Document Title: Forging a Florida Correctional Research Coalition: Evaluating the Impact of Florida's Habitual Offender Law, Final Report

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Document No.: 192995

Date Received: March 07, 2002

Award Number: 96-CE-VX-0007

192995

FINAL REPORT
of the
FLORIDA CORRECTIONAL RESEARCH COALITION
Submitted to the
NATIONAL INSTITUTE OF JUSTICE

FORGING A FLORIDA CORRECTIONAL RESEARCH COALITION: EVALUATING THE IMPACT OF FLORIDA'S HABITUAL OFFENDER LAW

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June 17,1999

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96-CE-VX-0007

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PROLOGUE

The Florida Correctional Research Coalition (FCRC) engaged in a variety of organizational activities during it's short duration, as well as the preparation of two additional research proposals. The FCRC was also involved in several short –term research projects which were aimed at providing information for the selection of one large-scale research project to conduct that seemed most timely, most useful, most important, and most realistic given the resources and time-frame of the grant.

The research project selected, which was the major activity of the grant, was concerned with the process of selective incapacitation, or, more specifically, an evaluation of Florida's habitual offender law. While the concept of selective incapacitation has been prominent in corrections for a long time, the amount of research aimed at trying to assess its impact is still very limited. This is due, at least in part, because few correctional programs lend themselves to an empirical assessment of the selective incapacitation effect.

Florida is one of the states that implemented an habitual offender statute which was aimed at providing a selective incapacitation effect. Additionally, The Florida Department of Corrections' (FDC) Bureau of Research has been one of the most progressive correctional research organizations in the country. The Bureau has also been one of the most advanced in the establishment of the kinds of databases that are required for conducting a research project such as the one implemented here.

The rest of this final report concentrates on the most significant research project conducted by the FCRC which was an in-depth assessment of the incapacitative effect of Florida's Habitual Offender statute. The FCRC is grateful to the FDC's Bureau of Research for making the data available for this study and for assisting in all phases of this project. We are also appreciative of the grant provided by the National Institute of Justice (NIJ) which made this research possible. However, the analysis, interpretation, and conclusions in this report are solely those of the FCRC and do not represent the positions or opinions of the FDC or NIJ.

CHAPTER 1

INTRODUCTION

Incapacitative Rationale

Many rationales for punishment have been used by the American criminal justice system with some being more prominent than others in particular eras. For a large portion of the 20th century the historical regard for incapacitation as the primary justification for imprisonment of offenders in American prisons was largely superseded by the rehabilitative ideal. At still other points deterrence has been a major focus in both correctional theory and penal policy. For one particular type of criminal, however, at least in recent years, incapacitation has served as the dominant justification for punishment. Incapacitation has been the most frequently cited rationale for the punishment of the "habitual" or "career" criminal (Zimring and Hawkins, 1995: 22-24). Habitual offender (HO) statutes, which typically provide for either mandatory or discretionary "sentence enhancements" for offenders who have extensive prior records, have been among the most popular crime-reduction strategies since the turn of the twentieth century.[1] One of the primary rationales for HO laws is to protect the public from known offenders. The logic of incapacitation suggests that by selectively incarcerating the most criminally active offenders for minimum or extended periods of time, crime levels should decline, as these offenders are

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[1] For example, the California legislature amended the penal code in 1927 to allow for enhanced penalties for offenders convicted of two previous convictions for certain offenses. Punishments included life imprisonment as an HO with no possibility of parole before the passage of twelve

unable to commit further crimes against the community (Blumstein, Cohen, Roth, and Visher, 1986; Zimring and Hawkins, 1995).

The popularity of HO laws regained prominence in the 1970's as many states amended or enacted new HO legislation aimed at repeat offenders (Bureau of Justice Assistance, 1996). In fact, forty-seven states, and the federal government had HO or recidivist laws in action by 1981 (Cooper, Kelley, and Larson, 1982). Renewed interest in this topic was spawned by a 1972 landmark study conducted on criminal careers by Wolfgang, Figlio, and Sellin (1972) entitled *Delinquency in a Birth Cohort*. Wolfgang and his associates tracked the criminal careers of a cohort of 9,945 boys from Philadelphia in 1945 from birth until their eighteenth birthday using official police records. The results of the cohort study revealed that a small percentage of delinquent youths (commonly referred to as "the chronic 6 percent") accounted for more than half of all reported crimes and approximately two-thirds of all violent crimes of the entire group.[2] Later cohort studies investigating career delinquency patterns supported the previous findings of the Wolfgang research (see generally, Wolfgang, 1983; Wolfgang, Thornberry, and Figlio, 1987; Shannon, 1988; West and Farrington, 1994).

More recently, state legislatures and the Federal government have once again augmented existing HO laws as part of a larger effort to "get tough on

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years, and life imprisonment with no possibility of parole for those convicted for a fourth felony (Zimring and Hawkins, 1995:23).

[2] Chronic recidivists were defined as those youths with five or more arrests during the time period studied. This resulted in 607youths being classified as chronic recidivists. These youths were

crime" and to get even tougher on repeat offenders (Stolzenberg and D'Alessio, 1997; Turner, Sundt, Applegate, and Cullen, 1995:16). These recently enacted laws provide for mandatory prison terms while severely restricting prosecutorial and judicial sentencing discretion in some manner (Clark, Austin, and Henry, 1997; Lyons, 1995; Turner et al., 1995). A 1996 study conducted by the National Council on Crime and Delinquency (NCCD) revealed that all states have some type of "sentencing enhancement" law that provides for add-on penalties for offenders with extensive prior records. Currently, the most popular of the HO laws fall under the rubric "Three-Strikes and You're Out" (Austin, Jones, Kramer, and Renninger, 1996). Between 1993 and 1995, 24 states and the federal government passed three-strikes laws (Clark, Austin, and Henry, 1997; Lyons, 1995).

Remarkably, while HO laws have existed in the U.S. for nearly a century, and are currently very popular under the highly publicized label "Three-Strikes and You're Out", there is a paucity of methodologically sound, empirical research assessing the efficacy of these laws. Thus, in spite of the popularity, it is far from clear whether or not the sentencing of repeat offenders to prison for extended periods of time is an effective crime-reduction tool.[3] To date, the only two

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responsible for 71 percent of the homicides, 73 percent of the rapes, 82 percent of the robberies, and 69 percent of the aggravated assaults.

[3] Other empirical research has focused on how and to whom HO laws are applied. For example, a report published by the Economic and Demographic Research Division Joint Legislative Management Committee (EDR) of the Florida Legislature revealed that Florida's HO law is applied more frequently to less serious offenders; is not used uniformly across jurisdictions; and is applied in a racially disparate manner with black offenders more likely to be sentenced as HOs than white offenders (EDR, 1992). Similarly, Crawford, Chiricos, and Kleck (1998) found a significant and substantial race effect in the application of Florida's HO law.

published studies to examine the HO law-crime relationship were conducted on California's "Three-Strikes" law (Stolzenberg and D'Alessio, 1997; Males, MacAllair, and Taqi-Eddin, 1999). Both of these studies concluded that California's "Three-Strikes" law has had no significant impact on serious crime levels. Studies which have examined other types of mandatory/discretionary "sentencing enhancement" laws (MDSE), for example, firearm "sentencing enhancement" laws (FSE), have generally found either mixed or no support for the MDSE law efficacy hypothesis.

Unfortunately, nearly all previous MDSE research has focused solely on the immediate to short-term deterrent and selective incapacitation effects. This implicitly assumes, however, that most offenders sentenced under MDSE provisions would not have received prison sentences in the absence of such laws. This is highly unlikely given the extensive criminal histories of most offenders sentenced under MDSE laws and the importance of prior record in criminal sentencing decisions, particularly in sentencing guidelines jurisdictions.[4] Mauer (1996:23), for example, has argued that

> if the three-strikes laws has had any impact on crime in California, it could not have been due to incapacitating more offenders, because virtually all of those imprisoned would have been incarcerated during this time anyway. The additional three-strikes years will add up later in their sentences. Therefore, any impact of the law must have been through deterrence.

Therefore, if this theoretical interpretation is correct, one would not expect the selective incapacitative effects of MSDE laws to appear until offenders serve that proportion of their sentences that would have resulted even in the absence of MDSE laws. Thus, for purposes of assessing the impact of Florida's HO law on crime rates it is not essential to know whether the HO law reduces crime rates immediately or over the short-term because these effects do not reflect the incapacitative effects of the HO law, but rather, of incapacitation in general. Rather, the long-term impact of HO sentencing on crime rates is assessed, while controlling for any initial or short-term effects which would have occurred even if Florida's HO law did not exist. Finally, the bulk of previous research on MDSE laws have relied on analytic procedures that cannot address simultaneity issues, and the possibility of spurious results due to omitted-variable bias.

