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Shall-Issue Laws and the Structure of Crime Data

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1. Introduction and Topic

The impact of firearms and their availability upon crime has been an extremely controversial topic in the social science literature, and there still does not appear to be any consensus regarding what the true impact of firearms may be. This issue is compounded in the case of controversial so-called shall-issue laws, which direct local law enforcement officers to issue permits allowing the concealment of handguns to citizens who meet certain criteria. The State of Ohio passed such a shall-issue concealed carry law on April 8, 2004. Here, I will consider the possible impact of that law in the context of the continuing debate in the Economic literature.

2. Previous Research and Literature Review

The debate on gun control in general, and shall-issue laws in particular, was shaped significantly by the so-called 'more guns less crime hypothesis' advocated by John Lott and David Mustard in their influential 1997 paper: *Crime, Deterrence, and Right-to-Carry Concealed Handguns*[1], and by Lott's quasi-popular book on that grew from the paper, *More Guns, Less Crime*[2]. Lott and Mustard, using Uniform Crime Report data, attempt to show that the presence of a shall-issue law reduces the natural log of crime rates. They find that the presence of shall-issue laws reduces the amount of violent crime but increases the amount of stealth-oriented property crime. Their paper, as the first to attempt to sway the argument by using panel data techniques, changed and reignited the debate in the Economic literature. In fact, as noted by Benson(1999), "…the focus of the policy debate on gun control has changed. It used to be that the gun-control advocates could claim without question that guns are a major source of

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[1] John Lott and David Mustard. Crime, Deterrence, and Right-to-Carry Concealed Handguns. The Journal of Legal Studies, Vol. 26, No. 1, (Jan., 1997), pp. 1-68.

[2] John Lott. *More Guns, Less Crime*. University of Chicago Press. 1998.

violence. Today these same advocates are forced to assert that the deterrence impacts of law-abiding citizens carrying concealed handguns are small relative to what Lott's evidence suggests, as they scramble to support their assertions in a manner that can look credible when compared to Lott's research." [3]

Several groups of writers quickly moved to respond to Lott and Mustard's controversial result. Perhaps the earliest such critics to emerge were Zimring and Hawkins(1997)[4], who argued that Lott's findings were distorted by the pre-existing prevalence of handguns in the United States, the reduction of which, they argued, would be more beneficial than the adoption of shall-issue laws. Alschuler(1997)[5] argues that even if Lott's claims regarding the efficacy of shall-issue laws is valid, policymakers would be well advised against adopting such policies, as they are likely to increase the amount of total violence, even if much of that violence is against criminals. Black and Nagin(1998)[6] present evidence that the conclusions of Lott and Mustard are not robust across a wide array of possible specifications, showing specifically that an inordinate amount of the observed variation in violent crime is caused by the data on Florida, and that the impact in the other states is inconsistent with the results from Florida. They also observe that if the time-series is de-aggregated the effect for the individual years since adoption can differ widely. Ludwig(1998)[7] noted that since there is a minimum age in order to obtain a permit even when the most liberal shall-issue policies are in effect, one would expect to find that the benefits

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[3] Bruce L. Benson. Review: John R. Lott, Jr., "More guns, less crime: Understanding crime and gun-control laws. Chicago: University of Chicago Press, 1998. x + 225 pages. $23.00 (cloth)". Public Choice. Volume 100, Numbers 3-4 September, 1999. 309-313.

[4] F.E. Zimring and G. Hawkins. "Concealed Handguns: The Counterfeit Deterrent." *The Responsive Community* 7(2):46–60. (1997).

[5] Albert W. Alschuler. "Two Guns, Four Guns, Six Guns, More Guns: Does Arming the Public Reduce Crime?" 31 Valparaiso University Law Review 365-73. 1997.

[6] D. Black, and D. Nagin. "Do 'Right to Carry' Laws Reduce Violent Crime?" *Journal of Legal Studies* 27(1):209 –219. (1998).

[7] Jens Ludwig. "Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data." International Review of Law and Economics Volume 18, Issue 3, September 1998, Pages 239-254.

of the policy were in some manner concentrated amongst people who were of age to hold the permits. To the contrary, however, he found that there were actually adverse effects among those age groups. Ayres and Donohue(1999)[8] observe that using a more precise definition of Lott's dummy for the presence of a shall-issue law, using the incarceration rate in place of the arrest rate, and failure to account for the issues resulting from the growth of crack-related crime in the 1990s all weaken Lott and Mustard's analysis, and when the models are re-estimated considering these factors the impact of the law is much diminished or even insignificant. In their 2003[9] paper extending this analysis, Ayres and Donohue held that updating the dataset with an additional five years also demonstrated the lack of robustness of Lott and Mustard's results, and even reversed their findings. Duggan(2001)[10] found that the presence of a shall-issue law was statistically insignificant and that the prevalence of guns in an area, which he measured by the rate of subscription to gun-oriented periodicals, correlated with increased incidence of crime. Maltz and Targonski(2002)[11] criticized the UCR dataset itself, noting that the way in which gaps in the data were handled changed in the middle of the sample, and that the data were inaccurate for many localities. This led to a debate in the literature between these authors and Lott and Whitley, who defended their use of the UCR data[12] by arguing that the errors in the data were centralized in low-population rural counties, and that this error is somewhat mitigated by the population weighting mechanism they included in their initial study. They go on to re-estimate their model

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[8] Ian Ayres and John J. Donohue III. "Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy." American Law and Economics Review, Oxford University Press, vol. 1(1-2), pages 436-70

[9] Ian Ayres and John J. Donohue III. "Shooting down the "More Guns, Less Crime" Hypothesis." Stanford Law Review, Vol. 55, No. 4 (Apr., 2003), pp. 1193-1312

[10] Mark Duggan. "More Guns, More Crime." The Journal of Political Economy, Vol. 109, No. 5. Oct. 2001. Pp.1086-1114.

[11] Michael D. Maltz and Joseph Targonski. "A Note on the Use of County-Level UCR Data." Journal of Quantitative Criminology, Vol. 18, No. 3, September 2002.

