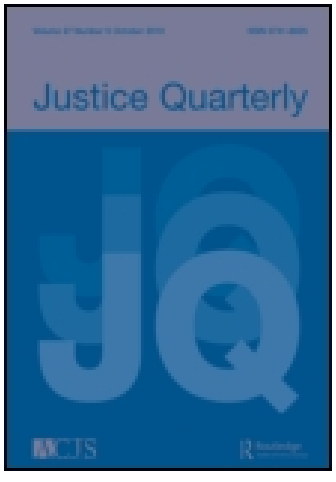
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Legislative Waiver Reconsidered: General Deterrent Effects of Statutory Exclusion Laws Enacted Post-1979

Benjamin Steiner, Craig Hemmens and Valerie Bell

In recent years, the juvenile justice system has undergone a "get tough" transformation. One component of the system which has experienced substantial alteration is the mechanism by which states allow for the waiver of juvenile offenders to adult criminal court. Most of the state systems now have some form of transfer procedure in place and many allow for the automatic transfer (or statutory exclusion) of juveniles who have been charged with certain offenses. Although the effects of waiver laws on individuals have received much empirical attention, their effects on the respective states' aggregate level violent juvenile crime rates are less understood. In this study, we examine the relative effects of legislative waiver laws in 22 states that have added statutory exclusion provisions since 1979. In doing so, we assess whether legislative waiver should be reconsidered.

Keywords waiver; transfer; juvenile; deterrence

Beginning with its conception, the juvenile justice system adhered to the Progressive era goal of rehabilitating the offenders who were brought to its

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attention. Under the progressive model, which began in the early part of the 20th century, the juvenile justice system attempted to intervene in the lives of wayward youth by acting in the "best interests of the child" (Cullen & Gilbert, 1982; Feld, 2003). Juvenile cases were handled individually and often informally. The focus was on the offender rather than the offense, reformation rather than retribution.

The progressive model continued until the early 1970s, when conservatives began condemning the individualized scheme being used in the adult criminal justice system, arguing that it was the cause of societal dissonance and rising crime rates (Cullen & Gilbert, 1982; Feld, 2003; Rothman, 1980; Ruth & Reitz, 2003). This criticism was eventually extended to the juvenile justice system. Conservatives argued that the rehabilitation-driven juvenile justice system was soft on crime and advocated instead for a retributive approach to juvenile crime (Feld, 2003). Their position was not without empirical support. Juvenile violence began to rise in the 1970s and escalated substantially in the 1980s (Feld, 1999, 2003; Snyder, 1997). The arrest rate for violent juvenile crime rose 58 percent between 1980 and 1994 (Feld, 1999; Snyder & Sickmund, 1999). Moreover, the juvenile homicide rate doubled between 1987 and 1993, and although it declined in the late 1990s it remained notably above the rate of the early 1980s (Ruth & Reitz, 2003; Snyder & Sickmund, 1999).

While conservatives were criticizing the criminal and juvenile justice systems for their perceived failure to reduce crime and for being too lenient in their treatment of offenders, liberals were also becoming disenchanted with the individualized justice approach, albeit for different reasons. Liberals were concerned that the discretion that pervaded the offender-focused criminal justice system under the guise of rehabilitation was linked to discrimination (Feld, 2003). They decried the fact that offenders suffered unfair treatment at the hands of the judiciary and corrections officials, and argued for procedural protections for criminal defendants and offenders. The conservative denigration and liberal disenchantment, coupled with a due process movement by the United States Supreme Court affording juveniles many of the same due process rights guaranteed adult defendants (see Feld, 1987), prompted the once informal juvenile justice system to become formalized and accountability driven (Champion, 1989; Feld, 1993, 2003; Jensen & Metsger, 1994). This "criminalized" juvenile court has been called the "just desserts" or justice model (Cullen & Gilbert, 1982; Feld, 1987, 1993, 2003; Ruth & Reitz, 2003).

One of the areas where the "just desserts" or "get tough" trend has been epitomized is the transfer, or waiver, of juvenile offenders to adult criminal court for prosecution. There are several ways a juvenile can be transferred to criminal court. Judicial waiver is the process by which a juvenile judge may, at their discretion, transfer a juvenile to criminal court. Prosecutorial discretion, or direct file, authorizes prosecutors to file certain cases in either juvenile or criminal court, under a concurrent jurisdiction statute. Statutory exclusion, or legislative waiver, is the method by which state legislatures have mandated the exclusion of certain offenses from the juvenile court. By 1979, every state

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allowed some form of transfer option (Fritsch & Hemmens, 1995; Steiner & Hemmens, 2003). In the 1980s, juvenile waivers increased nationally by 400 percent (Krisberg & Austin, 1993). During the same time period, and continuing to the present day, virtually every state modified or amended its juvenile court jurisdiction in some fashion. Most states added offenses that were eligible for judicial waiver and lowered the age at which a juvenile could be transferred to criminal court. In addition, many states greatly expanded the scope and use of legislative waiver (Steiner & Hemmens, 2003).

Clearly the legislature's goal in enacting a waiver statute is to have an effect on criminal behavior and criminal offending, whether it be through retribution, rehabilitation, or deterrence. A relatively unanswered question regarding juvenile waiver statutes is whether these statutes have a general deterrent effect on juvenile offending. Few studies have examined this question in any detail.

Prior Research

There has been extensive research assessing the various effects of juvenile waiver. Most of this research has focused on tracking the difference in sentences imposed in the juvenile and adult criminal courts, as well as the amount of time actually served by waived juvenile offenders.

Waiver and Sentencing Practices

A number of researchers have compared the sentences that juvenile offenders receive in juvenile court with the sentences that similarly situated youthful offenders receive in criminal court. Some research indicates that juveniles typically receive lighter sentences (i.e., probation) when transferred to criminal court than they would have received had they remained in the juvenile system (Bortner, 1986; Champion, 1989; Kinder, Veneziano, Fichter, & Azuma, 1995; Redding, 2003). On the other hand, other research indicates that the sentence outcome is contingent on offense type, with non-violent offenders receiving harsher sentences in the juvenile court (Barnes & Franz, 1989; Clement, 1997; Fagan, 1995; Podkopacz & Feld, 1996, Redding, 2003) and violent offenders receiving tougher sentences in the criminal court (Barnes & Franz, 1989; Clement, 1997; Fagan, 1995; Houghtalin & Mays, 1991; Kupchik, Fagan, & Liberman, 2003; Myers, 2003b; Podkopacz & Feld, 1996; Redding, 2003; Snyder, Sickmund, & Poe-Yamagata, 2000; Strom & Smith, 1998).