Purpose of Study

The purpose of this study is to assess the impact of Florida's HO law on crime rates over the past seventeen years (1981-1997) using a methodological strategy that can address these theoretical and methodological concerns. Using individual-level prison admissions data from the Florida Department of Corrections (FDC), a county-level measure of the extra amount of prison time imposed on HO's as a result of the HO law is constructed (as described in Chapter 4)—and its lagged effect on crime is assessed using a multiple time-series (MTS) design. Also examined is the possibility that some "favorable effects" of HO sentencing are only immediate or short-term by including both HO

[4] Previous research on criminal sentencing consistently finds prior record to be one the most important causal variables of sentence length (Steffensmeier, Ulmer, and Kramer, 1998).

admissions and HO incarceration rate variables in the crime models. The study also investigates the extent to which the effects of the HO law varies across county populations, and variable configurations. If the evidence suggests that the additional time HO's receive as a result of HO sentencing does not consistently coincide with reduced crime levels, then one might conclude that Florida's HO law is ineffective as a crime control measure. Finally, the study mitigates specification bias by using the Granger causality test to address simultaneity issues between HO prison levels and crime rates, and by adding nearly 150 proxy variables to address omitted-variable bias. Both the Granger causality test, and the proxy variables are discussed at length in Chapter 4.

Overview of Report

Chapter 1 provides an introduction to the study and discusses the incapacitative rationale, the purpose of the study, and provides an overview of the report. Chapter 2 provides an historical account of Florida's HO law, a descriptive analysis of the impact of the HO law on correctional systems, and a profile of offenders typically targeted by the HO law. Chapter 3 presents a brief examination of deterrence and incapacitation theories as they relate to the present study. Chapter 4 provides a review and critique of the empirical literature on MDSE laws, specifically those studies which examine the impact of FSE laws on crime rates. The data and methods used in the present study are discussed in Chapter 5 and the findings are presented in Chapter 6. Finally, a brief summary, discussion, and conclusion section is presented in Chapter 7.

CHAPTER 2

HISTORY OF FLORIDA'S HABITUAL OFFENDER LAW

Early History of HO Laws

Since its adoption in 1927, the Florida HO law has undergone many dramatic changes. The original HO law allowed for enhanced and minimum sentences for repeat felony offenders.[5] The law was amended in 1971 so that the court could (but was not required to) impose harsher sentences on HO's for the protection of the public (EDR, 1992).[6] In the fall of 1986, the Florida Supreme Court ruled in Whitehead v. State that the existing statutes were in conflict with current sentencing guidelines.[7] Under the 1983 sentencing guidelines an offender's prior record was included among the sentencing factors determining recommended sentence length. As a result, the Florida Supreme Court invalidated the use of the HO law because prior record was essentially being counted twice, once under the newly created guidelines, and again in the HO statute. However, in 1988 the Florida Legislature passed legislation re-establishing enhanced sentences for offenders designated as habitual. These sentences were categorically exempt from the sentencing guidelines thus avoiding the conflict raised in the Whitehead decision (EDR, 1992).[8]

The 1988 amendment to the HO law led to several significant changes. Prior to this amendment, HOs were not eligible for provisional credits (i.e. early-

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[5] HOs were later made eligible for parole under the auspices of the Parole Commission which was created in 1941.
[6] Florida Statute 775.084
[7] 498 So.2d 863 (Fla. 1986).

release programs designed to maintain prison populations within federally prescribed limits), but they were allowed to earn basic gain-time which typically reduces an offender's sentence by one-third (Bales and Dees, 1992).[9] Under the 1988 amendment, basic gain-time was eliminated for HOs with offenses on or after October 1, 1988 (Florida Department of Corrections, 1997b). As a result, offenders sentenced under the HO law for offenses committed on or after October 1, 1988 could expect to serve an average of 75 percent of the sentence imposed. Conversely, non-mandatory offenders could expect to serve 30 to 33 percent of their court imposed sentences because of other gain-time mechanisms (i.e. incentive and meritorious gain-time) and an early prison release program which started in February, 1987 due to prison overcrowding (EDR, 1992; Bales and Dees, 1992).[10]

The Florida legislature eliminated the application of basic gain-time for all felony offenders with offenses committed on or after January 1, 1994 (Florida Statute 944.275).[11] In 1995 the Florida legislature passed a "truth-in-sentencing"

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[8] HOs were ineligible for parole following the 1988 amendment.

[9] HOs serving sentences for offenses committed on or between 7-1-78 and 10-1-88 were awarded basic gain-time (basic gain-time is non-discretionary) at the rate of 10 days per month for each month of each sentence imposed on them (Florida Department of Corrections, 1997b).

[10] Provisional credits (PCs) were awarded at a non-discretionary rate of 60 days per month for the time period January to June, 1990 (Bales and Dees, 1992). PCs replaced administrative gain-time (first prison population control program implemented in February, 1987 to maintain prison population within federally prescribed limits) in July, 1988. PCs were replaced by the Control Release Authority in January 1991 in order to provide a more discretionary early-release mechanism. The newly created early release program was managed by the Parole Commission (EDR, 1992). Incentive gain-time was awarded to inmates as earne, at a rate of up to 20 days for each month served from June 15, 1983 to December 31, 1993 (Florida Department of Corrections, 1997b).

[11] Early prison release programs (i.e. Control Release Authority) were discontinued in December 1994 due to a decline in prison admissions and a massive prison building campaign (Florida Department of Corrections, 1998a).

law, requiring all felony offenders with offenses dates on or after October 1, 1995 to serve a minimum of 85 percent of their court sentences regardless of the amount of incentive, meritorious, or educational gain-time earned.[12] As a result, the only significant difference between HOs and non-habitual offenders with respect to time-served is the length of the prison sentence. Despite the drastic increase in time-served for non-habitual offenders, prosecutors have not refrained from applying the HO provision. As seen in Table 2 the number of HOs sentenced to the FDC has increased since 1995.

The second major change in the HO law involved the process for determining HO status. Prior to 1988, the law required a separate judicial hearing to determine if meting out an extended sentence was necessary for public safety. A preponderance of the evidence was required to "habitualize" the offender. These requirements were eliminated in the 1988 amendment, making all offenders who meet the requisite statutory requirements eligible for a habitual sentence (Bales and Dees, 1992; EDR, 1992). As noted by the EDR (1992) study, if the state attorney requests that an eligible HO be sentenced under the law, then the sentencing court will usually impose the harsher sentence. However, if a judge believes that habitualizing an offender would not be in the interest of justice or public safety, he or she may refuse to impose the additional sentence allowed by state law (EDR, 1992). Conversely, a judge can determine if an offender is statutorily eligible for a habitual sentence, and can impose the

[12] Florida Statute 944.275(4)(b)3

enhanced penalty even if the state attorney does not charge the offender under the HO provision.[13]

Current Status of Florida's Habitual Offender Law

The 1988 amendment differentiated between two types of HOs—regular habitual felony offenders (regular HOs) and habitual violent felony offenders (VHOs). VHOs differ from regular HOs in two ways: (1) they need only have one prior felony conviction, and (2) among their prior offenses there must be at least one violent felony. The court may impose sentence levels equal to that of the regular habitual offender, but they may require the offender to serve a minimum portion of the sentence. The VHO law is rarely used because little is gained in the way of sentencing (amount of time-served is approximately the same for regular HOs and VHOs). Since FY 1992-93 only 11 percent (1,444) of all offenders categorized as HOs (12,756) have been sentenced under the VHO law (FDC, 1997). Therefore, no differentiation is made between regular HOs and VHOs in the present study. Table 1 provides a summary of the main sentencing features of the 1971, and 1988 HO statutes.[14]

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[13] Of the 25,806 individuals eligible for habitualization in the EDR (1992) study only 4,783 (18.5 percent) were sentenced as HOs. Unfortunately, the study does not report (probably due to data limitations) the percentage of requests for habitualization made by state attorneys which were granted/denied by judges.

[14] In 1995, the Florida legislature created another category of HO. The newly created "violent career criminal" statute targets offenders with three or more prior felony convictions or other qualified offenses and establishes minimum and maximum sentences the court must impose based upon the degree of crime committed. Violent career criminals were not included in the present study because the statute did not take effect until October 1, 1995, an inadequate "post-period" for a policy impact assessment. In fact, by the end of FY 1996-1997 only 71 offenders were serving sentences imposed under the Violent Career Criminal Statute (Florida Department of

Impact of Florida's Habitual Offender Law on Prison Systems

Table 2 shows the growth in admissions of HOs to the FDC from 1980 to 1997. Following the 1988 amendments there was a dramatic increase in the application of the HO law. Between 1980 and 1988, HO admissions were relatively infrequent, ranging from a low of 50 in 1987 to a high of 115 in 1988. During the first full year following the 1988 amendments, the number of inmates sentenced under the HO law reached 1,097 in 1989 and then increased to 2,635 in 1990. The peak year for HO admissions was 1992 when 3,033 offenders were admitted to the FDC. The number of inmates sentenced under the HO law has increased from 2,108 to 3,031, or by 43.8 percent, from 1990 to 1997.