[12] John Lott and John Whitley. Measurement Error in County Level UCR Data. Journal of Quantitative Criminology, Vol. 19, No. 2, June 2003.

including only states that were unaffected by the measurement errors, and conclude that their findings were indeed robust against these errors. Maltz and Targonski(2003), however, are not convinced by this analysis, and claim that it is not possible for the effects of the shall-issue law to be as pronounced as Lott argues, because of the interplay of many policies designed to combat crime and regression to the mean. They also argue that Lott and Whitley's techniques, while mitigating the error, do not eliminate it.[13]

Numerous other authors have come forward to support the results obtained by Lott and Mustard for a variety of reasons. Bartley and Cohen(1998)[14] show that the positive impact of shall-issue laws is robust against the time-based criticism of Black and Nagin through the use of extreme bound analysis. Moody(2001)[15] was skeptical of some of the analysis presented by Lott and Mustard, and re-estimated the model to ensure it was robust against mis-specification, omitted variables, and second-order bias in the t-ratios. He found that the results were robust against these concerns, although in some specifications the results were less pronounced than initially claimed by Lott and Mustard. Plassmann and Tideman(2001)[16] offer support for Lott's hypothesis in the form of a Poisson model, which they find confirms the positive impact of the shall-issue law in many states, although they find evidence of mixed or even negative results in

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[13] Michael D. Maltz and Joseph Targonski. Measurement and Other Errors in County-Level UCR Data: A Reply to Lott and Whitley. Journal of Quantitative Criminology, Vol. 19, No. 2, June 2003.

[14] William Alan Bartley and Mark A. Cohen. "The Effect of Concealed Weapons Laws: An Extreme Bound Analysis." *Economic Inquiry*, 1998, vol. 36, issue 2, pages 258-65

[15] Carlisle Moody. "Testing for the Effects of Concealed Weapons Laws: Specification Errors and Robustness." Journal of Law and Economics, Vol. 44, No. 2, Part 2, Guns, Crime, and Safety: A Conference Sponsored by the American Enterprise Institute and the Center for Law, Economics, and Public Policy at Yale Law School (Oct., 2001), pp. 799-813.

[16] Florenz Plassmann and T. Nicolaus Tideman. "Does the Right to Carry Concealed Handguns Deter Countable Crimes? Only a Count Analysis Can Say." Journal of Law and Economics, Vol. 44, No. 2, Part 2, Guns, Crime, and Safety: A Conference Sponsored by the American Enterprise Institute and the Center for Law, Economics, and Public Policy at Yale Law School (Oct., 2001), pp. 771-798

others. Benson and Mast(2001)[17] showed that the Lott-Mustard hypothesis was robust with respect to concerns regarding privately-provided security, and that such security could not have been a source of the conjectured effects of the shall-issue laws. Helland and Tabarrok(2004)[18] attempted to re-estimate the Lott and Mustard model using a probability density function generated from placebo laws. Using this approach, they found that while they are less certain of the results than the original authors, there is still much to commend the 'more guns, less crime' hypothesis, especially as it regards the substitution away from violent crime into property crime.

Other researchers, instead of siding with either Lott or his critics, took a middle stance. Kahan and Braman(2003, 2005)[19],[20] criticize more directly the entire project of analyzing gun control empirically, and put forth an alternative 'cultural theory' for examining the issue. Olson and Maltz(2001)[21] found that while the presence of a shall-issue law was likely to reduce pre-meditated violence, it would also increase the chance that unforeseen bouts would result in serious gun-related injuries. They also stressed the importance of examining the effects of the shall-issue law on different groups of individuals, as the impact would not be homogenous. Kovandzic and Marvell(2003)[22] argue that while their analysis does not show that the shall-issue

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[17] Bruce L. Benson and Brent D. Mast. "Privately Produced General Deterrence." Journal of Law and Economics, Vol. 44, No. 2, Part 2, Guns, Crime, and Safety: A Conference Sponsored by the American Enterprise Institute and the Center for Law, Economics, and Public Policy at Yale Law School (Oct., 2001), pp. 725-746

[18] Eric Helland and Alex Tabbarrok. "Using Placebo Laws to Test 'More Guns, Less Crime'." The B.E. Journal of Economic Analysis & Policy, Berkeley Electronic Press, vol. 0(1). 2004.

[19] Dan M. Kahan and Donald Braman. "More Statistics, Less Persuasion: A Cultural Theory of Gun-Risk Perceptions." *University of Pennsylvania Law Review*, Vol. 151, No. 4 (Apr., 2003), pp. 1291-1327

[20] Donald Braman, et al. "Modeling Facts, Culture, and Cognition in the Gun Debate." *Social Justice Research, Vol. 18, No. 3, September 2005*

[21] David E. Olson and Michael D. Maltz. "Right-to-Carry Concealed Weapon Laws and Homicide in Large U. S. Counties: The Effect on Weapon Types, Victim Characteristics, and Victim-Offender Relationships." Journal of Law and Economics, Vol. 44, No. 2, Part 2, Guns, Crime, and Safety: A Conference Sponsored by the American Enterprise Institute and the Center for Law, Economics, and Public Policy at Yale Law School (Oct., 2001), pp. 747-770

[22] Tomislav V. Kovandzic and Thomas B. Marvell. "Right-to-Carry Concealed Handguns and Violent Crime: Crime Control Through Gun Decontrol?" Criminology & Public Policy Volume:2 Issue:3 July 2003 Pages:363 to 396

law is particularly significant with respect to crime, it may still be beneficial if it helps to alleviate the regulatory burden of policing otherwise law-abiding gun owners. Levitt(2004)[23] suggested that the impact of shall-issue laws was negligible next to the profound impact of increases in police, prison population, demographic trends, and the legalization of abortion.

However, the debate is far from settled. Salvoes of papers between Moody and Marvell[24,25] and Ayres and Donohue[26,27] continue to be traded in the literature, and the American public is still deeply divided on questions pertaining to the proper role of government regulation with respect to firearms. It is for these reasons that I hope to be able to make some contribution to the literature by considering the case of Ohio, which adopted a shall-issue law much more recently than the states considered in the original analysis of Lott and Mustard, and even more recently than the 2003 analysis of Ayres and Donohue. It is my hope that the recent nature of this enactment will serve to shed light on the possible impact of shall-issue laws on the incidence of crime.