While these studies are limited in that they are primarily descriptive and often lack matched control groups (see Fagan's (1995) study for a comparison group matched on age, offense, and time period), the findings have still been relatively robust when compared to the evidence garnered from multivariate analyses assessing sentencing outcomes (e.g., Barnes & Franz, 1989; Myers, 2003b). That is, juveniles who are waived for property and drug crimes are

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sentenced to community sanctions more often in criminal court when compared to juveniles sentenced in juvenile court, while juveniles waived for violent offenses are incarcerated more often in criminal court than in juvenile court.

Waiver and Time Served

With regards to sentence length or time served for waived youth, violent offenses have garnered longer sentences than those typically imposed in the juvenile system, while non-violent offenses have yielded longer sentences in the juvenile system (Bishop, Frazier, Lanza-Kaduce, & Winner, 1996; Clement, 1997; Fagan, 1995; Myers, 2003b; Podkopacz & Feld, 1996). In addition, Kurlychek and Johnson (2004) found that even when all legal and extralegal factors were controlled for, juveniles waived to criminal court were sentenced more stringently than 18–24 year olds sentenced over the same period of time.

In contrast, other researchers have found that even when criminal courts imposed substantial sentences, parole authorities have typically released youthful offenders after serving less time than the offenders would have served had they remained in the juvenile system (Bortner, 1986; Clement, 1997; Fritsch, Caeti, & Hemmens, 1996).

Recidivism and Waiver

Researchers have also examined the connection between waiver and recidivism rates of juvenile offenders in both juvenile and criminal court. Recidivism rates have generally been lower for youth retained in juvenile court when compared to those transferred to criminal court (Fagan, 1995; Myers, 2003a; Podkopacz & Feld, 1996; Winner, Lanza-Kaduce, Bishop, & Frazier, 1997). Transferred juveniles are also found to re-offend quicker and more often than those youth sentenced in the juvenile system (Fagan, 1995; Winner et al., 1997). These findings are surprising in that they contradict the results anticipated by advocates of deterrence.

The individual-level findings regarding the practice of juvenile waiver appear to indicate that the use of the waiver does not necessarily result in juveniles being punished more severely either in sentence outcome, sentence length, or time served. In addition, the waiver may not have a specific deterrent effect on most juvenile's future offending. Although interviews with waived juveniles have revealed that some youthful offenders report being deterred by the sentences imposed in adult criminal court (see, for example, Lane, Lanza-Kaduce, Frazier, & Bishop, 2002), these offenders were not yet released. Moreover, the exploratory nature of these studies did not allow for conclusions regarding what types of juveniles these may be. Future research will have to examine further whether or not juveniles transferred to criminal court are in fact specifically deterred, and, if so, seek to reveal what the characteristics of these youthful offenders are.

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On the other hand, it is conceivable that the legislative intent of the recent modifications of the waiver statutes (see Steiner & Hemmens, 2003) was two-fold. While it is likely the state legislatures were trying to improve the judiciary's ability to sentence serious and violent juvenile offenders more severely by allowing for adult criminal sanctions in certain cases, it is equally plausible that they were attempting to generally deter potential serious and violent juveniles. These aggregate-level effects would not be tapped in the aforementioned individual-level studies.

General Deterrence and Waiver

There has been much less research conducted on the general deterrent effects of juvenile waiver laws. This is not surprising, given the methodological difficulties inherent in such research. In view of the lack of empirical work examining the general deterrent effects of legislative waiver laws, the extant evidence regarding their effectiveness can hardly be called conclusive. In fact, our search of the published works revealed only three such evaluations. Compounding the problem of lack of research is the fact that each of these studies employed slightly different methods in assessing their respective states' law.

Singer and McDowall (1988) used an interrupted time series design to assess the effectiveness of the New York Juvenile Offender Law in New York City and upstate New York. They examined monthly arrest data between the years 1974 and 1984 for juveniles who were eligible for legislative waiver for each of the five crimes (homicide, aggravated assault, robbery, rape, and arson) which allowed for prosecution in adult criminal court. Singer and McDowall (1988) found significantly lower rates of arrest for rape and arson in New York City after the law went into effect; however, their control series, Philadelphia, yielded similar results for arson and significantly lower arrest rates for homicide and aggravated assault, despite having no waiver mechanism, suggesting a history effect. Additionally, they revealed significantly higher arrest rates for aggravated assault and robbery in upstate New York after the waiver law went into effect. As a result, Singer and McDowall (1988) concluded that New York's Juvenile Offender Law had no appreciable effect on deterring violent juvenile crime.

Jensen and Metsger (1994) used a multiple time series design and a regression analysis to evaluate the effect of Idaho's legislative waiver statute on violent juvenile crime. In their multiple time series analysis they examined combined mean arrest rates for four of the offenses covered in the statute for the 5 years prior to the enactment of the law and the 5 years after the statute was enacted. Although arrest rates went down in the control states during the time after the waiver law was enacted, the juvenile arrest rate for violent crime actually increased 18 percent in Idaho.

In their regression analysis, Jensen and Metsger (1994) used the annual total number of arrests for the four offenses as their dependent variable. Their

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primary independent variable was the existence of the waiver law, coded as a dichotomous variable. Jensen and Metsger (1994) controlled for the infant mortality rate in an effort to tap economic deprivation, the estimated juvenile population of the state, and the number of law enforcement agencies reporting during the years under study in order to account for any fluctuations that may have occurred. Their findings confirmed the conclusion from the multiple time series analysis, namely that when controlling for infant mortality, population, and agencies' reporting, the legislative waiver statute was not significantly related to arrests for violent juvenile crime.