Table 3 provides a comparison of sentence lengths and expected time-served for HOs and non-HOs. Table 3 shows that the average HO sentence is 12.5 years for HOs versus 4.9 for non-HOs. 72.2 percent of HOs received sentences greater than five years in comparison to only 27.3 percent of non-HOs. HOs can also expect to serve much longer periods of time in prison than non-HOs. 63.4 percent of the non-HOs served less than one year in prison compared to just 5.1 percent of the HOs. Further, 46.4 percent of HOs served more than 5 years in prison versus 7.5 percent of the non-HOs. Overall, HOs will serve an average of 9.6 years in prison compared to 2.5 years for non-HOs

The rapid increase in HO admissions, coupled with their longer prison sentences and longer time-served than non-HOs, has resulted in significant

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Corrections, 1997a). This represents 0.7 percent of the total HO population or 0.1% of the total state prison population.

increases of HOs in the prison population. Table 4 depicts the accumulation of HOs in Florida's prisons. At the end of FY1988-89, HOs accounted for only 1.5 percent of the prison population, but by FY1996-97 they constituted 16 percent of Florida's prison population. From FY1988-89 to FY 1996-97 the HO population increased an average of 1,125 inmates per year (or 61.2 percent). During this time period, the percentage of the prison population sentenced under the HO law steadily increased from 1.5 percent in FY 1988-89 to a high of 16.2 percent in FY 1992-93. It has since leveled off, ranging from 13.6 percent to 16 percent.

Profile of Inmates Sentenced Under the Habitual Offender Statute

Tables 5 and 6 presents a profile of inmates sentenced under the HO law from 1989 to 1997. It is important to note that the majority of HOs were sentenced for non-violent crimes (Table 6). In fact, less than a third (30.8 percent) of all inmates sentenced under the habitual offender statute were sentenced for crimes against persons. The typical HO is a black (70.8 percent) male (96.9 percent) between the ages of 20 and 39 (84.7 percent) and has one or two prior prison commitments (50.5 percent). The typical sentence length for a HO ranges from 7.4 to 11.4 years for non-violent offenses and increases dramatically to 13.4 to 34.6 years for violent offenses. Finally, the average expected time-served by HOs for violent offenses ranges from 10.6 to 28.2 years and decreases to 5.0 to 8.5 years for non-violent offenses.

Table 6 provides a detailed summary of the offenses for which HOs are convicted. The majority of HOs have been sentenced to prison for burglary offenses (27.8 percent), and drug offenses (22.7 percent). These findings are similar to those obtained by Clark, Austin, and Henry (1997) in their recent study of the impact of California's Three-Strikes Law on correctional systems. Their results reveal that the largest shares of two and three-strike offenders were convicted of burglary (14.1 percent and 18.7 percent, respectively) and drug offenses (31.6 percent and 21.9 percent, respectively).

CHAPTER 3

THEORETICAL CONSIDERATIONS

HO laws are intended to reduce crime through general and specific deterrence and through selective incapacitation. Marvell and Moody (1995) present the best theoretical framework on the topic of MDSE laws, and the following discussion draws heavily from their work.[15]

Deterrence Theory

One of the basic premises of general deterrence theory is that the offender accurately perceives the costs and benefits associated with a potential criminal act (Goves and Geerken, 1977). Thus, HO laws may deter crime as potential HOs learn about the law's practical application and fear the stiffer penalties. However, to the extent that criminals are not concerned about getting caught (*e.g.* confident about their own criminal abilities) or are misinformed about (or are unaware of) HO laws and their practical operation, the laws will have little impact on an offender's current decision to commit a crime. As Beha (1977:321-322) notes,

> we know relatively little...about how deterrence information is received and factored into any prospective offender's own "deterrence calculus". The sentencing stage is the very last stage in the detection, apprehension and sanctioning process. Whether a radical change in sentencing

[15] Although Marvell and Moody (1995) focus on the impact of firearm sentencing enhancement laws (FSE laws), their arguments regarding the theoretical link between FSE laws and crime are no less applicable in the present case. Both laws are designed to lengthen prison terms (either through mandatory or discretionary add-on sentences) for statutorily eligible offenders under a given law. Of course, the most obvious difference between FSE and HO laws is that the former targets offenders who commit crimes with a firearm while the latter targets high-rate offenders.

behavior will produce an impact on the attitudes of prospective offenders is unclear.

Moreover, HO laws will only have a substantial deterrent effect if the additional penalties provided under the sentencing mandate are greater in proportion to the penalties already imposed.[16] Stolzenberg and D'Alessio (1997) suggest that one of the reasons California's "Three-Strikes" law had no significant effect on crime rates was that most "three-strike" offenders were already receiving lengthy prison terms prior to the passage of the law. On the other hand, Florida's HO law does not allow offenders to earn basic gain-time, and the dramatic increase in length of time-served for HOs versus non-HOs (HOs will serve at least 75 percent of their court imposed sentence, compared to an average of 40 percent for non-HOs) may serve as a powerful deterrent for prospective criminals (Florida Department of Corrections, 1994).[17] Finally, as demonstrated in Chapter 5, HOs are subject to much longer prison sentences than non-HOs for all crime types (i.e. UCR index crimes) after controlling for all relevant factors of sentencing outcome.

HO laws may also have an impact on crime through specific deterrence. Specific deterrence theory proposes that offenders who receive a severe sanction are more likely to refrain from future criminal behavior than those experiencing a lesser sanction (Dejong, 1997). Thus, one might expect HOs,

[16] Feeley (1983) suggests that one of the reasons Massachusetts's Bartley-Fox law had no long-term effect on the criminal justice system was because it targeted offenders who by virtue of previously enacted sentencing enhancement statutes (e.g. armed robbery) were already expected to serve long prison terms.

[17] Unfortunately it was not possible to determine the average length of time-served for HOs before the passage of the 1988 HO amendment.

who serve longer prison terms, to react in several ways: leave the State of Florida, commit fewer crimes in the future, substitute less serious crimes (misdemeanors) for serious crimes (felonies), or desist from criminal behavior altogether. Recent research by Dejong (1997) suggests that the incarceration of experienced criminals did little to affect their probability of recidivism, but longer periods of incarceration significantly increased the amount of post-release time before they re-engaged in criminal behavior (i.e. time from custodial release until re-arrest for a new crime).[18] In the present case, this would suggest that Florida's HO law may reduce crime if released HOs delayed their return to crime.

It is also possible for HO laws to increase crime rates. If, Florida's HO law motivates chronic, habitual, or career criminals to avoid the greater sanctions, one might expect these offenders to substitute less serious crimes (misdemeanors) for more serious crimes (felonies). Because felonies tend to be more lucrative than misdemeanors, these offenders would have to commit more crimes if they wish to maintain their current lifestyles. As Marvell and Moody (1995:250) note, MDSE laws may increase crime levels "as criminals switch to more vulnerable, but less lucrative, victims."

The notion that deterrence reduces crime has been the subject of much debate (see, e.g. Blumstein et al., 1978). Moreover, as Vitiello (1997:441) explains, "deterrence arguments are notoriously difficult to assess, in large part because society is not set up to allow carefully controlled experiments." The

[18] Experienced criminals were defined as those offenders with one or more prior arrests before the current arrest.

empirical research to date generally supports the conclusion that certainty of punishment has a greater deterrent effect than severity of punishment (Blumstein, 1995). Blumstein (1995:408-409) comments:

> Research on deterrence has consistently supported the position that sentence 'severity' (that is, the time served) has less of a deterrent effect than sentence 'certainty' (the probability of going to prison).

For example, recent research by Marvell and Moody (1996) suggests that increasing police-levels reduces most crime types. Therefore, if certainty is more important than severity, then HO laws are utilizing the weakest link in the deterrence equation by allocating resources away from where they are considered most effective (Vitiello, 1997). Vitiello (1997) argues that:

> Resources are finite. One possibility is that the legislature will be forced to allocate resources away from law enforcement to prison construction and maintenance. But if that is the case, fewer street officers will lead to fewer arrests and less certainty of punishment. On the plausible assumption that resource allocation will be necessary, Three-Strikes may mean longer terms of imprisonment for offenders who will grow old (and less dangerous) in prison while younger offenders who will face less chance of being caught, and even when they are, they will face shorter terms of imprisonment until they run afoul of Three Strikes. The net result is that we may extend the career of younger felons while warehousing older felons.

Thus, from a deterrence perspective it seems only logical that more emphasis be placed on certainty of punishment while focusing less on the severity of punishment (Blumstein, 1995).

Selective Incapacitation

The third mechanism by which Florida's HO law is expected to reduce crime is through selective incapacitation. Indeed, "by locking them up and throwing away the key", HOs cannot commit future crimes against society. Thus, the link between selective incapacitation and crime reduction seems quite simple—by focusing scarce criminal justice resources on repeat offenders, significant decreases in crime can be realized (Haapanen, 1990; Stolzenberg and D'Alessio, 1997). While the mechanisms of selective incapacitation seem rather straightforward, the theoretical link between selective incapacitation and its impact on crime levels is considerably more complex than is frequently assumed.