3. Data and Methods

I made use of FBI Uniform Crime Report data on Ohio over the period 1960-2007 to conduct my analysis of four time series. Though these data have been criticized by some authors, they are the standard in the literature, and are the most comprehensive series available for the United States. Further, much of the issue raised concerning the UCR data was regarding the county-level data specifically, and my analysis concerns itself with state-level series. I

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[23] Steven D. Levitt. "Understanding Why Crime Fell in the 1990s: Four Factors That Explain the Decline and Six That Do Not." The Journal of Economic Perspectives, Vol. 18, No. 1 (Winter, 2004), pp. 163-190

[24] Carlisle E. Moody and Thomas B. Marvell. "The Debate on Shall-Issue Laws." *Econ Journal Watch, Volume 5, Number 3,September 2008, pp 269-293.*

[25] Carlisle E. Moody and Thomas B. Marvell. "The Debate on Shall-Issue Laws, continued." Econ Journal Watch Volume 6, Number 2 May 2009, pp 203-217

[26] Ian Ayres and John J. Donohue III. "Yet Another Refutation of the More Guns, Less Crime Hypothesis – With Some Help From Moody and Marvell." Econ Journal Watch Volume 6, Number 1 January 2009, pp 35-59

[27] Ian Ayres and John J. Donohue III. "More Guns, Less Crime Fails Again: The Latest Evidence from 1977 – 2006." Econ Journal Watch Volume 6, Number 2 May 2009, pp 218-238

considered four crimes central to the analysis presented by Lott and Mustard: murder, assault, rape, and burglary. However, contrary to the analysis that has been done on the topic in the past, I employed structural break analysis to consider if the structure of the series changed during the period of analysis, 2004-2005, when the Ohio shall-issue law went into effect. This type of analysis is recommended to address policy concerns by Phiel et al.(2003)[28] and Lee and Suardi(2008)[29], as finding evidence of a structural break at the date of a policy implementation would be convincing evidence that the policy had had a real impact, while failure to find a break in the series would provide evidence that the policy was not particularly effective in this regard. I modeled all four of the series using the methodology prescribed by Box and Jenkins(1976)[30], which is standard in the time-series literature for the specification of an Autoregressive and Integrated Moving Average (ARIMA) process. Useful discussions of the model and modeling methodology are given by Kennedy(2008)[31] and Hamilton(1994)[32], which are excellent general references. In order to normalize the time series, and to help guard against heteroskedasticity, I transformed the series by adjusting them to be incidences of the given crime per 100,000 Ohio residents and took the natural logarithm of each series. The series are plotted in figure 1. The incidence of burglary, rape, and assault seem to be generally increasing over the period of analysis, although burglary seems to have declined noticeably following a peak in 1981. The incidence of murder, by contrast, behaves much more erratically than do the other series.

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[28] A. Piehl, S. Cooper, A. Braga and D. Kennedy. (2003). "Testing for Structural Breaks in the Evaluation of Programs." *Review of Economics and Statistics*, 85, pp. 550-558.

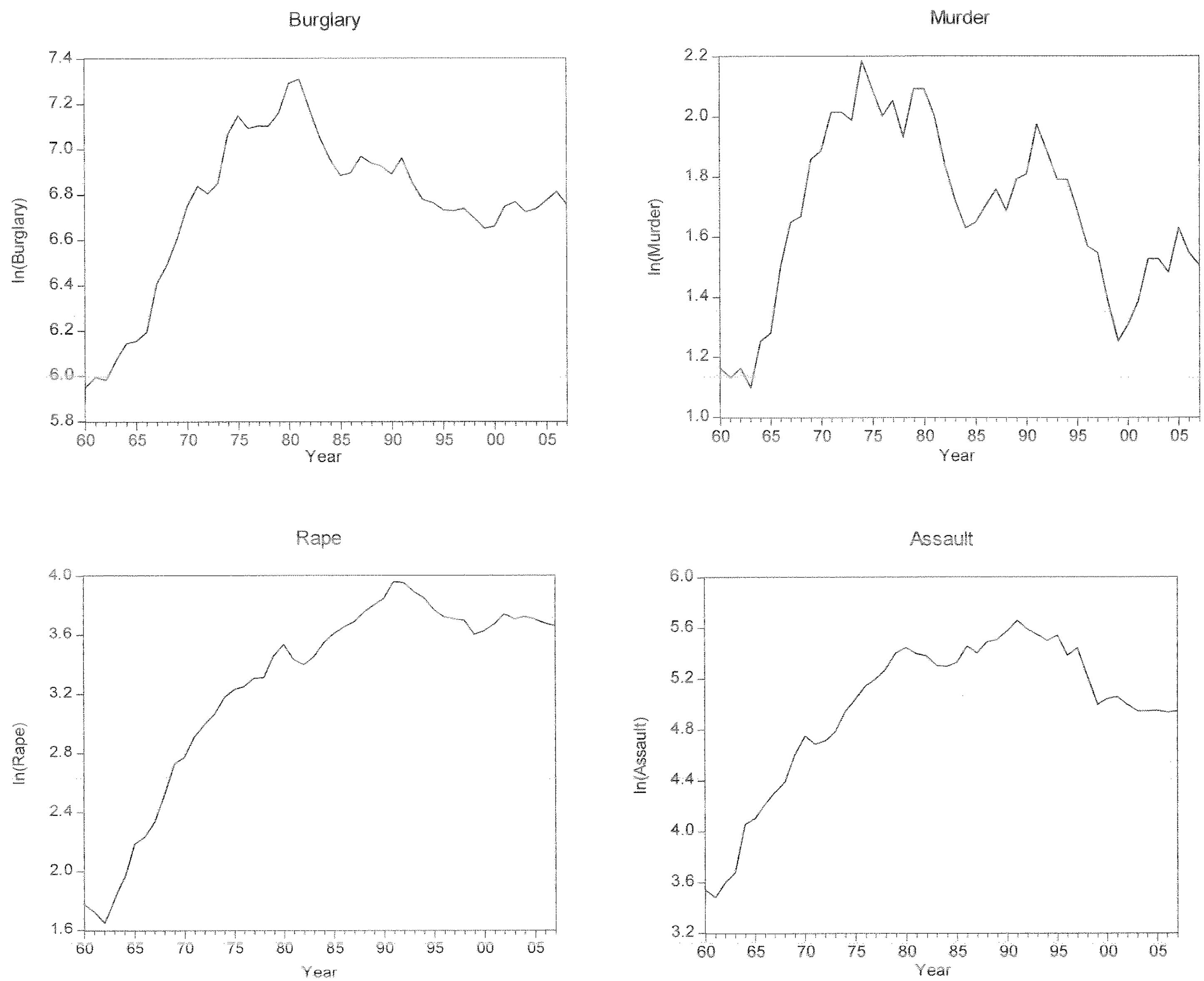
[29] Wang-Sheng Lee and Sandy Suardi. "The Australian Firearms Buyback and Its Effect on Gun Deaths." Melbourne Institute Working Paper Series wp2008n17, Melbourne Institute of Applied Economic and Social Research, The University of Melbourne. 2008.