Risler, Sweatman, and Nackerud (1998) assessed the effect of Georgia's Juvenile Justice Reform Act. They compared the mean number of juvenile arrests for each of the violent index crimes for the 2 years prior to the enactment of the legislation to the mean number of juvenile arrests for each of the violent index crimes for the year the law was enacted and the subsequent year. No significant differences in the mean juvenile arrest rates for any of the violent crimes after the waiver law was enacted were revealed. These findings also held when they examined the effects of the law on different racial/ethnic groups. Accordingly, Risler and his colleagues (1998) concluded that Georgia's legislative waiver law was not effective in deterring violent juvenile crime.

The findings from these three studies suggest that legislative waiver statutes do not achieve a general deterrent effect. However, aside from Singer and McDowell's (1988) study, the existing evidence regarding legislative waiver laws should be interpreted cautiously since these studies did not use statistical techniques that allowed them to control for both serial correlation and regression effects (see McDowall, McCleary, Meidinger, & Hay, 1980).

Other aggregate-level deterrence research has revealed some evidence that supports a deterrent effect resulting from certain policy changes aimed at increasing the level of punishment. However, these effects have been characterized as short term (Nagin, 1998). Additionally, many of the policy changes show little or no effect (Cullen, Pratt, Micelli, & Moon, 2002; Nagin, 1998; Robinson & Darley, 2004), and there have even been instances of some increases in crime (Sherman, 1993). This suggests that general deterrence-based policies may not be good policies. This conclusion is perhaps best supported by Pratt and Cullen's (2005) findings from their meta-analysis of the macro-level predictors of crime. Pratt and Cullen (2005) found only one criminal justice related predictor (incarceration effect) to be greater than .30 and only one criminal justice related predictor (arrest ratio) to be greater than .20. The majority of criminal justice related predictors had effects under .10 (Pratt & Cullen, 2005).

The addition of legislative waiver laws has been the most prevalent "get tough" juvenile justice legislation since the rise in juvenile crime (Snyder & Sickmund, 1999), with 32 states creating or modifying legislative waiver statutes since 1979 (Steiner & Hemmens, 2003). Research examining the general deterrent effect of these legislative waiver laws, while limited, suggests that a

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deterrent effect is not being achieved. In spite of the evidence against policies and laws aimed at deterring crime, such laws and policies still pervade the criminal justice system and they have filtered down to the juvenile justice system. While there have been substantial empirical tests of a deterrent effect for policies aimed at adults, their effects on juvenile populations are less understood. The goal of this study is to fill this void.

Methodology

As of 2003, 31 states and the District of Columbia had some form of legislative waiver. Twenty-three states either created or repealed legislative waiver statutes between 1979 and 2003. In this study, we examined the relative effects of the legislative waiver laws in 22 states that either enacted legislative waiver laws or substantially amended their existing laws after 1979 (see Steiner & Hemmens, 2003).[1] Given that the validity of the assessment of each state's law was dependent on whether or not we examined the correct time period in which the law took effect, we took extra precautions to ensure the dates we examined were well founded.

First, we examined the legislative history of the statutes for each state that added legislative waiver provisions (e.g., Alaska) or modified their existing provisions significantly (e.g., Indiana) after 1979 (see Fritsch & Hemmens, 1995; Steiner & Hemmens, 2003). Second, in order to verify our findings we conducted an Internet survey (see Dillman, 2000) of each state's attorney general's office as well as each state's legislative services committee. After receiving these responses, we compared the data from the state offices to our initial findings and changed one state's effective date. Except for this one state, the results from the survey were either unusable (i.e., state officials were unsure) or confirmed our own findings.

In order to assess the relative effects of the legislative waiver laws in the twenty-two states, we used a quasi-experimental multiple interrupted time series design similar to the one used by Singer and McDowall (1988). We chose this method because a multiple interrupted time series design is the quasi-experimental design that is the least susceptible to internal validity threats (Campbell & Stanley, 1963; Shadish, Cook, & Campbell, 2002). The respective series used to assess the effects of the waiver laws consisted of each state's monthly juvenile arrest rate for homicide/manslaughter as well as all violent index crime for each month during the 5 years prior to the

1. "Substantially amended" was defined as expanding their law from including solely murder to incorporating additional offenses. We did not examine eight states that already had legislative waiver laws in place in 1979. Two other states (Florida and New Hampshire) were excluded from our analysis because of data collection issues. Florida added a legislative waiver statute in 2000; however, data were unavailable for all of the months surrounding the statutes' effective date. New Hampshire added a legislative waiver provision in 2002. Accordingly, there were not enough observations after the law went into effect to assess its effects.

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effective date of the law and for each month of the 5 years after the law went into effect.[2]

Data

In determining the monthly juvenile arrest rate for violent crime per 100,000 persons under 18, we obtained the total number of juvenile arrests for violent index crime (murder, rape, robbery, and aggravated assault) from several data sets housed at the International Consortium for Political and Social Research. First, we used Susan Carlson's monthly arrest data compiled from the *Uniform Crime Reports* for the years 1975–1993.[3] Next, we merged Dr. Carlson's data with the monthly arrest data from the individual *Uniform Crime Reports* for the years 1994–2002, which were also housed at the Consortium.[4] Finally, we calculated the juvenile arrest rates for both violent index crime as well as homicide/manslaughter by dividing the number of juvenile arrests for each by the individual state's total population estimates of persons under 18 and then multiplied the quotient by 100,000. We decided to conduct separate analyses for both the juvenile arrest rate for violent index crime and homicide/manslaughter because some states (e.g., Minnesota) statutorily excluded only those juveniles who were charged with homicide. The population estimates were obtained directly from United States Bureau of the Census.

Arrest data are advantageous because they offer age-specific information on various crimes. Additionally, when used to assess only changes in the level of arrests, the data are not subject to the criticism that official data under-represent delinquent activity (Snyder & Sickmund, 1999). However, arrests are imperfectly related to juvenile crime, as they are influenced by the behavior of police as well as juveniles. Although interrupted time series designs can tolerate some measurement bias, the data used here could be subject to a threat that Shadish and his colleagues (2002) referred to as instrumentation.

In this study, it was reasonable to assume that an instrumentation effect could occur if the police altered their arrest practices as a result of the respective waiver laws going into effect. For example, police may have arrested juveniles more vigorously because they felt the juveniles would be subject to greater

2. Kentucky's legislative waiver law went into effect January 1, 2001. As a result, data were only available for 2 years after the law went into effect. Attempts were made to attain more recent data from the state of Kentucky; however, the data were unavailable.