As discussed in Chapter 1, the underlying rationale for amending/passing HO laws in the past two decades can mainly be attributed to the research findings of cohort studies investigating career delinquency patterns. These studies consistently find that a small number of high-rate offenders account for the vast majority of all crime. As many have already noted, however, these cohort studies have arrived at these conclusions regarding criminal career behavior retrospectively (Cohen, 1983; Gottfredson, 1999). That is, longitudinal cohort studies identified high-rate offenders after they had committed their criminal acts. On the other hand, selective incapacitative strategies, such as HO laws, rely on the ability of CJ officials to identify future high-rate offenders prospectively. However, criminologists generally agree that prosecutors, judges, and parole boards are rather poor at predicting which offenders are most likely to commit future criminal acts (Gottfredson, 1999). This problem was well defined by Gottfredson (1999:427):

> Most public policymakers are familiar with the fact that a small number of offenders commit a vastly disproportionate numbers of crime. By identifying and restraining this group of offenders, policymakers hope to have a significant impact on crime. But they may not know that the studies uncovering this important fact were all *retrospective* in nature; the small groups of offenders responsible for large numbers of crimes all were identified after the fact—that is, after they had accumulated long criminal records. Incapacitation policies, however, are *predictive* in nature. In order to be successful, they require that likely persons be identified before they have had a chance to commit numerous crimes.

This problem of prediction is especially problematic for HO laws because they typically target offenders after they have accumulated lengthy criminal records, and thus, in a sense are like cohort studies in that they identify high-rate offenders retrospectively--that is, when the vast majority of criminal acts have already taken place. Moreover, this identification occurs when repeat offenders are in their late 20s or early 30s, a time when criminal behavior begins to desist considerably (Gottfredson, 1999).

Second, if those sent to prison for a particular offense are simply replaced by new recruits (e.g. incapacitation of one drug dealer leads to another drug dealer taking his place) then the net-impact of individual incapacitation on aggregate-level crime rates will probably be negligible (Blumstein et al., 1986; Clear, and Barry, 1983; Levitt, 1995).[19] Similarly, even if HO laws produce more and longer prison terms, they may not reduce crime because many crimes have multiple offenders (e.g. gangs, organized fences) (Blumstein et al, 1978, 1986; Reiss, 1986; Marvell and Moody, 1994). Using data collected from the

[19] Clear and Barry (1983:539) suggest that the totality of the prison experience could increase crime in the post-release period by "aggravating characteristics" which resulted in the initial criminal behavior (see also Marvell and Moody, 1994:118).

1989 National Victimization Survey (NCVS), Marvell and Moody (1994) estimate that 21 percent of robberies were committed by two or more persons, 11 percent by three, and 11 percent by four or more. This suggests a co-offending rate of two offenders per crime. To the extent that other members of co-offending groups persist in their pattern of criminal activity, the net-impact of individual incapacitation on crime will probably be lowered (Blumstein et al., 1986; Zimring and Hawkins, 1995). However, co-offending is most common among juveniles and younger adults (Marvell and Moody, 1994), and the impact of co-offending on HO laws is probably minimal because the average age of admission for HOs is thirty (Florida Department of Corrections, 1998b).

Another limitation of selective incapacitation on crime levels pertains to correctional systems. If HO laws are fully enforced they may lead to overcrowded prisons (Bales and Dees, 1992; Kleck, 1991; Marvell and Moody, 1995) and force the early-release of violent and repeat offenders who were not sentenced under the HO law (Florida HOs are statutorily ineligible for any type of early-release program). In fact, Florida's prison system came under federally prescribed population limits from February 1987 to December 1994 resulting in thousands of inmates serving an average of 33 percent of their court imposed sentences (EDR, 1992). If, for example, the admission of HOs for drug offenses resulted in the early-release of highly active property offenders, then HO laws could actually increase crime levels.

The final limitation of HOs and crime levels deals with the relationship between age and crime (Stolzenberg and D'Alessio, 1997). As noted above, the effectiveness of HO laws as crime control measures is largely dependent on the length of criminal careers. Since participation in criminal activity begins to decline for most offenders by age 30 (Nagin, Farrington, and Moffitt, 1995) and the average age for HOs at the time of admission is 30 (Florida Department of Corrections, 1998b), it seems unlikely that incapacitating HOs past the age they are "at risk" to commit crime would significantly lower crime levels (Stolzenberg and D'Alesiso, 1997:466). Consequently, increases in the application of the HO law could result in drastic increases in the aging of the prison population while younger high-rate offenders remain free on the streets or are sentenced to less restrictive forms of community supervision (e.g. probation, electronic monitoring, house arrest). Conversely, research on duration of criminal careers suggests that offenders committing index crimes in their thirties (i.e. HOs) are likely to remain criminally active until they are in their forties (Blumstein et al, 1986) and, thus, the selective incarceration of HOs for extended periods of time could still have a crime-reduction impact, yet still probably less than would be achieved by incarcerating younger (in their 20's) offenders.

In all, the link between HO laws and crime is not as simplistic as the theoretical literature on deterrence and selective incapacitation would suggest. As Marvell and Moody (1995) note, present theory does not even allow one to specify the direction of the impact between mandatory sentencing laws and crime. The present study attempts to shed some additional light on these theoretical issues by empirically examining the relationship between Florida's HO law and crime. A review and critical assessment of the empirical literature on MDSE laws and crime is provided in Chapter 4.

CHAPTER 4

REVIEW OF THE LITERATURE

California's Three-Strikes Law

As noted in Chapter 1, there have been only two studies which have examined the impact of HO laws on crime rates (Stolzenberg and D'Alesiso, 1997; Males et al., 1999). Both of these studies were conducted on California's three-strikes law.[20] The first study, conducted by Stolzenberg and D'Alessio (1997), used an interrupted time-series design with ARIMA modeling techniques and estimated the deterrent/incapacitative effect of California's "Three-Strikes" law on total crime rates and on a control series measured as the petty theft rate. The analysis consisted of separate univariate ARIMA models for both the total crime rate and the petty theft rate for each of the 10 cities covering the years 1985 to 1995.[21] While three intervention point models were estimated, the authors decided to use the abrupt permanent change model (i.e. date the law went into effect) because it provided the best fit to the data.[22] The results suggest that California's "Three-Strikes" law has had no significant impact on

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[20] The only other study to examine the impact of HO laws on crime levels was a projection analysis conducted by Greenwood et al. (1996) on the incapacitative effect of California's three-strikes law. Using a poisson-based model the authors simulated the flow of criminals through the criminal justice system for 25 years. The results of the projection analysis suggest that over the next 25 years California's three-strikes law will reduce serious felonies by 23 to 34 percent with the greatest crime reduction impact being observed for burglary and assault. The authors suggest that the analysis probably underestimates the impact of the law on crime rates because it did not account for general deterrent effects.

[21] The following 10 cities were included in the analysis: Fresno, Long Beach, Los Angeles, Oakland, Sacramento, Anaheim, San Diego, San Francisco, San Jose, and Santa Ana.

[22] Other intervention time points considered by the authors included a gradual impact model in which the impact of the law increases gradually over time and an abrupt temporary model which assumes an immediate short-term impact with crime levels returning to preexisting trends over time.

reducing total crime levels with the exception of Anaheim which witnessed a significant crime reduction (16 percent) following passage of the three-strikes law. The authors attributed the finding for Anaheim to random chance given the lack of significant negative coefficients for the other nine cities. These findings closely match the results of a recent analysis of California's "Three-Strikes" law. Males et al. (1999:9) examined the percentage change in crime before (1991-1993) and after (1995-1997) the passage of the law in California's 12 largest counties. The authors report that counties which have enforced the "Three-Strikes" law more strictly than others have not witnessed greater reductions in crime:

> Data clearly show that counties that vigorously and strictly enforce the "Three-Strikes" law did not experience a decline in any crime category relative to more lenient categories. The absence of any difference in relative crime rates occurred despite the fact that the six largest counties applied the law at a rate 2.2. times greater than the six counties that invoked the law least.

And whereas Stolzenberg and D'Alessio (1997) used ARIMA analysis with various intervention points, they (justifiably) included a measure which more closely resembles the extent to which law is enforced—the number of offenders sentenced under the "Three-Strikes" law per 1000 violent crime arrests. As discussed below, however, both of these studies are subject to a variety of theoretical and methodological flaws and are not suitable for drawing any type of substantive conclusions regarding the effectiveness of HO laws.

Given the paucity of scholarly research on HO laws, the literature reviewed here includes studies which have evaluated other types of MDSE laws.[23] Most research concerning any single type of MDSE law has pertained to FSE laws. FSE laws typically mandate minimum sentences or significantly enhance penalties for crimes committed with a firearm. FSE and HO laws are similar in that both are expected to reduce crime levels through general deterrence (fear of imprisonment) and incapacitation (extended prison terms). Moreover, both types of laws are directed at a small proportion of offenders who are generally considered to be responsible for the vast majority of their respective crime types. The following section provides an in-depth examination of studies conducted on FSE laws and identifies the more common theoretical and methodological problems associated with these studies.

Research Methods in Prior Studies

FSE evaluations have generally utilized one of the following three research designs: (1) interrupted time-series design, (2) cross-sectional design, and the (3) multiple time-series design. The present review includes studies which have employed any of these research designs to test the effectiveness of FSE laws.

Interrupted Time-Series Designs: The most common form of research methodology employed to evaluate the efficacy of FSE laws has been the interrupted time-series design with a univariate auto-regressive moving average

[23] The term "mandatory sentencing law" as used in this analysis also refers to sentencing enhancement laws.