[30] G. Box and G. Jenkins. *Time Series Analysis, Forecasting and Control.* San Francisco: Holden Day. 1976.

[31] Peter Kennedy. *A Guide to Econometrics, 6e.* Blackwell Publishing. 2008.

[32] James D. Hamilton. *Time Series Analysis.* Princeton University Press. 1994.

Figure 1: Plots of the Series



Note: These plots make use of the number of instances of the given crime per 100,000 Ohio residents.

4. Analysis

First, I consider the stationarity of the series, as the presence of a unit root in a time series, if not properly identified and controlled for, will cause any estimation to give unreliable results. A unit root occurs when one is a root of the system's characteristic polynomial. Such an occurrence will cause the series to be non-stationary, which is to say that considering lagged or leading values of the series will change the nature of the probability distribution. Specifically, as was famously shown by Granger and Newbold(1974)[33], failure to account for a unit root will artificially inflate test statistics, providing statistically significant results with no underlying meaning. Fortunately, the unit root can be corrected by differencing the equations, a process which reduces the available number of data points, but does not otherwise harm the sample properties. For the purpose of testing for a unit root, I employ Augmented Dickey-Fuller (ADF) Test[34], and the Phillips and Perron(1988) (PP) Test[35], both common in the time series literature. However, as noted by Perron(1998)[36], failure to account for a structural break can cause these tests to falsely identify a unit root when the series is actually stationary around such a break. Since the object of this paper is to analyze the series with respect to possible structural breaks, I also consider the Zivot and Andrews(1992)[37] (ZA) test, which was designed specifically to allow for an endogenous break in a trend function, an intercept term, or in both. The ZA test "…test is a sequential test which utilizes the full sample and uses a different dummy variable for each possible break date. The break date is selected where the t-statistic from the ADF test of unit root

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[33] C. W. J. Granger and P. Newbold (1974). "Spurious regressions in econometrics". *Journal of Econometrics* **2**: 111–120.

[34] S.E. Said and David A. Dickey (1984), "Testing for Unit Roots in Autoregressive Moving Average Models of Unknown Order.", Biometrika, 71, p 599–607.

[35] P.C.B Phillips and P. Perron (1988), "Testing for a Unit Root in Time Series Regression.", *Biometrika*, 75, 335–346

[36] P. Perron. (1989). "The Great Crash, the Oil Price Shock and the Unit Root Hypothesis." *Econometrica*, 57, pp. 1361-1401.

[37] E. Zivot and D. Andrews. (1992). "Further Evidence on the Great Crash, the Oil-Price Shock, and the Unit-Root Hypothesis." *Journal of Business & Economic Statistics*, 10, pp. 251-270.

is at a minimum (most negative). Consequently a break date will be chosen where the evidence is least favorable for the unit root null."[38] Thus, the ZA test can also be used as a method to endogenously select likely structural breaks in series considered to be trend stationary. The results of these tests are given in table 1. I chose the lag lengths for the ADF test based on the Akaike(1974)[39] Information Criterion (AIC), and the bandwidth for the PP test according to the method suggested by Newey and West(1994)[40]. When the series are estimated with only an intercept term, there is disagreement with respect to the presence of a unit root in several series. The ADF and PP tests find no evidence of a unit root in the cases of assault and rape, but their results are opposed in the case of burglary where the ADF test finds no evidence of a unit root but the PP test cannot reject the null hypothesis that one exists. In the case of murder, both tests find evidence of integration. For all four series, the ZA test finds evidence that a unit root may be present. However, when the series are modeled with both an intercept term and a trend function all of three of the tests agree in failing to reject the null hypothesis of an integrated series, providing considerable and robust evidence that the series posses a unit root. Since the series all show evidence of integration when modeled with both a trend and an intercept, and since the ZA test which was designed to consider cases similar to the topic of this study finds evidence of integration in all cases, I proceed on the presumption that all of the series posses a unit root, in accordance with the null. The series appear to be characterized by first-order integration, because after one difference is taken, the correlograms display no further evidence of any integration.

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[38] J. Glynn, N. Perera, and R. Verma. "Unit Root Tests and Structural Breaks: A Survey with Applications." *Journal of Quantitative Methods for Economics and Business Administration*. vol. 3(1), pages 63-79, June 2007.

[39] Hirotugu Akaike (1974). "A New Look at the Statistical Model Identification". *IEEE Transactions on Automatic Control* 19 (6): 716–723.

[40] W. Newey and E. West. "Automatic Lag Selection in Covariance Matrix Estimation." *Review of Economic Studies*, 61, 631-653. 1994.