3. We contacted Dr. Carlson to assess the validity of the state-level data. Dr. Carlson advised that univariate ARIMA predictions models were used to estimate some of the state-level data for two states (Michigan and Texas), neither of which were assessed here.

4. From 1991 to the present, Alabama did not report monthly arrest data to the FBI. Accordingly, the data for Alabama were obtained directly from the Alabama State Criminal Justice Information Center. In 1991 Iowa did not report monthly arrest data to the FBI. The Iowa data were obtained from the Iowa Department of Public Safety. Missouri's legislative waiver law went into effect January 1, 1999. We obtained Missouri's 2003 data, which was not yet available from the FBI, directly from the Missouri State Highway Patrol.

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punishment and not just a "slap on the wrist." On the other hand, police may also have charged juveniles with less serious crimes to protect them from being subjected to the perceived harshness of the criminal court. These possibilities were also noted by Singer and McDowall (1988). However, unlike Singer and McDowall (1988), who examined the effect of the New York Juvenile Offender Law on juvenile arrest rates in New York City, we assessed the effects of the legislative waiver laws on the juvenile arrest rates for each of the respective states. Accordingly, for an instrumentation effect to have occurred multiple police agencies within the individual states would have to alter their arrest practice in the same direction. Although possible, we find this extremely unlikely.

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Analytical Strategy

After computing the monthly arrest rates, each series was then analyzed using autoregressive integrated moving average (ARIMA) techniques. ARIMA modeling of the data allowed us to control for any serial dependence between the time points and estimate the differences before and after the effective date of the respective waiver laws (Cochran, Chamlin, & Seth, 1994; McDowall et al., 1980). The process involved developing a "white noise" model for each series to control for non-stationarity, serial correlation, and seasonality, all of which are often observed with time series data (for a detailed description of "prewhitening" see Cochran et al., 1994; McDowall et al., 1980). Next, an intervention series (the time points after the respective waiver laws went into effect) was added to the model to represent the effects of the intervention. If a change was observed following the effective date of the intervention (the waiver law), it was reflected in the intervention model.

In order to model the intervention series, the appropriate transfer function (zero order, first order, or pulse) is used to estimate the impact of the intervention series on their corresponding white noise dependent series. In this study, we relied on Singer and McDowell's (1988) prior work and hypothesized that an abrupt permanent impact (zero order transfer function) would be realized after the intervention series was added to the model. However, in the event our hypothesis was not specified correctly; we estimated the models using both the first order and pulse function as well. After determining the functional form of best fit we subjected each final model to a number of diagnostic checks to verify whether the final model was indeed adequate. By estimating each of the various transfer functions and subjecting the final models to diagnostic testing, we were confident that we determined the most precise model. Put simply, we felt confident in our ability to gauge the general deterrent effect of the various legislative waiver laws.

Methodological Limitations

The interrupted time series is a strong design. However, as is the case with most research designs in the social sciences, it has some limitations. As Campbell and

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Stanley (1963) and more recently Shadish et al. (2002) have noted, the only threat to internal validity that poses a problem for an interrupted time series research design is history. In other words, did some other unmeasured and uncontrolled factor prompt the observed change in the dependent series? History threats of this nature are best resolved by introducing a control series (Campbell & Stanley, 1963; Shadish et al., 2002; Singer & McDowall, 1988).

However, as Britt, Kleck, and Bordua (1996) note, it is important for the control group to resemble the intervention group as much as possible. In the current study, this posed a problem for two reasons. First, we did not want to use states that enacted a legislative waiver provision during, or close to, the period we were assessing in the corresponding intervention state. As a result, several states (see Steiner & Hemmens, 2003) were excluded as possible controls. Second, we realize that two states are rarely alike and thus we were limited in our possible choices for controls. Nonetheless, we still attempted to address this threat as adequately as possible by selecting two different states as controls for each state in which an effect was observed. Each control state did not have a legislative waiver provision go into effect during, or near to, the time period assessed in the intervention state. Additionally, each control state resembled the state in which a significant change in the juvenile arrest rate was observed as closely as possible in size, location, percentage of the population under 18, juvenile arrest rate for violent crime, unemployment rate, and percentage of the population not White. Comparisons of the intervention states and their respective control states on the aforementioned indicators are displayed in Appendix A.

McCleary and Hay (1980) suggest that the identification and estimation of ARIMA models can be confounded by large intervention series and short time series, such as those containing fewer than 50 observations. Shadish and his colleagues (2002) assert that an interrupted time series design requires 100 observations to model trends, seasonality, and the structure of correlated error in the series before conducting an impact assessment. However, Britt et al. (1996) note that findings occasionally vary depending on the length of the series. While we believe that the 120 observations used here were sufficient to capture any trends, seasonality, or structure of correlated error in the series before the effective dates of the waiver laws, as well as any effects that the waiver laws may have engendered, we acknowledge that we would be remiss not to investigate the possibility that we were wrong. Accordingly, six states with available data were selected to test Britt et al.'s (1996) suggestion. Illinois, Kansas, and North Carolina were all examined by adding an additional 5 years (120 observations) to the period *after* the waiver law went into effect, while Georgia, Virginia, and Wisconsin were all assessed by adding an additional 5 years to the period *before* the waiver law went into effect. We contend that conducting these further analyses strengthens the findings of this study.

Another concern was our specification of the intervention model. While the effective date of the various legislative waiver laws was, in our opinion, the most likely time point for an effect to be observed, other scenarios are possible.

For example, the enactment of the law may have been accompanied by a press release that itself created a deterrent effect. Then again, this would require that potential juvenile offenders be cognizant of either the legislative session or any accompanying news releases. We find this particularly unlikely. Instead, we suggest it is much more reasonable that potential juvenile offenders will be informed of changes in the law through informal channels (see, for example, Anderson, 1999).