(ARIMA) model.[24] In fact, eight of the eleven FSE law studies reviewed in Table 2 have employed this technique. ARIMA modeling requires the examination of patterns in monthly or annual violence rates for a single jurisdiction. If the statistical pattern demonstrates a downward shift in violent crime around the time the FSE law was introduced, then the case strengthens for concluding that the FSE law had an impact on reducing violence levels. Put simply, the observed change in crime levels cannot be attributed only to preexisting trends.

Despite the extreme popularity of the ARIMA time-series method in FSE law evaluations, it is subject to a number of serious specification problems. First, ARIMA time-series analysis does not directly measure any determinants of crime. Therefore, researchers who attribute a decline in violence levels to the FSE law are unable to rule out rival explanations of the decline because they do not control for third variable influences. Campbell and Stanley (1967) suggest that these third variable influences, which are commonly referred to as "history", can threaten the validity of inferences drawn from interrupted time-series designs.[25]

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[24] Pierce and Bowers (1981) use another form of longitudinal analysis—year before and year after percentage change differences (B-A analysis). The authors attempt to demonstrate a causal relationship between Massachusetts's Bartley-Fox law and reduced violence levels by comparing percentage differences in violence levels before and after the passage of the law. While this method may be useful for exploring general patterns of violent crime, B-A analysis should not be used to explain causal relationships because it fails to account for third variable influences which may contribute to any noticeable changes in violence levels.

[25] Often researchers attempt to control for third variable factors (historical threats) by using "control" jurisdictions with which to compare the area under study. As Kleck and Patterson (1993) comment, "analysts make do with comparisons to control jurisdictions which, it is assumed, would show crime trends similar to those in the intervention jurisdiction, were it not for the impact of the gun law changes" (p.251). Two of the ten longitudinal analyses reviewed made use of control jurisdictions (Pierce and Bowers, 1979; McPheters et al., 1984).

Another approach used to reduce the potential impact of historical threats is to compare trends in gun violence with tends in non-gun violence. The most obvious problem with this tactic is that researchers necessarily assume that trends in gun and non-gun rates would follow similar patterns were it not for the FSE law. Any declines in gun violence levels which are greater than those witnessed for non-gun violence levels (control series) is considered evidence that the FSE law reduces violence. This is a strong assumption, given that gun and non-gun violence rates are likely to be influenced by different sets of exogenous variables even in the absence of the FSE law. For instance, Loftin and McDowall (1984:257) comment that the use of unarmed robbery as a control series for armed robbery in their evaluation of Florida's FSE law was flawed because both crime series were probably influenced by disparate causal processes. Thus, comparisons of gun and non-gun violence rates are completely speculative, and should not be used to draw any type of substantive conclusions about the impact of FSE laws.

The most critical flaw with ARIMA time-series analysis is the difficulty associated with specifying the most appropriate intervention point for the FSE law. Typically, researchers have used the law's "effective date" as the intervention point. Other analysts have suggested earlier time points due to an "announcement effect" (*e.g.* Pierce and Bowers, 1981). Kleck (1991:387-388) highlights possible intervention time points researchers could consider when evaluating an FSE law. They include: time points when the FSE law was first publicly proposed, signed into law, first heavily publicized, its effective date, first enforced, and first enforced in a publicized way. For example, McPheters and his colleagues (1984) concluded that the Arizona FSE law caused gun robbery rates to decline in Maricopa county even though the downward shift began six months before the law was passed (*i.e.* announcement effect). There are a host of possible candidates when it comes to specifying an intervention time point, making it extremely difficult to falsify the FSE law effectiveness hypothesis (a more thorough critique of uninterrupted time-series evaluations can be found in Britt, Kleck, and Bordua, 1996).

Cross-Sectional Designs: Cross-sectional designs allow researchers to elaborate on the causal theory or mechanisms regarding the relationship between homicide rates and other social, economic, demographic, and legal factors. By comparing many legal jurisdictions (*e.g.* cities, counties, states) with or without a certain type of FSE gun law, researchers are able to determine whether those areas with a certain type of FSE law experienced lower levels of violence than those without the law or a different type of FSE law (Kleck and Patterson, 1993).

Unlike ARIMA time-series analyses, cross-sectional designs are able to control for many other determinants of crime rates. For example, poverty, unemployment, and income inequality are just a few of the factors that have been associated with higher violence rates (see Kovandzic, Vieraitis, and Yeisley, 1998 and the research reviewed therein). This information is readily available to researchers for decennial census years for cities, counties, SMSA's, and states. Thus, cross-sectional designs can account for difference in crime rates due to factors which coincide with the existence of FSE gun laws, and therefore rule out any rival hypotheses. Further, Kleck and Patterson (1993) argue that it is possible to disentangle the effects of FSE laws which are sometimes lumped together with other forms of gun control to create a single new gun law, or enacted at the same time as other separate independent gun laws, while this is impossible with longitudinal designs.[26]

In addition, cross-sectional designs require researchers to make only weak, plausible assumptions as to where the most pronounced effects for a FSE law took place rather than much stronger assumptions about when the effect took place, as is the case with ARIMA time-series analyses. As discussed above, ARIMA time-series analyses are subject to numerous problems associated with determining when the effects of a new gun law become most apparent. With respect to cross-sectional designs it is quite clear that the impact of a FSE gun law should be expected in the area where the law was implemented (Kleck and Patterson, 1993). Furthermore, cross-sectional designs can also serve as complements to single-jurisdiction ARIMA time series designs, given the relative strengths and weaknesses of each design (Kleck, 1991:389).

Cross-sectional designs are also subject to some weaknesses. First, if the researcher fails to include other extraneous variables in the model which affect crime rates, the estimates obtained for the FSE gun law variable would be biased. In other words, the researcher would falsely attribute declines in

[26] For example, Massachusetts's Bartley-Fox Law that enhanced penalties for illegal carrying and mandated a two-year minimum sentence for possession of a firearm during the commission of a

violence levels to the FSE law when the change was due to omitted third variable factors (i.e. spurious correlations). Another problem with cross-sectional designs is their inability to detect short-term impacts (Marvell and Moody, 1995:256). For instance, if a certain FSE law were successful in lowering violence rates for only a short period of time, it would be impossible for the cross-sectional design to detect this impact given that cross-sectional designs only examine differences across jurisdictions for one point in time, typically a decennial census year.

Multiple Time-Series Designs (MTS): The MTS design, which has been widely used in applied econometrics, is considered one of the best designs for social science evaluation (Berk et al., 1979; Campbell and Stanley, 1967: 55-57). One strength of this type of model is that it allows for more degrees of freedom, and thus enhances the power of statistical tests of significance; another is that it allows for the evaluation of many separate legal changes (Marvell and Moody, 1995:257).[27] Despite the clear advantages of MTS designs over time-series and cross-sectional studies, only one of the eleven FSE studies reviewed utilized a MTS design (Marvell and Moody, 1995). This study provides the most persuasive and convincing evidence regarding the effectiveness of FSE laws.

Results of Previous Research

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felony.

[27] The multiple time-series (MTS) design also provides control groups. In this model each unit (i.e. city, state, or county) acts as a control for the other. The design also enables researchers to control for missing variables that are not included in the model and to detect any short-term impacts that might have occurred on violence rates (see Marvell and Moody 1995: 257-258 for a more complete discussion on the advantages of the MTS design).

A review and critique of the research on FSE laws as well as the two earlier studies on California's "Three-Strikes" Law are summarized in Table 7.[28] The first set of studies to evaluate the impact of FSE laws were conducted on Massachusetts's Bartley-Fox law. In fact, five of the twelve studies reviewed in Table 7 are evaluations of the Bartley-Fox law (Beha, 1977; Deutsch and Alt, 1977; Deutsch, 1981; Hay and McCleary, 1979; Pierce and Bowers, 1981). The law mandated a two-year minimum sentence for crimes committed with a firearm and/or a one year mandatory penalty for individuals carrying a gun without a license (Beha, 1977; Pierce and Bowers, 1981). These studies generally employ simplistic research methodologies using before-after percentage change data analysis (Beha, 1977; Pierce and Bowers, 1981) and ARIMA time-series analysis techniques (Deutsch and Alt, 1977; Deutsch, 1981; Hay and McCleary, 1979). In addition, researchers have failed to separate the effects of the FSE provision from the mandatory penalty for illegal gun carrying, making it impossible to determine which part of the law effected violence levels (Marvell and Moody, 1995). The results of these studies are mixed, with researchers finding support for both increases and decreases in homicide, robbery, and assault following passage of the law (Kleck, 1991:391-392).

Other studies have examined the impact of FSE laws in single jurisdictions (Loftin, Heumann, and McDowall, 1983; Loftin and McDowall, 1981, 1984; Loftin and Wiersema, 1992; McPheters et al., 1984). McPheters et al.

[28] See also Kleck (1997:Ch. 11) and Marvell and Moody (1995:252-256) for comprehensive reviews of FSE law studies.