Table 1: Results of the Unit Root Tests

Intercept + Trend	ADF	PP	ZA
Murder	-1.870283	-1.937132	-3.870
	(0)	(3)	(0)
	[.6539]	[.6194]	[1966]
Assault	-.0324933	-0.982709	-2.572
	(9)	(7)	(0)
	[.9869]	[.9367]	[1974]
Burglary	-2.32748	-1.745299	-3.901
	(2)	(0)	(2)
	[.4112]	[.7149]	[1974]
Rape	-0.490267	-.572489	-2.963
	(0)	(3)	(0)
	[.9806]	[.9761]	[1968]
Intercept			
Murder	-1.829430	-1.993240	-3.797
	(0)	(4)	(0)
	[.3621]	[.2887]	[1966]
Assault	-3.747048	-3.167743	-2.348
	(1)	(2)	(0)
	[.0064]	[.0284]	[1996]
Burglary	-3.602316	-2.533835	-3.913
	(6)	(2)	(2)
	[.0100]	[.1142]	[1967]
Rape	-3.576620	-3.576620	-2.481
	(0)	(0)	(0)
	[.0100]	[.0100]	[1963]

Note: Figures in ( ) for the ADF and PP tests are the AIC-based selected lag length and the Newey-West selected bandwidth respectively. Figures in [ ] are p-values. The Zivot and Andrews (ZA) (1992) test has a null hypothesis of a unit root with no break, and an alternative hypothesis of stationarity with a single break. The figure in [] under ZA test denotes a break date, while the figure in () denotes the number of lags determined to be marginally significant at the .10 level. The 1%, 5% and 10% critical values for the ZA test with break(s) in intercept (intercept and trend) are -5.34 (-5.57), -4.80 (-5.08), and -4.58 (-4.82) respectively.

Having found no evidence of higher-order integration in the series, it can be presumed that the first differences are now stationary. The differenced series are plotted as growth rates after being scaled up by multiplication by one hundred. The resulting series are plotted in figure 2. It is difficult to determine if there exist any structural breaks in the series from these plots, due to their volatile nature. Therefore, a formal analysis of the series to test for the presence of such breaks will be conducted after ARIMA models are specified for the series.

In order to conduct an analysis of possible breaks in the series, the series must be formally estimated, and the specification of the model to be estimated has an important impact on the reliability of the results obtained. Here, I will consider univariate time series both because they lend themselves to this modeling, and because they are extremely useful when considering variables such as crime for which the structural models are not clear. The optimal specification of the ARIMA(p,d,q); where p is the number of lags of the autoregressive process, d is the order of integration, and q is the number of lags of the moving average process; is evaluated by attempting to minimize the value of the AIC or the Schwartz(1978)[41] Bayesian Information Criterion (BIC) while also considering the parsimony of the proposed specification in the face of systems with many minima. The results of these specification diagnostics and the estimates of the resulting models are given in table 2. Fortunately, the AIC and BIC selected specifications were the same in each case, indicating robustness against misspecification. The growth rate of burglary was estimated as a (0,1,1) process, and the growth rate of rape as a (1,1,0) process. The growth rate of assault was modeled as a (5,1,0) process. The growth rate of murder was estimated as a random walk because although a (1,1,1) process minimized the information criteria, that specification did not have white noise errors. In order to check for the presence of

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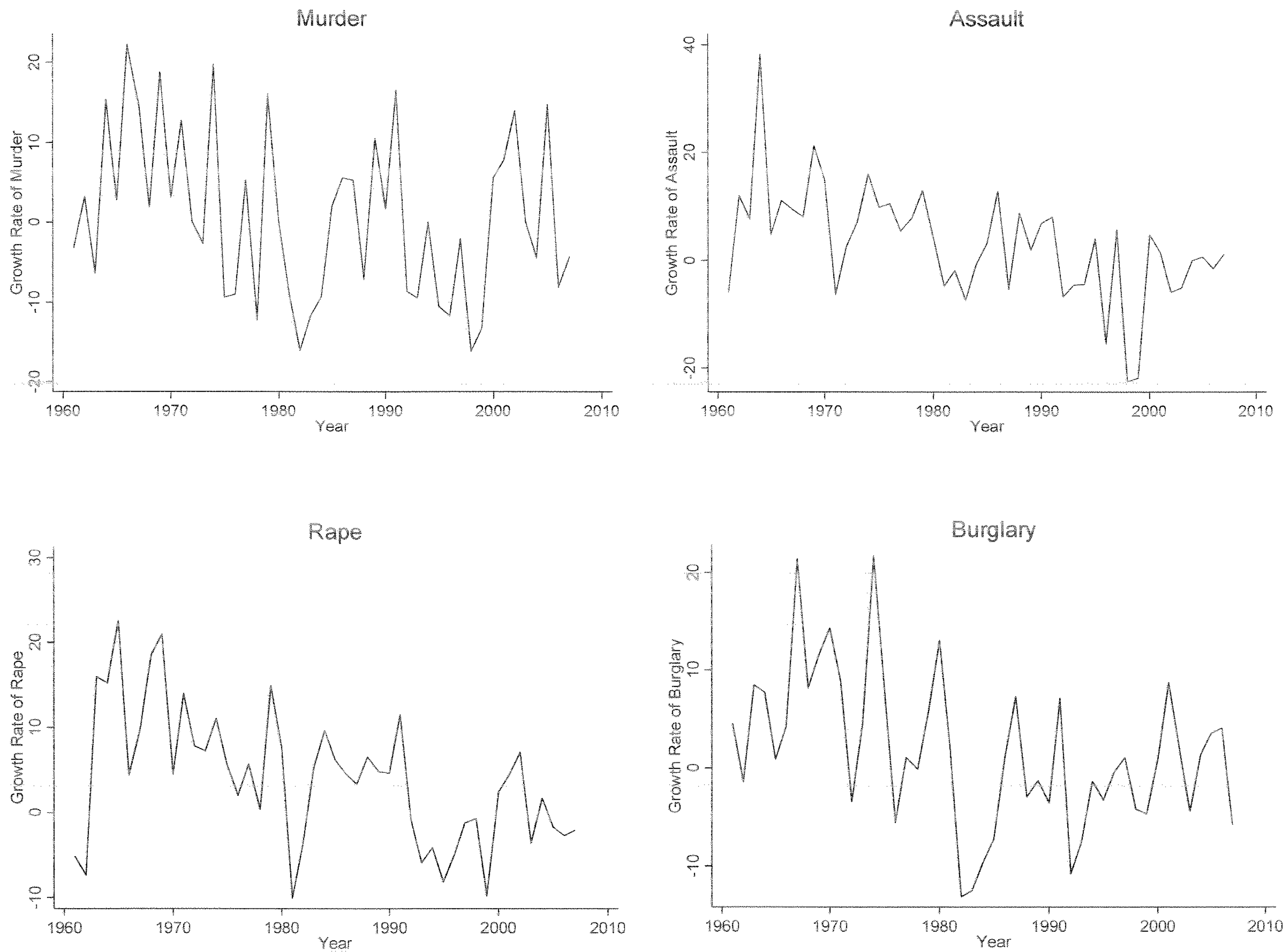
[41] Gideon E. Schwartz (1978). "Estimating the Dimension of a Model". *Annals of Statistics* 6 (2): 461–464.