If juveniles are receiving this information through informal channels, another time point (the date the first juvenile offender was waived in each state) could be a plausible intervention point. Indeed, many of the evaluations assessing the impact of the death penalty have used the date of the first execution and not the date of the change in the law as the appropriate time point (e.g., Cochran et al., 1994). While we acknowledge this time point may merit consideration as an intervention period, the data were simply not available to investigate that possibility here. Moreover, we feel confident that in larger populated states (e.g., Pennsylvania) a waiver would have been likely to occur soon after the change in the law. An automatic transfer of a juvenile and subsequent trial takes much less time than a death penalty trial and subsequent execution (the current average time to execution after sentence is in excess of 10 years). As such, our estimation of the gradual permanent change function may observe a deterrent effect which resulted from any waivers that occurred immediately after the effective date of the law. However, further research would be needed to support this claim.

Finally, it could also be possible that states that enacted a legislative waiver statute already had a wavier mechanism (e.g., judicial waiver, direct file) in place. The enactment of this earlier legislation could have actually caused any deterrent effect that may have occurred on that state's violent juvenile crime. With respect to judicial waiver, we find this extremely unlikely. Judicial waiver has been a part of most state juvenile codes for many years before the period under examination here (Feld, 1987; Fritsch & Hemmens, 1995). Judicial waiver is a mechanism designed to remove from juvenile court those juveniles who have exhausted the resources of the juvenile system (Feld, 2001), not to deter. The judicial waiver represents the juvenile court's approach to individual sentencing; it is not deterministic or certain (Feld, 1993). In order for juveniles to be deterred it would have to be perceived that they would be waived for the act they may potentially commit. This may not always be the case for those juveniles suitable for judicial waiver. On the other hand, it is certain that those cases eligible for legislative waiver will begin in adult criminal court.

Direct file statutes exist in 14 states, 2 of which (Georgia and Vermont) were examined here. In both Georgia and Vermont, the direct file statute was enacted at the same time as the legislative waiver statute. As such, prior deterrent effects could not have been observed for the direct file statutes in those two states. Additionally, the direct file statutes in both states include additional offenses for which a juvenile could be waived. Since we are examining

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the effects of only the most serious violent crimes, all of which are covered by the legislative waiver statutes in these two states, we felt confident that juveniles would be automatically transferred if they were charged with any of these offenses.

As noted above, a multiple interrupted time series design is the quasi-experimental design that is the least susceptible to internal validity threats. On the other hand, the design is weak externally (Britt et al., 1996; Campbell & Stanley, 1963; Shadish et al., 2002). The reason for this external weakness is that an intervention is typically assessed in one geographic locality during one period of time. However, Briggs, Sundt, and Castellano (2003) recently noted that the assessment of multiple series in different locations over different time periods, as is the case in this study, strengthens the external validity of the findings. We contend that unlike previous studies (e.g., Singer & McDowell, 1988), consistent findings in this analysis of 22 states could aid in determining whether legislative waiver laws are having a general deterrent effect on serious and violent juvenile crime.

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Findings

The last two decades have seen juvenile arrests for violent crimes reach an all-time high (Snyder, 2004). In response, many states have added or expanded the scope of their legislative waiver provisions. We comment briefly on those changes here (see Steiner & Hemmens, 2003 for a complete discussion of all the changes).

Legislative Waiver 1979–2003

By 2003, a substantial majority of states had some form of legislative waiver in place. While the states differed in the details, in general these legislative waiver statutes provided for the automatic exclusion from the jurisdiction of the juvenile court those offenders charged with serious (and sometimes not-so-serious) criminal activities.

Our historical examination of the state legal codes in combination with the responses to our survey of state agencies revealed that in 1979, 13 states and the District of Columbia had some form of legislative waiver. By 1995, nine other states enacted legislative waiver statutes and one repealed their statute, leaving 21 states that had some form of legislative waiver. Between 1995 and 2003, 12 more states enacted legislative waiver statutes and two repealed their existing statutes. As of 2003, 31 states and the District of Columbia now have a legislative waiver provision in place.

In addition to adding legislative waiver provisions, many states have modified their existing statutes. Between 1979 and 2003, 18 states modified existing statutes, some of those doing so more than once. Most states either added offenses

that were waiver eligible or lowered the age at which a juvenile is automatically transferred to adult criminal court. Indiana is an example of a state that modified its existing legislative waiver statute. In 1979, Indiana automatically excluded from juvenile court jurisdiction only those juveniles 16 years of age and older charged with murder. By 1995, Indiana had modified its legislative waiver statute to automatically exclude 16 year olds charged with sexual assault, kidnapping, armed robbery, carjacking, or any felony if the juvenile had a prior misdemeanor or felony conviction. By 2002, Indiana had eliminated the age requirement altogether for some of its excluded offenses. Other states have followed patterns similar to Indiana's. The current (as of 2003) status of the 22 states assessed in this study and the effective dates of the initial or substantive portion of the legislative waiver statutes are detailed in Appendix B.

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Impact of the Legislative Waiver Laws

After determining the effective dates of the legislative waiver laws in each state we modeled the monthly number of juvenile arrests for violent crime and homicide/manslaughter for the 5 years before and 5 years after the date the waiver law went into effect. Summaries of the individual time series designs for the juvenile arrest rates for violent crime are presented in Table 1. States which experienced interventions are in regular type, while the respective control states, if a change was observed, are in italics. Since we found the zero order transfer function model to be the most appropriate for the majority of the series, we reported the estimate of the parameter (ω), which represents the shift in the level of the series. When we determined that either a first order transfer function or pulse function was more appropriate we reported the parameter (ω), representing the shift in the level of the series, and the parameter (δ), which estimates the rate of change for the shift in the level of the series. The final models and fit statistics for each of the violent crime arrest rate series can be found in Appendix C.

As can be seen in Table 1, most of the series (18) were unaffected in the period after the intervention was added to the model. On the other hand, several states did observe changes in the rate of juvenile arrests for violent crime. Indiana experienced an increase in juvenile arrests for violent crime. At the same time, similar effects were not observed in either of the two control series, Ohio or Nebraska. Maine experienced a decline in its juvenile arrest rate for violent crime, which was immediate and permanent.[5] A similar change was not observed in either of the control series, New Hampshire or Connecticut. Missouri experienced an increase in its juvenile arrest rate for violent crime. However, the rate of change in Missouri was, although permanent, very

5. A significant first order transfer function was estimated for Maine. However, after examining the δ parameter it was determined that the zero order transfer function was more appropriate (see McDowell et al., 1980).