(1984) evaluated the impact of Arizona's FSE law on gun robbery rates using monthly data for Maricopa and Pima counties. The results of the ARIMA analysis suggest that gun robberies declined significantly after the law became effective with little or no noticeable change in non-gun robbery and property crimes. Also, the authors found no significant declines in gun robbery rates using five other southwestern cities as control jurisdictions. Marvell and Moody (1996) are critical of the findings, suggesting that the apparent decline in gun robberies can simply be attributed to a 75 percent increase in gun robberies before passage of the law and the resultant return to historical levels in the following years.[29] Further, Marvell and Moody (1996) note that the decline in gun robbery rates for Maricopa county began six months before the law became effective. This suggests that gun robbery rates were simply returning to historical levels before the laws effective date and continued to decline thereafter. As noted above, concluding that a particular law is effective in reducing crime, regardless of when the impact occurs, makes it nearly impossible to falsify any type of FSE law effectiveness hypothesis.

One of the most comprehensive studies on FSE laws was conducted by Kleck and Patterson (1993). The authors used a cross-sectional research design to assess the impact of FSE laws, as well as 17 other major forms of gun control restrictions, on gun prevalence and all major forms of violence which

[29] Kleck and Patterson (1993:251) also point out that communities are most likely to respond to a particular crime problem when it is approaching or at its peak. Thus, crime levels are likely to decline to historical levels regardless of any legislative or community efforts made to combat the crime problem.

typically involve guns: homicide, suicide, fatal gun accidents, robbery, assaults, and rape. Data for 170 U.S. cities (over 100,000 population) were collected and coded for both mandatory and discretionary FSE laws. Violence models were estimated with two-stage least squares methods in order to deal with the two-way relationship between gun prevalence and violence levels.

Findings indicated that FSE laws may reduce total rates of homicide and robbery. Surprisingly, the supportive results obtained in the analysis were not for mandatory FSE laws but rather for discretionary FSE laws. In fact, discretionary FSE laws were among the few types of gun control laws that were associated with lower violence rates. This is why the results of the Kleck and Patterson (1993) study are considered "mixed" in Table 7.

Perhaps the most authoritative evaluation of FSE laws was conducted by Marvell and Moody (1995). By using a multiple time-series design (MTS) with a "fixed-effects" model, the authors mitigated many of the methodological problems encountered by previous FSE law studies. Marvell and Moody (1995) collected data for nearly all states over two decades (1980-1993) and estimated the direct and indirect effects of FSE laws on serious crime levels.

The results for the prison analyses suggested that FSE laws did not have an indirect effect on violence levels through increased prison admissions, court commitments, or prison populations. In addition, the authors constructed several short-term, long-term, and distributed lagged variables to test for any short-term or delayed impacts of FSE laws on prison levels. However, in separate regression analyses the authors found no evidence of a short-term or delayed impact for FSE laws on prison levels.

The initial set of crime regressions estimated the direct effect of FSE laws on homicide rates. The results of the homicide analysis produced coefficients far from significant. Specifically, the FSE law coefficients were non-significant in regressions for total homicide, gun homicide, and non-gun homicide. Likewise, the results for the short-term and distributed lagged variables were also non-significant in all homicide models. Finally, crime regressions for the remaining UCR crimes: rape, assault, robbery, burglary, larceny, and auto-theft displayed little evidence that FSE laws lead to lower crime levels. The authors also found no impact of FSE laws on gun assaults, and gun robberies when they restricted the analysis to states with enough data. In sum, there was little evidence that FSE laws lead to increased prison admissions, court commitments, or prison populations, and did little to reduce gun or overall crime levels.

Conclusions From the Literature

The 13 studies reviewed provided little evidence that MDSE laws reduce crime. Only 2 of the 13 studies find significant reductions in crime levels following the passage of a MDSE law. On the other hand, 10 of the 13 studies found either mixed or no support for the MDSE law efficacy hypothesis. With few exceptions (Kleck and Patterson, 1993; Marvell and Moody, 1995), the studies presented in Table 2 did little to mitigate specification problems. These specification problems include: the use of unrealistic intervention time points, the lack of relevant control variables, and the inclusion of inappropriate control jurisdictions or crime control series.

Perhaps the most serious problem with previous MDSE research deals with the use of inappropriate intervention points. This has been a problem of all MDSE studies, regardless of the research design implemented. With the exception of Marvell and Moody (1995), none of the MDSE studies included lags extending beyond one-year. As noted in Chapter 1, however, MDSE laws are primarily designed to lengthen prison terms, rather than to incarcerate individuals who would otherwise not be sentenced to prison upon conviction. This is probably true of most MDSE laws, given that the laws are designed to target the select few offenders who are responsible for the majority of crimes in each of their respective crime categories. Thus, when researchers include dummy law variables with no lags (i.e. immediate impact) or dummy variables lagged a couple of years (i.e. short-term impact), they implicitly assume that most offenders targeted by MDSE laws would not have been sent to prison prior to the passage of such laws.

In the present case, however, this assumption is completely inaccurate as most offenders sentenced under Florida's HO law would have received prison sentences even without the HO provision. For example, 80 percent of those "habitualized" in FY1996-97 scored to a mandatory prison term under Florida's 1995 sentencing guidelines. An additional 17 percent scored in the discretionary range and could have received prison sentences. This suggests that between 80 to 97 percent of those "habitualized" in FY1996-97 would have received prison sentences even if the HO law did not exist. Of those who could have received prison terms (i.e. offenders scoring to a mandatory or discretionary prison term), 75.2 percent could have been sentenced to 3 years or greater, 61 percent to 5 years or greater, and 18 percent to 10 years or greater (Florida Department of Corrections, Bureau of Research and Data Analysis). These results are completely consistent with those obtained by Marvell and Moody (1996) in their comprehensive examination of FSE laws. As noted above, the authors failed to find any significant effects of FSE laws on prison admissions, populations or court commitments. If FSE laws are intended to incarcerate gun criminals who normally do not receive prison terms, then one might expect prison levels to increase substantially over the short-term. This was not the case as all short-term lags (one, two, and three-year lags) failed to yield any significant effects of FSE laws on prison levels. This suggests that judges were already sentencing the vast majority of gun criminals to prison even before the passage of FSE laws. Finally, previous research on "career-criminal" prosecution programs reveals that over 90 percent of the criminals who would be classified as dangerous repeat offenders are sentenced to prison prior to the implementation of such programs (Walker, 1994).

Consequently, a measure which reflects the increased prison time given to offenders under Florida's HO law must be included if one wishes to assess the actual deterrent/selective incapacitative impact of Florida's HO law. For example, if offenders would have been sentenced to five years without the HO law, but receive additional years under the new law, additional incapacitative effects due to the new law would not start to show up until after five years. If the law fails to produce a reduction in crime levels during this extended period of incarceration then one might conclude that Florida's HO law is ineffective as a crime-reduction measure.

The present study avoids the problems described above by using the multiple time-series design with recent advances in econometric time-series analysis. Also, more nuanced measures of HO growth are included to estimate the short and long-term impact of Florida's HO law on crime rates. A complete description of these measures is provided in Chapter 5. The result is a more accurate estimate of the impact of HO growth than estimates derived from dummy variables.

CHAPTER 5

RESEARCH METHODS

Research Design

The present study utilizes a multiple time-series (MTS) design, pooling annual data from all Florida counties for seventeen years, covering the time period 1981-1997.[30] The MTS design is a derivation of what is commonly referred to as the pooled time series-cross-sectional design (Berk et al., 1979:387; Marvell and Moody, 1995:257; Pindyck and Rubinfield, 1991:202-203). Unlike other pooled designs which include cross-sectional variation (e.g. random-effects model), the MTS is unique in that it only utilizes time-series variation. The data are sorted by unit (i.e. county) and then by year within unit. That is, the first 17 rows contain over-time observations (i.e.1981-1997) for the first county, the second 17 rows contain the over-time observations for the second county, and so forth until 1139 rows are filled (17 years x 67 counties).

According to Campbell and Stanley (1967), the MTS design is considered an excellent evaluation design because of its feasibility and ability to mitigate internal validity threats. It has several advantages over the more commonly used time-series or cross-sectional designs. These advantages include: (1) the design provides for a very large sample size, which enhances statistical power while allowing one to enter numerous control variables and still retain a large

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[30] The Florida Department of Corrections computerized database for inmate admissions and status populations begins in FY1979-80, however, FY1979-80 was deleted because data for HOs were not readily available.

number of degrees of freedom; (2) it provides control groups in that for each county the others act as controls; and (3) it allows one to enter proxy variables for unknown factors (i.e. omitted-variables) which cause the dependent variable to vary over time and over counties. The proxy variables are discussed more fully below.

