, property crime rate became lower. Detailed regression results for property crime, as well as the corresponding robustness check, are included in the supplementary "Appendix B."

5 Conclusion

In this paper we employ a panel data approach to evaluate the average treatment effect of Virginia's 1995 justice reform on violent crime as well as the non-violent property crime. Empirical results show that the average treatment effect of the justice reform on violent crimes is abrupt and significant, but the decline is mainly driven by the decrease in robbery. A series of robustness check further confirm our findings. A closer examination on three types of violent crime—rape, robbery, and murder—detects deterrent effect on robbery and murder. Treatment effect on property crime began to take hold four years after 1995. This lagging impact is consistent with criminological theories, which indicate that some violent offenders substitute to less severely penalized property crime.

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