Table 1 Summary of intervention analyses for states that enacted legislative waiver statutes post-1979

Series	ω	*t*	δ	*t*
Violent crime				
Alabama	−.17	−.84		
Alaska	−.04	−.62		
Georgia	−.11	−.65		
Idaho	.11	.54		
Illinois	−.01	−.04		
Indiana	.25	1.85*		
Ohio[a]	.01	.09		
Nebraska	.15	.91		
Iowa	−.31	−1.44		
Kansas	−.04	−.20		
Kentucky	.02	.11		
Maine	−.58	−3.33*		
New Hampshire	.07	−.41		
Connecticut	−.11	−.64		
Minnesota	−.11	−1.47		
Missouri	.86	2.00**	−.97	−19.19**
Nebraska	.00	.04	−1.18	−2.14
Oklahoma	.00	.01	1.15	.24
Montana	.25	1.38		
Nevada	−.64	−1.26		
New Mexico	.03	−.11		
North Carolina	−.01	−.05		
North Dakota	−.09	−.43		
Ohio	−.16	−1.06		
Pennsylvania	.09	.39		
Vermont	−.16	−.91		
Virginia	−.14	−1.11		
Wisconsin	−.37	−1.72*	.61	1.92**
Nebraska	.04	.56	−.81	−.40
Indiana	−4.47	−.58	.20	.28

[a] Ohio and the other control states are italicized.
* $p < .10$; ** $p < .05$.

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gradual.[6] Nonetheless, the change was not observed in either of the control states, Nebraska or Oklahoma.[7] Lastly, Wisconsin also exhibited a decline in its juvenile arrest rate for violent crime after its legislative waiver law went into

6. A significant pulse function was estimated for Missouri. However, after examining the δ parameter it was determined that the first order transfer function was more appropriate (see McDowall et al., 1980).

7. Because 2003 monthly data were not yet available the control series were modeled with only 4 years of observations after the intervention. A Nebraska pulse model approached significance ($t = 1.51$, $p < .13$) and might be worth reassessing when the data become available.

effect. However, the pulse function was the model of best fit, which suggests an immediate temporary impact. A similar decline was not observed in either of the control states, Nebraska or Indiana. Thus, the difference in Wisconsin's rate of arrest for violent juvenile crime after the intervention, along with those observed in Indiana, Maine, and Missouri, should be treated as legitimate.

In view of the fact that several states excluded only those juveniles charged with homicide, we estimated models for each state's respective juvenile arrest rate for homicide. None of the states experienced differences in their individual juvenile arrest rate for homicide after their legislative waiver laws went into effect. As a result, the parameters are not reported here.[8]

With regard to interrupted time series like the ones used here, Britt et al. (1996) note that findings occasionally vary depending on the length of the series. We tested for this possibility by lengthening the series of six states and subjecting them to further analyses. The findings are presented in Table 2.

As noted previously, we extended the series for Georgia, Virginia, and Wisconsin by adding 5 years of observations before the intervention and by adding 5 years of observations after the intervention in Illinois, Kansas, and North Carolina. As Table 2 makes clear, the substantive findings did not change for any of the series. The Georgia, Virginia, Illinois, Kansas, and North Carolina series all

Table 2 Summary of intervention analysis after lengthening the series

Series	ω	*t*	δ	*t*	Final model	*Q*-statistic
Georgia 1990–1999	−.11	−.65			(0,1,1)	23.48 $p < .91$
Georgia 1985–1999	−.01	−.04			(0,1,1)	22.05 $p < .94$
Virginia 1992–2001	−.14	−1.11			(0,1,1)(0,0,3)s	35.29 $p < .36$
Virginia 1987–2001	−.17	−1.34			(0,1,1)(0,0,3)s	39.81 $p < .13$
Wisconsin 1990–2000[a]	−.37	−1.72*	.65	1.92**	(0,1,3)	37.56 $p < .23$
Wisconsin 1985–2000[a]	−4.02	−2.82**	.65	3.70**	(0,1,1)(0,0,2)s	39.02 $p < .18$
Illinois 1978–1987[a]	−.01	−.04			(0,1,2)(0,0,2)s	30.29 $p < .50$
Illinois 1978–1992[a]	.14	.69			(0,1,2)(0,1,2)s	24.83 $p < .17$
Kansas 1978–1987	−.04	−.20			(0,1,1)	22.55 $p < .93$
Kansas 1978–1992	.02	.17			(0,1,1)	27.82 $p < .76$
North Carolina 1975–1984	−.01	−.05			(0,1,1)	32.62 $p < .54$
North Carolina 1975–1989	.08	1.07			(0,1,1)	31.48 $p < .31$

Notes.
(p,d,q)(P,D,Q)s:
p = auto-regressive process, d = differenced, q = moving average process.
P = seasonal auto-regressive process, D = seasonally differenced, Q = seasonal moving average process.
Q = Box–Jenkins test statistic for the null hypothesis that the models' residuals are distributed as white noise.

[a] Log transformations were performed in order to make the series stationary in their variance.
* $p < .10$; ** $p < .05$.

8. These parameters and final models are available from the corresponding author upon request.

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remained unchanged after the interventions were added to the models in both the 10- and 15-year models. The Wisconsin series maintained a decline in arrests after the intervention. The coefficient became much larger, suggesting an even stronger relationship. On the other hand, the model of best fit for the Wisconsin series was the pulse model, indicating an abrupt temporary impact. Modeling a longer series of stationary arrest rates would yield a stronger effect.

All told, the findings from these secondary analyses strengthen the validity of the findings from our initial assessments. Accordingly, the findings observed here should be taken seriously.

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Discussion and Conclusions

Beginning in the late 1970s and continuing through the early 2000s the juvenile arrest rate for violent crime followed a similar pattern as the overall rate of violent crime. It began a gradual move upwards, peaked in the early 1990s, and has subsequently declined (Ruth & Reitz, 2003; Snyder, 2004; Snyder & Sickmund, 1999). Yet the juvenile arrest rate for violent crime remains substantially above the relatively stable rate of the years preceding the upswing.