Ordinary-least squares (OLS) is not appropriate with pooled data because observations in a unit are not independent, and thus violate a basic assumption of regression analysis (Pindyck and Rubinfield, 1991; Marvell and Moody, 1996:620). Moreover, the OLS procedure makes the unreasonable assumption of constant slope and intercept (Pinkyck and Rubinfield, 1991:224). In order to address this problem, the current standard method for multiple time-series analysis, is to employ least squares procedures with dummy variables or "LSDV" (Berk et al., 1979; Hsiao, 1986; Marvell and Moody, 1995; Mundlak, 1978; Pindyck and Rubinfield, 1991). This is often referred to as an analysis of covariance with "fixed-effects" (Berk et al, 1979). Covariance analysis involves the addition of dummy variables ("fixed effects") for each county and each year, except the first year and county. The inclusion of unit (i.e. county) dummies eliminates any cross-sectional variation between counties. The result is a pooled time-series regression in which each county is allowed to have its own intercept but to share the slope coefficients with the other counties (Marvell and Moody, 1994; Pindyck and Rubinfield, 1991).[31]

Data Transformations

Stationarity tests were conducted using the augmented Dickey-Fuller (ADF) test (Enders, 1995). Following Marvell and Moody (1996), the ADF tests were conducted with two lags of differenced dependent variables. The ADF tests resulted in a rejection of the null hypotheses of non-stationarity in levels for all crime and independent variables. With no exceptions, the (absolute) critical values are well above the .01 critical value of 3.4. Since the ADF test reveals that the series are stationary in levels suggests that the analysis should be conducted in levels and, thus, first-differencing variables is not appropriate.

All of the continuous variables are expressed as natural logs to reduce the impact of outliers, and are divided by population so that large counties do not dominate the results (Marvell and Moody, 1995).[32] This procedure of taking logarithms allows the coefficients to be interpreted as elasticities. The coefficients are interpreted as the percentage change expected in the dependent variable from a one percent change in the independent variable (estimated at the means of the two variables).

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[31] The dummy variables should not be omitted from the regression model (unless they are insignificant as a group) because the parameter estimates of the remaining variables will be biased (Marvell and Moody, 1995:257; Pindyck and Rubinfield, 1991:226).

Heteroscedasticity, a common problem with cross-sectional data, arises when the variance of the error term is not constant. Correcting for heteroscedasticity is necessary in order to avoid inefficient (although unbiased) parameter estimates, and biased estimated variances for these parameters. Heteroscedasticity was detected using the modified Glesjer test. Weighting the crime regressions by the square root of county population eliminated the problem of heteroscedasticity.

Time-series data is often plagued with the problem of autocorrelation. Autocorrelation occurs when the error terms from a regression model are systematically related to themselves and, thus, result in inefficient (although unbiased) parameter estimates, and inflated *t*-ratios (Marvell and Moody, 1996). However, in the present study it was not possible to correct for autocorrelation using any of the typical correction procedures (e.g. lagged dependent variables, first differencing, and applying separate first-order serial correlation coefficients) due to the lagged HO variables which shorten the length of the time-series considerably. According to Marvell and Moody (1996:621),

> These procedures, as well as entering a lagged dependent variable, run the risk that the lagged dependent variable (on either the right or left side) is probably correlated with the error term. The impact on a lagged dependent variable is a negative bias on the coefficient, and a corresponding net positive bias is distributed among the remaining independent variables. The bias is substantial only when the time series is short. There are no standards for minimum length, but to our knowledge all discussions of the problem are limited to data sets with 10 or fewer time units (p.621).

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[32] For all of the continuous variables, if the true rate equals zero, a 1 was added to all observations (including those with a nonzero rate) before the natural log transformation.

Marvell confirmed this suspicion in personal communication and suggested that this positive bias on the independent variables could extend beyond the arbitrary 10 year cut-off. Thus, correcting for autocorrelation could push the parameter estimates for the HO measures upward (that is, lower a negative coefficient) and bias the results against finding a relationship between the HO measures and crime rates. Thus, the models are estimated without correcting for autocorrelation. One implication of this is that the corresponding *t*-ratios for each independent variable are probably inflated—making significance tests easier to pass and thus favoring the HO law effectiveness hypothesis. Thus, to avoid use of a corrective measure that would work against the hypothesis, a deliberate choice was made that probably favors it, and later, lagged dependent variables are added as one of the robustness checks.

Collinearity is not a problem with the key independent variables or other specific control variables, however, there is multicollinearity between the fixed-effects. This does not impact the results regarding the key independent variables (i.e. HO measures). Perfect collinearity among each set of dummy variables is avoided by dropping one year dummy (1981), one unit dummy (Alachua county), and one set of county trends (Alachua county).[33]

Proxies for Unknown Variables

[33] Failing to drop one year dummy, one unit dummy, and one county trend would result in the first dummy variable from each set of dummy variables being perfectly collinear with the intercept (Marvell, personal communication, 1998).

One of the most significant advantages to the MTS design is the use of proxy variables in order to mitigate the problem of omitted-variable bias (Marvell and Moody, 1996). This requires the entering of year dummies, county dummies, and county trends to serve as proxy variables for these omitted variables. The year and county dummies are an integral part of the fixed-effects model in that they partially control for variables not entered in the analysis.[34] The county dummies control for factors specific to a particular county that raise or lower crime levels (not captured by the specific control variables). As Marvell and Moody (1996:622) note,

> The unit dummies control for unobserved heterogeneity among units...They are similar to dummies for southern states or cities often entered into cross-sectional crime regressions, but they have the benefit of capturing differences between individual units rather than groups of states or cities.

The year dummies control for specific factors that raise or lower crime levels in a given year across all counties. The year dummies are similar to the linear time trend variable typically entered in time-series analyses to control for changes over time, but year dummies have the advantage in that they do not assume a linear trend (Marvell and Moody,1996). Specifically, year dummies are separate variables (one variable for each year, except the first) which rise or fall without a pre-set pattern, whereas the time trend variable is a single variable which assumes a linear trend or other set pattern (Marvell and Moody, 1995).

[34] The unit and year dummies control for unknown factors (omitted variables) that move the mean for a particular county or year from the overall mean (Marvell and Moody, 1995:263).

The final category of proxy variables includes separate trends for each county, and are similar to time trend variables routinely put in single time-series (Marvell, 1998, personal communication). That is, each county has its own trend variable, which equals 1 in 1981, 2 in 1982, and 17 in 1997. Specifically, the trend variables control for trends in a county that depart from the trends captured by the year dummies. In other words, they account for factors that make crime levels in one area grow more or less than statewide trends, for which the year dummies control (Marvell and Moody, 1996). As Marvell and Moody (1995:273) note,

> criminal justice systems and crime incentives are not static within a state, and the year and unit dummy variables do not control for factors that effect only some years in a particular state........and a myriad of other local changes affect prison population trends and crime rate trends that differ from the nationwide trends, for which the year dummies control.

Although the deterministic trend can be any power of a linear trend, (e.g. quadratic trend) the present study includes the most common—the linear trend (Marvell and Moody, 1996). A quadratic trend (linear trend squared) is later entered into the crime regressions along with the linear trend as another robustness check. The quadratic trends serve as a proxy for factors that are changing even faster than those picked up by the linear trends.

Data and Variables

Crime Measures: Crime is measured by the UCR index crimes during the period 1981-1997 (calendar year data). The FBI crime reports include seven categories of crime: murder, rape, aggravated assault, robbery, auto theft, burglary, and larceny. All of the crime data are divided by county population and are expressed in rates per 100,000 population. Crime data were obtained from the Florida Department of Law Enforcement on computer disk.

Many researchers have questioned the validity and/or utility of the UCR crime statistics due to the number of crimes not reported to the police. This is especially true when comparing official crime levels to those obtained in victimization and self-report surveys (Gove, Hughes, and Geerken, 1985). Despite low reporting rates for most of the index crimes, they have been relatively consistent since 1975 (Bastian, 1993). Fortunately, the unit and year dummies partially control for statewide trends in underreporting and for differing reporting biases by individual counties. Furthermore, the UCR crime statistics are considered valid indicators of the more serious crimes within each crime type (Gove, Hughes, and Geerken, 1985).

Habitual Offender Measures: As noted briefly in Chapters 1 and 4, the present study does not focus solely on the immediate and short-term effects of HO incarceration on crime levels, but instead includes a more technically relevant measure of the extra amount of prison time imposed on HOs which can be specifically attributable to the HO law. Thus, the first two HO measures, while they do not measure the impact of the HO law, determine whether any contemporaneous or short-term causation exists between imprisoning HOs and crime levels. It is reasonable to assume, however, that if locking up HOs has no short-term impact on crime levels, there is probably no impact during the extra prison time offenders serve as a result of the HO law.

The first measure included in the crime models is a HO admissions variable (HOA). The variable is defined as the number of HOs admitted to the FDC during the calendar year divided by county population. The HOA variable estimates the immediate (current-year) deterrent/incapacitative impact of locking up HOs in a given county on crime rates. In the present case, it is suggested that the HOA effects are probably predominantly deterrent effects because the law is more likely to have a substantial deterrent impact on crime the more the law is enforced. One would expect counties with higher HOA rates to have lower crime levels than counties which use the law infrequently or not at all. For example, criminals may learn of the greater sanctions imposed on criminal associates as a result of the HO law, and be motivated to escape these sanctions by desisting from crime all together or avoiding, or committing fewer, offenses that would qualify them for habitualization. Prospective criminals are not as likely to be deterred if the HO law is not strictly enforced or is enforced so infrequently that criminals do not become aware of or fear the greater sanctions. The HOA variable may also reflect some mild incapacitative effects, especially given that HOs are likely to be nearer their highest crime committing rates in the first year they are incarcerated (i.e. the year of admission).