Figure 2: Plots of the Series Growth Rates



Note: These plots make use of the number of instances of the given crime per 100,000 Ohio residents.

Table 2: ARIMA Model Selection and Estimation

Series	Specification	AIC	BIC	DW
Murder	(0,1,0)	7.560746	7.600111	1.743760
Assault	(5,1,0)	7.237440	7.320187	1.608966
Burglary	(0,1,1)	6.744215	6.822944	1.933174
Rape	(1,1,0)	6.780279	6.859785	2.041015

Note: The values given (p,d,q) for the model specification represent the number of included lags of the AR process, the order of integration of the series, and the number of included lags of the MA process; respectively.

	Coefficient	Std. Error	P-Value
Murder			
Constant	.725374	1.530871	.6379
Assault			
Constant	1.376119	1.975192	.4900
AR(5)*	.298437	.126001	.0228
Burglary			
Constant	1.630227	1.537930	.2948
MA(1)**	.532543	.126904	.0001
Rape			
Constant*	4.270328	1.972430	.0359
AR(1)**	.474550	.131211	.0008

Note: The standard errors given are heteroskedastity-robust. * denotes significance at the 5% level, and ** denotes significance at the 1% level.

white noise errors, I examined the correlogram of the residuals of each estimation, to be sure that they were free of any significant lags. Also given are the Durbin and Watson(1951)[42] (DW) test statistics for the presented specification. While most of the values are reasonably close to the desired value of 2, the DW statistic is known to underestimate the presence of autocorrelation in the residuals of ARIMA models. Although some researchers have employed the Ljung and Box(1978)[43] test for this purpose, the Monte-Carlo study of Hall and McAleer(1989)[44] demonstrated that those test statistics were not reliable either, and therefore Kennedy recommends a qualitative approach to the problem of residual autocorrelation through the examination of the correlogram of the residuals. The estimated autoregressive or moving average processes, where present, are all significant, most extremely so. The intercept term for the regression concerning the growth rate of rape was also significant.

When attempting to test for the presence of a structural break in the series, it is necessary to trim the sample by some factor in order to calculate the estimates, because there must be at least some data on either side of a given point in order to reasonably determine if a break is present at that point. Since the UCR series I consider have only 47 observations, it is not possible to employ the standard 15% trimming advocated by Andrews(1993)[45], indeed, I limit myself to trimming 5% of the data so that I can still consider the period of interest 2004-2005. The use of the 5% trimming is common in the time series literature when dealing with periods of interest that are still rather recent. For the purpose of identifying structural breaks in the series without

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[42] J. Durbin and G.S. Watson. (1951) "Testing for Serial Correlation in Least Squares Regression, II." *Biometrika* 38, 159–179.

[43] G. Ljung and G. Box. (1978). "On a Measure of a Lack of Fit in Time Series Models." *Biometrika*, 65, pp. 297-303.

[44] A.D. Hall and Michael McAleer. 1989. "A Monte Carlo Study of Some Tests of Model Adequacy in Time Series Analysis," Journal of Business & Economic Statistics, American Statistical Association, vol. 7(1), pages 95-106, January.

[45] D. Andrews. (1993). "Test for Parameter Instability and Structural Changes with Unknown Change Point." *Econometrica*, 61, pp.821-856.

choosing potential breaks *a priori*, I employ the Quandt(1960)[46] test, based on the Chow(1960)[47] statistic. The Quandt test attempts to identify endogenous structural breaks by searching for the highest Chow statistic over all possible break dates. This has the effect of splitting the sample into two subsamples around the hypothesized break date, and then calculating a Wald(1943)[48] statistic. The Wald statistic compares how much the residual sum of squares (RSS) has changed when the two sub-samples are considered alongside the entire sample. The results of this test are plotted in figure 3. These Wald statistics are distributed according to a non-standard distribution, the critical values at the five percent level of this distribution were calculated by Andrews to be 9.84 with one restriction and 12.93 with two restrictions. Inspection of the plots indicates that the growth rate of murder never reached its 9.84 critical value, and that there is therefore no evidence of a break in that series. The growth rate of assault has possible breaks during the periods 1980-1981, and 1992-1993; while the growth rate of burglary has possible breaks during 1966, 1976, 1995, 1999, and 2002-2003. The growth rate of rape, meanwhile, has possible breaks in 1975, 1981, and 1992. The maxima of the Wald statistics are given in table 3. Tellingly, while there is evidence for breaks in three of the four series, there is no evidence of a break in any of the series during the period of interest. Thus, I must conclude that the Quandt test fails to find any evidence that the 2004 Ohio shall-issue law had any impact on the rate of incidence of the four crimes considered here.