The standard response to the rise in juvenile crime has been to "get tough" with the youthful offenders who come to the attention of the juvenile court. One way in which the states have gotten tougher with juvenile offenders is to transfer them to adult criminal court where the range of sanctions is presumably greater. The findings from this study, as well as others (Fritsch & Hemmens, 1995; Steiner & Hemmens, 2003), revealed that most states have modified their respective waiver statutes to make it easier to transfer juvenile offenders to the adult criminal court. As a result, over the last two decades more juveniles have found themselves subject to criminal prosecution (Krisberg & Austin, 1993; Snyder & Sickmund, 1999). In view of these findings, and those which have shown a reduction in the overall rate of violent juvenile crime (Ruth & Reitz, 2003; Snyder & Sickmund, 1999), it follows that the increased legislation and subsequent transfer of more juveniles to adult criminal court may have had a deterrent effect on potential violent juvenile offenders. The findings from this study do *not* support this claim.

The findings from this 22-state analysis generally agree with the negative findings from other studies (Jensen & Metsger, 1994; Risler et al., 1998; Singer & McDowall, 1988) regarding the general deterrent effect of legislative waiver laws. Only two states in this study experienced declines in their respective arrest rates for violent juvenile crime after their legislative waiver laws went into effect. Of those two states, only Maine experienced an abrupt permanent change. Wisconsin's change was modeled using a pulse function, which suggests a publicity effect (McDowall et al., 1980). Therefore, the findings from only one state (Maine) are supportive of a general deterrent effect.

In addition to the findings from the analyses of violent crime, the separate analyses for homicide/manslaughter did not reveal any significant changes in their respective juvenile arrest rates after their legislative waiver laws went into effect. While we acknowledge that this may be due to homicide being an infrequent occurrence in some of the lesser populated states (e.g., North Dakota), the findings obtained from more densely populated states (e.g., Ohio) should be taken seriously.

A possible reason for the findings from this study may be that juveniles in these states are not deterred by the possibility of being waived to adult criminal court because they are not receiving harsher sentences in the adult criminal court. Indeed, there is some empirical work that supports this notion (Bortner, 1986; Champion, 1989; Kinder et al., 1995; Redding, 2003). On the other hand, when offense type is examined, juveniles who are waived to adult criminal court for violent offenses, such as those examined here, are more likely to receive longer sentences than they would have in the juvenile court (Bishop et al., 1996; Clement, 1997; Fagan, 1995; Myers, 2003b; Podkopacz & Feld, 1996). Yet the notion of this wisdom filtering down to the youth committing crimes seems less than plausible. Thus, it is not likely that this is having a noticeable effect on many juvenile's decision to commit violent crime.

Perhaps a stronger conclusion is that the legislative waiver of juveniles to adult criminal court does not have a deterrent effect on violent juvenile crime. Indeed, this has been the case with other get-tough legislation (Cullen et al., 2002; Nagin, 1998; Pratt & Cullen, 2005; Robinson & Darley, 2004). While this may not sit well with policy makers who suggest toughening the juvenile justice system's response to serious and violent juvenile crime as the panacea to reducing its occurrence (see Ruth & Reitz, 2003 for a review), it may be time to reexamine these laws and their effects on the individuals they target. After all, although the public is supportive of punishment, the public is also supportive of rehabilitation, and specifically the rehabilitation of its young (Applegate, Cullen, & Fisher, 1997; Cullen, Fisher, & Applegate, 2000; Mears, 2001). In addition, it has been well documented that juveniles who are sentenced to adult correctional facilities are victimized more often (Maitland & Sluder, 1998; Redding, 1999) and suffer from psychological problems more frequently than other inmates in prison (Bingam & Power, 1997) and juveniles sent to training schools (Forst, Fagan, & Vivona, 1989). Moreover, juveniles who are waived to criminal court have been found to recidivate at much higher rates than those youth who are retained in the juvenile system (Fagan, 1995; Myers, 2003a; Podkopacz & Feld, 1996; Winner et al., 1997). Thus, the waiver of juveniles to adult criminal court may not impact the individual youth it targets in an effective manner. These findings, along with those from this study, support what some scholars have previously argued (see, for example, Bishop, 2004; Butts & Mears, 2001; Singer & McDowall, 1988), that it may be time for policy makers to question their current response to violent juvenile crime. It may be time to reconsider legislative waiver.

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Appendix A. Comparisons of Control States to Intervention States

Intervention states and *control states*	Percentage of population under 18	Juvenile arrest rate: violent crime	Unemp. rate	Land area rank	Percentage of population non-White	Geographic region
Indiana	26	4.51	5.9	38	10	MW
Ohio	26	7.28	6.4	34	12	MW
Nebraska	27	5.78	2.7	16	6	MW
Maine	25	3.14	7.5	39	2	NE
New Hampshire	25	2.47	7.2	46	2	NE
Connecticut	23	8.17	6.7	48	13	NE
Missouri	26	13.28	6.6	21	12	MW
Nebraska	27	5.78	2.7	16	6	MW
Oklahoma	27	11.78	6.7	20	18	S
Wisconsin	27	7.04	5.4	23	8	MW
Nebraska	27	5.78	2.7	16	6	MW
Indiana	26	4.51	5.9	38	9	MW

Appendix B. Legislative Waiver Statutes 2003

State and citation	Ages at which legislative waiver applies	Crimes eligible for legislative waiver	Legislative waiver law effective
Alabama AL Code 12-15-34.1	16 and older	Capital offenses, Class A felony, felony involving the use of a deadly weapon, felony causing serious physical injury, trafficking drugs	1/1/1997
Alaska AK Welfare, Social Services, and Institutions Sec. 47.12.030	16 and older	Unclassified felony or Class A felony against person, arson in 1st degree, Class B felony against person in which the minor is alleged to have used a deadly weapon and the minor was previously adjudicated of an offense that involved an offense against a person with the use of a deadly weapon	9/10/1996

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Georgia GA Code Ann. 15-11-28	13 and older	Murder, voluntary manslaughter, rape, aggravated sodomy, aggravated child molestation, aggravated sexual batter, aggravated robbery if committed with a firearm	1/1/1995
	15 and older	Burglary, if three prior burglary adjudications	
Idaho ID Code 20-509	14 and older	Murder, attempted murder, sexual assault, robbery, mayhem, illegal possession of drugs/firearms near school or school event, assault or battery with intent to commit a serious felony, 1st degree arson	7/1/1981
Illinois IL Ann. Stat. 405/5-801, 405/5-130	13 or older	1st degree murder committed during the course of either sexual assault or aggravated kidnapping	1/1/1983
	15 or older	Aggravated sexual assault, armed robbery, possession or sale of drugs at school or school event, if prior felony adjudication and forcible felony in furtherance of organized gang activity	
Indiana IN Code Ann. 31-6-2-1(d)	Any age	Murder, kidnapping, rape, robbery if committed while armed with a deadly weapon or it results in serious bodily injury	1/1/1982