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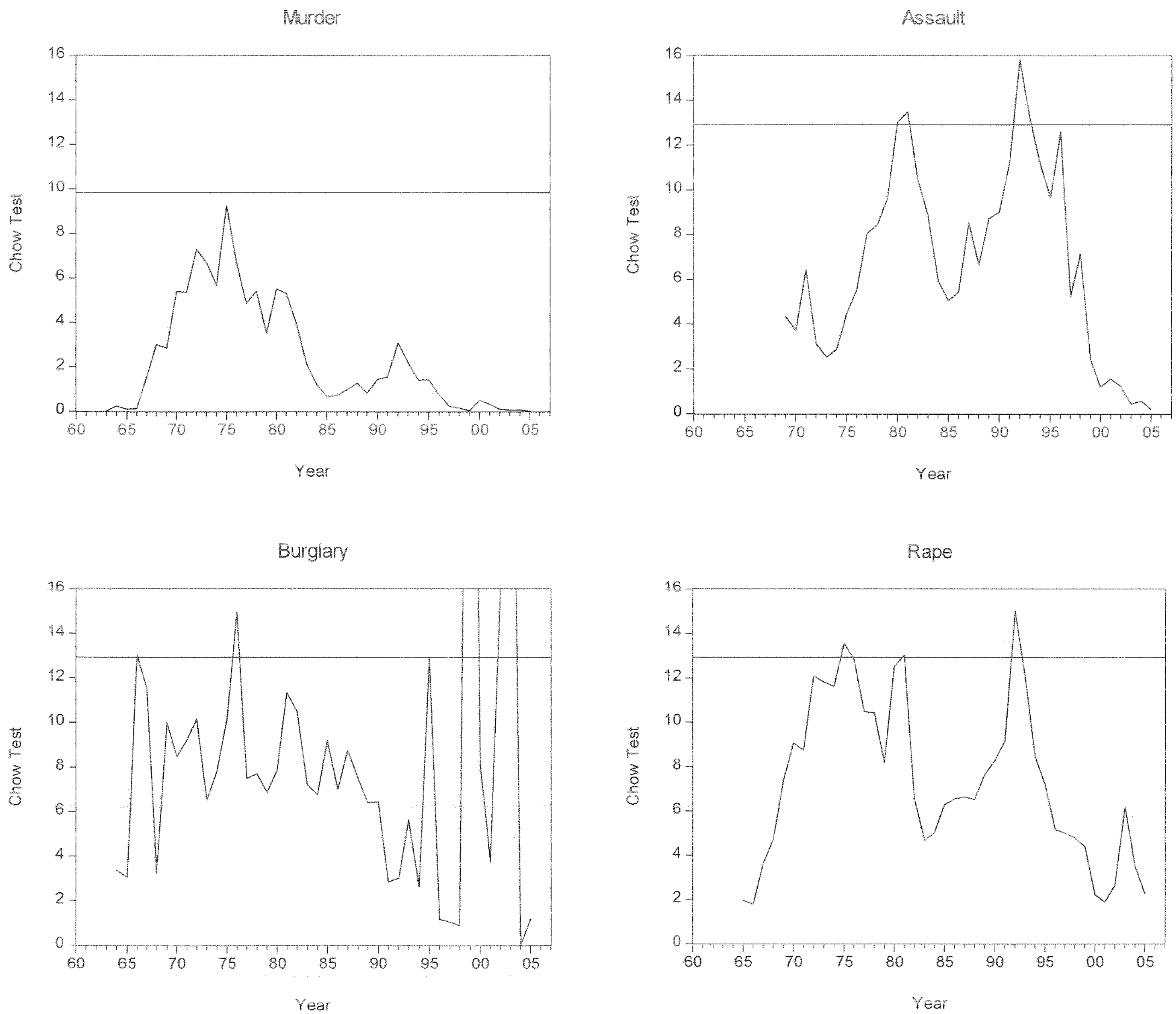
[46] R. Quandt (1960). "Tests for the Hypothesis That a Linear Regression System Obeys Two Separate Regimes." *Journal of the American Statistical Association*, 55, pp. 324-330.

[47] Gregory C. Chow (1960). "Tests of Equality Between Sets of Coefficients in Two Linear Regressions". *Econometrica* 28(3): 591–605.

[48] Abraham Wald. "Tests of Statistical Hypotheses Concerning Several Parameters When the Number of Observations is Large," *Transactions of the American Mathematical Society*, 54, (1943), 426-482

Figure 3: Chow Test Sequence as a Function of Potential Break Year



Note: These plots make use of the number of instances of the given crime per 100,000 Ohio residents.

Table 3: Wald Statistic Maxima from Quandt Test

	Murder	Assault	Rape	Burglary
Maximum Wald Statistic	9.24	15.84	15.01	51.01
Year Attained	1975	1992	1992	1999

5. Generalization

In light of the apparent success of the breakpoint analysis as applied to the case of Ohio, it seems natural to expand the analysis to consider whether the lack of evidence for a break in the structure of the crime series occurring simultaneously with the adoption of such a policy is unique or an exception. To this end, I will consider further series, allowing for some simplifying assumptions. First, for obvious reasons, I will consider only the cases of the thirty-seven states that have adopted shall-issue policies. In order to limit the number of series to consider here, I will only consider two crimes with respect to these thirty-seven states, murder and burglary. I consider murder because it is, by far, the greatest contributor to cost of crime estimates. Burglary is included because the previous analysis shows it to be the most variable of the series in the case of Ohio. In order to endogenously determine the location which has the strongest evidence in favor of a break, each of the series was differenced until it was stationary, and the break date which minimized the t-statistic resulting from the Zivot-Andrews test was recorded. These findings are presented in Table 4. Most of the series and all of the burglary series were determined to be I(1), while two of the murder series were I(2) and eight of them were I(0). None of the endogenously determined breakpoints occurred in the same year as the adoption of a shall-issue law, only one occurred within one year of such an adoption, and one more within two years. With the exception of these two cases, the Indiana and Louisiana murder series, no breaks occur within the window of interest surrounding such an adoption, and indeed less than three percent of the series presented evidence of an endogenously selected breakpoint within two years of the enacting of shall-issue policy, and one of those instances occurred in 1981, which was a common breakpoint selection among the considered series. The ZA test, it seems, casts further doubt on