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Iowa IA Code 232.8	16 or older	Any felony	1/1/1997
Kansas[a] KS Stat. Ann. 38-1602(b)(3)	14 and older	Felony, if prior adjudication	1/1/1983
Kentucky KY Code Ch. 635.020	14 or older	Any felony in which a firearm was used	1/1/2001
Maine ME Ch. 503 Sec. 3101	Any age	Murder, manslaughter, elevated aggravated assault, arson that recklessly endangers a person, robbery, sexual assault	1/1/1998
Minnesota MN Stat. Ann. 260B.125	16 and older	Murder	1/1/1995
Missouri MO Stat. 211.071	17	Any offense if the juvenile court already has jurisdiction over the child	1/1/1999
Montana MT Code Ann. 41-5-204	16 and older	Murder, sexual intercourse without consent, assault on a peace officer or judicial officer, arson, aggravated assault, assault with a weapon, robbery, burglary or aggravated burglary, aggravated kidnapping, possession of explosives, criminal distribution or possession of dangerous drugs, use of threat to coerce criminal street gang membership or use of violence to coerce criminal street gang membership, escape	1/1/1988

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Nevada NV Rev. Stat. 62.040	Any age 16 or older	Murder or attempted murder Sexual assault, or an offense involving the use of a firearm if previously adjudicated of a felony, a felony resulting in substantial bodily harm to the victim if committed on school property or the person intended to cause great bodily harm to more than one person	1/1/1996
New Mexico NM Stat. Ann. 32A-2-20, 32A-2-3	14 and older	1st degree murder	7/1/1993
North Carolina NC Gen. Stat. 7B-2200, 7B, 1604	13 and older 16 or older	Class A felony Any offense	1/1/1980
North Dakota ND Ses. Laws 27-20-34	14 or older	Murder, sexual assault, drug charges	1/1/1998
Ohio OH Rev. Code Ann. Tit. 21, 2151.26	Any age	Murder, 1st and 2nd degree felony if prior murder adjudication	1/1/1996
Pennsylvania 42 PA Cons. Stat. Ann. 6302, 6355	Any age 15 or older	Murder Sexual assault, aggravated assault, robbery, kidnapping, voluntary manslaughter	1/1/1996
Vermont VT Stat. Ann. tit. 33, 5506	Any age 14 or older	Murder Manslaughter, aggravated assault, armed robbery, kidnapping, aggravated sexual assault, aggravated burglary	7/17/1981
Virginia VA Code 16.1-269.1	14 or older	Murder, aggravated malicious wounding	1/1/1997

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Wisconsin WI Code 938.183	Any age	Assault or battery on an agent of the government while placed in a facility for delinquency	11/17/1995
	10 or older	1st or 2nd degree intentional homicide	
	15 or older	1st degree reckless homicide, attempted homicide	

[a] Kansas repealed its legislative waiver statute in 2003.

Appendix C. Final Models and Fit Statistics

Series	Final model	*Q*-statistic	*df*
Violent crime			
Alabama	(0,1,1)	33.88 $p < .47$	118
Alaska[a]	(0,1,1)	33.14 $p < .51$	118
Georgia	(0,1,1)	23.48 $p < .91$	118
Idaho	(0,1,1)	27.87 $p < .76$	118
Illinois[a]	(0,1,2)(0,0,2)s	30.29 $p < .50$	115
Indiana	(0,1,1)	41.60 $p < .17$	118
Ohio	(0,1,1)(0,0,2)s	41.16 $p < .13$	116
Nebraska	(0,1,1)(0,0,3)s	29.82 $p < .53$	115
Iowa	(0,1,1)(0,1,1)s	25.40 $p < .23$	116
Kansas	(0,1,1)	22.55 $p < .93$	118
Kentucky[b]	(0,1,1)	38.04 $p < .29$	118
Maine	(0,0,0)(2,1,0)s	29.07 $p < .14$	106
New Hampshire[a]	(0,1,1)	38.46 $p < .27$	118
Connecticut	(0,1,1)(0,0,1)s	34.32 $p < .40$	117
Minnesota[a]	(0,1,1)(0,1,1)s	21.49 $p < .49$	105
Missouri	(0,1,1)(0,1,1)s	23.32 $p < .33$	105
Nebraska	(0,1,3)	43.56 $p < .08$	116
Oklahoma	(0,0,1)(0,1,1)s	30.46 $p < .11$	106
Montana	(0,1,1)	34.42 $p < .45$	118
Nevada	(0,1,1)(0,0,1)s	31.22 $p < .56$	117
New Mexico	(0,0,1)(0,1,1)s	29.76 $p < .12$	106
North Carolina	(0,1,1)	32.62 $p < .54$	118
North Dakota	(0,1,1)	32.84 $p < .52$	118
Ohio	(0,1,1)(0,0,1)s	44.52 $p < .09$	117
Pennsylvania	(0,1,0)(0,0,1)s	41.40 $p < .18$	118
Vermont[a]	(0,1,1)	44.05 $p < .12$	118

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Virginia	(0,1,1)(0,0,3)s	35.29 $p < .36$	115
Wisconsin[a]	(0,1,3)	37.56 $p < .23$	116
Nebraska[a]	(0,0,1)(0,1,1)s	29.21 $p < .14$	106
Indiana[a]	(0,1,1)(0,0,2)s	43.72 $p < .08$	116

Notes.
(p,d,q)(P,D,Q)s:
p = auto-regressive process, d = differenced, q = moving average process.
P = seasonal auto-regressive process, D = seasonally differenced, Q = seasonal moving average process.
Q = Box–Jenkins test statistic for the null hypothesis that the models residuals are distributed as white noise.

[a] Log transformations were performed in order to make the series stationary in their variance.

[b] Because Kentucky's legislative waiver statute went into effect January 1, 2001 the model only included the 24 observations after the intervention.

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