Table 4: Zivot-Andrews Test Results

		Murder		Burglary	
State	Adoption Year	ZA Test Stat	Break Date	ZA Test Stat	Break Date
Alaska	1994	-8.896	1984	-7.201	1980
Arizona	1995	-5.927	1981	-7.167	1977
Arkansas	1994	{11.650}	2000	-5.883	2001
Colorado	2003	[-5.406]	1970	-6.545	1981
Connecticut	1969	[-5.028]	1975	-7.226	1982
Florida	1987	-5.838	1974	-7.446	1981
Georgia	1989	-9.276	1973	-5.156	1981
Idaho	1990	-11.919	1978	-6.321	1981
Indiana	1980	-7.148	1981	-7.539	1976
Kansas	2006	-7.841	1989	-6.235	1981
Kentucky	1996	[-5.454]	1968	-7.11	1983
Louisiana	1996	-7.122	1994	-7.6	1981
Maine	1985	[-8.35]	1972	-6.714	1976
Michigan	2001	{16.056}	1978	-7.042	1975
Minnesota	2003	-10.132	1985	-6.679	1982
Mississippi	2003	-8.034	1974	-6.911	1971
Missouri	1990	-5.563	1971	-6.436	1981
Montana	1991	-13.737	1986	-8.258	1982
Nebraska	2006	[-5.261]	1972	-6.637	1982
Nevada	1995	-9.992	1981	-6.48	1981
New Hampshire	pre-sample	-7.077	1978	-6.756	1981
New Mexico	2003	-10.115	1981	-7.162	1976
North Carolina	1995	-6.537	1973	-5.421	1992
North Dakota	1985	[-8.379]	1979	-9.881	1976
Ohio	2004	-7.836	1975	-7.453	1982
Oklahoma	1995	-13.039	1983	-5.963	1988
Oregon	1989	-12.738	1976	-5.916	1976
Pennsylvania	1989	-7.515	1976	-7.088	1981
South Carolina	1996	-8.049	1973	-6.464	1976
South Dakota	1986	[-8.071]	1967	-6.128	1981
Tennessee	1994	-9.792	1975	-8.154	1978
Texas	1995	-5.783	1981	-4.977	1989
Utah	1995	-7.794	1980	-7.576	1981
Virginia	1994	-11.475	1976	-7	1976
Washington	1961	-8.421	1976	-5.447	1995
West Virginia	1989	-8.601	1986	-7.854	1982
Wyoming	1995	[-6.319]	1968	-7.286	1982

Note:. The 1%, 5% and 10% critical values for the ZA test with break(s) in intercept (intercept and trend) are -5.34 (-5.57), -4.80 (-5.08), and -4.58 (-4.82) respectively. The values in [] indicate that the series achieved was I(0), while the {} indicate that the series was I(2). The unmarked values indicate that the series was I(1).

the notion that the adoption of shall-issue laws might cause a change in the structural models underlying the crime series.

6. Conclusions

In this paper I attempted to shed light on the debate concerning the impact of shall-issue laws by considering the case of a late-adopting state, Ohio, through structural break analysis of the growth rates of the incidences of four major crimes: murder, assault, rape, and burglary. I sought to extend that analysis to other states that had adopted by endogenously selecting breaks in the series. The analysis, in apparent agreement with Kovandzic and Marvell(2003) and Levitt(2004), suggests that the adoption of a shall-issue law, whatever its other merits, does not have a noticeable impact on the long-run structure of the crime series. This analysis would benefit from extension when still more data are available in order to make possible greater trimming of the sample, as this would serve to make the results even more robust. Further, the generalization could be expanded to consider more series in more depth as more advanced techniques become practical.

Appendix: UCR Ohio Data 1960-2007

Year	Murder	Rape	Assault	Burglary
1960	3.2	5.9	34.5	383.9
1961	3.1	5.6	32.5	401.8
1962	3.2	5.2	36.6	396.3
1963	3	6.1	39.5	431.5
1964	3.5	7.1	57.9	466.3
1965	3.6	8.9	60.7	470.5
1966	4.5	9.3	67.8	490.7
1967	5.2	10.3	74.5	607.6
1968	5.3	12.4	80.7	659.4
1969	6.4	15.3	99.8	740.1
1970	6.6	16	115.9	853.9
1971	7.5	18.4	108.7	932.6
1972	7.5	19.9	111.4	901.3
1973	7.3	21.4	119.5	943
1974	8.9	23.9	140.2	1171.8
1975	8.1	25.3	154.6	1271.4
1976	7.4	25.8	171.7	1203.2
1977	7.8	27.3	181.1	1216
1978	6.9	27.4	195.8	1214.5
1979	8.1	31.8	222.8	1287.2
1980	8.1	34.3	232.2	1466.3
1981	7.4	31	221.3	1493.6
1982	6.3	29.9	217	1309.6
1983	5.6	31.5	201.4	1155.6
1984	5.1	34.7	199.9	1049.9
1985	5.2	36.9	206.4	976.5
1986	5.5	38.6	234.7	987.8
1987	5.8	39.9	222.5	1062.5
1988	5.4	42.6	242.7	1031.4
1989	6	44.7	247.1	1018.2
1990	6.1	46.8	264.7	982.5
1991	7.2	52.5	287	1055.2
1992	6.6	52.1	268.2	947.3
1993	6	49.1	256.3	878.1
1994	6	47.1	245.1	866.3
1995	5.4	43.4	255	838.8
1996	4.8	41.3	218.4	835.4
1997	4.7	40.8	231.2	844.6
1998	4	40.5	184.5	810.1
1999	3.5	36.7	148.2	773.1
2000	3.7	37.6	155.3	780.7
2001	4	39.3	157.4	852.1

Appendix: UCR Ohio Data 1960-2007

2002	4.6	42.2	148.3	869.2
2003	4.6	40.7	140.8	831.3
2004	4.4	41.4	140.7	842.9
2005	5.1	40.7	141.5	873.4
2006	4.7	39.6	139.2	909.8
2007	4.5	38.8	140.7	859.1