The second variable is a HO incarceration rate variable (HOI). The HOI variable is similar to the incarceration measure typically used in prison population studies (e.g. Marvell and Moody, 1994; Levitt, 1996) except that in the present study only HOs are included. Once the HOA variable is controlled, which serves to control the contemporaneous impact of incarcerating HOs, the coefficient for

HOI should measure only the more long-term effects of keeping HOs off the streets. Simply put, while the HOA variable reflects only the number of HOs sent to prison in the current year, the HOI variable reflects instances of HOs sentenced to prison in past years as well. The HOI variable is defined as the total number of HOs in prison at the end of the fiscal year (ending June 30th) divided by county population. Again, it is important to note that any apparent reduction in crime associated with the HOI measure should not, by itself, be considered evidence of the effectiveness of the HO law because the majority of HOs were subject to lengthy prison terms before the passage of the HO law.

The final measure estimates the actual impact of HO sentencing on crime levels (i.e. extra prison time imposed on offenders due specifically to the HO law). It is worth stressing that it was only possible to measure the amount of time imposed on HOs and not the actual amount of time-served, since it hasn't, in many cases, been served yet. Thus, there is some imperfection in the measure of potential incapacitative effects to the extent that the percentage of time actually served by HOs can currently vary between 85 and 100 percent.

In order to estimate the average amount of extra prison time received by HO offenders for all crimes, it was necessary to conduct a HO sentencing disparity analysis. Data on the 271,015 prisoners sentenced to prison in Florida from 1989 to 1997 for all crimes were collected and coded for factors demonstrated to be important correlates of sentencing outcomes (see Crawford et al., 1998 and the research reviewed therein). These factors include: (1) habitual offender status; (2) type of primary offense; (3) age at admission; (4)

race; (5) sex; (6) marital status; (7) prior prison commitments; (8) felony class of primary offense; (9) total counts at time of sentencing; (10) guilty plea for primary sentencing offense; (11) a primary offense qualifier which denotes whether the offender committed, attempted to commit, or conspired to commit the primary offense; (12) sentencing circuit; (13) and a probation violator flag.[35] A complete description of the variables is reported in Appendix A. A HO sentencing disparity analysis was conducted for each year from 1989 to 1997 resulting in nine separate regressions. The primary variable of interest is the HO dummy variable which denotes whether the inmate was sentenced as a HO. The coefficient obtained for the HO dummy variable represents the average number of extra prison months imposed on offenders statewide in a given year for all offenses due specifically to the HO provision, controlling for all other relevant factors of sentencing.[36]

Table 8 shows only the OLS parameter estimates obtained for the HO law dummy variable from 1989 to 1997. The full set of results, including coefficient estimates for all control variables is presented in Appendix A. The results suggest that HOs are subject to much longer prison terms as a result of being sentenced under the HO provision. The HO dummy variable is one of the

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[35] Due to data limitations it was not possible to conduct the HO sentencing disparity analysis for the years 1981 to 1988. As a result, the mean number of extra prison months imposed on HOs for all crime types from 1989 to 1997 is used in the calculation of the HOPM measures. Given the infrequent use of the HO law before the 1988 amendment (see Table 2) this should produce very little measurement error in the HOPM variable.

[36] Since the present sample represents the entire population of offenders sentenced to Florida prisons from 1989 to 1997, and is subject to minimal, if any, sampling error, tests of statistical significance are rendered meaningless. That is, the focus of the sentencing disparity study is on

strongest predictors of sentence length across all years. Next, the average number of extra prison months received by HOs statewide (i.e. HO law dummy coefficient) was multiplied by the total number of HOs sentenced to prison from a given county in a given year. For example, Hillsborough county sentenced 222 offenders under the HO law in 1990. This number is then multiplied by 98.1 months, the average number of extra prison months imposed on HOs statewide in 1990. This yields a total of 21,776 extra prison months or 1,815 extra prison years. The extra prison months variable (HOPM) is then divided by 100,000 county population so that large counties do not dominate the results.

Data Limitations: Due to data limitations it was not possible to conduct the HO sentencing disparity analysis for each of Florida's 20 judicial circuits. To the extent that certain judicial circuits are more/less punitive than the statewide average, the HOPM variable could over/under estimate the number of extra prison months in counties which are in more/less punitive circuits. Similarly, it was not possible to conduct the HO sentencing disparity analysis for individual offense types. To the extent that judicial circuits target more/less serious offenders with the HO provision, the HOPM variable may also over/under estimate the amount of extra prison years in a given county. Finally, the actual impact of the law on crime rates may be less than the data would suggest if, for example, the law prompts criminals to move elsewhere, including out of the state of Florida and commit crimes against the residents of other states (i.e.

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the magnitude of the coefficient associated with being "habitualized", and is not concerned about drawing inferences to larger inmate populations.

displacement effects). Conversely, the impact may be greater if a high incarceration rate in one county prohibits those offenders from moving and committing crimes in other counties (i.e. free-riding effects). Nevertheless, the extra prison months variable should have its strongest impact, albeit not all of its effects, in the county where the offender was convicted.

It is also important to note that all three of the HO measures refer to persons sentenced as HO's in a court located in a given county, regardless of where the offender committed their conviction offense or where they ended up being incarcerated. That is, the county in which HOs were sentenced determines the county to which they were assigned. An official from the Florida Department of Corrections suggests, however, that the vast majority of offenders are sentenced in a court located in the county in which their conviction offense was committed with the only exceptions usually occurring for rare high-profile cases (Maria Toscano, 1998, personal communication). Thus, there is probably very little measurement error in the HO measures—that is, attributing the incarceration of a HO to the wrong county.

Specific Control Variables: Legislative changes in state sentencing policy can have a significant impact on both crime rates and prison systems (Bales, Amankawaa and Bryant, 1994; Bales and Dees, 1992:309). For example, in 1994 Florida implemented sentencing guidelines through the passage of the "Safe Streets Act". This eliminated determinate sentencing practices under the 1983 sentencing guidelines. Recent research by Bales, Vossberg, and Nimer (1997) suggests that changes in Florida's sentencing policies for felony offenders, inmate gain-time, and prison release practices over the past two decades (FY1979-80 to FY1996-97) have had a dramatic impact on the expected time served by criminals sentenced to prison. For example, the average time-served for all crime types reached a low of 1.4 years in FY1987-88 and FY1988-89 as compared to a high of 4.9 years in FY1996-97 (Bales et al., 1997:4). As a result of recent changes to sentencing policy (i.e. 1994 and 1995 sentencing guidelines) and gain-time mechanisms, prison populations in Florida have skyrocketed since the 1988 HO amendment from 33,681 in FY1987-88 to 64,713 in FY1996-97, a 92 percent increase.

Deterrence theory would suggest that such changes could effect a potential criminal's desire to commit a crime. Given the potential impact of these changes on crime levels, it is necessary to include a measure which adequately controls for these general sentencing reforms in the crime regressions. Failing to control for these recent changes in sentencing policies could result in spurious results. That is, crime levels may be affected by the overall increase in reliance of incapacitation as a crime prevention tool for all criminal offenders and not just repeat offenders.

In the present study, non-habitual offender incarceration rates (NHOI) are included in the crime regressions because recent research on the impact of prison populations on crime rates has shown it to be strongly related to crime reduction (see Marvell and Moody, 1994; Levitt, 1996). Also, the non-habitual incarceration rate should capture the long-term stacking effect of offenders in prison due to the dramatic increase in time-served. Further, unlike previous aggregate level studies of prison population growth and crime (e.g. Marvell and Moody, 1994; Levitt, 1996), which typically group offenders into heterogeneous groups (i.e. total prison population rates), this measure allows for the comparison of imprisoning high (i.e. HOs) and low-rate offenders.

Other Control Variables: In addition to the NHOI variable, four other control variables which prior research and theory suggest may impact the use of HO sentencing and crime levels were also included. The link between poor economic conditions, punishment, and crime is well documented in both the theoretical and empirical literature. Therefore two economic variables—unemployment and real per capita income (measured in 1992 dollars)—were included in the present analysis.[37] With respect to the economic distress-crime relationship, crime may be a way for those suffering from chronic unemployment or underemployment, to acquire the material goods they have been unable to obtain in more legitimate ways (Chiricos, 1987; Land et al., 1990). Previous research has also indicated a direct relationship between punishment levels and poor economic conditions (Chiricos and Delone, 1992). In the present context, this suggests that the legal system (i.e. prosecutors and judges) may attempt to alleviate some of the criminogenic effects of economic distress by increasing punishment levels (i.e. sentencing under HO provision) for defendants considered "surplus' or "marginalized labor". Since theory and prior research

[37] Annual county-level income data is not currently available for 1997. Personal income for 1997 was estimated by assuming that the percentage change in personal income from 1995 to 1996 (6.3 percent) was similar to the change in personal income from 1996 to 1997. Personal income data was converted from a current dollar estimate to a constant-dollar 1992 basis by dividing

suggests that poor economic conditions increases both punishment levels and crime rates, failing to control for these economic indicators would suppress the negative impact of HO sentencing on crime rates, if incarcerating offenders for extended periods of time reduces crime. That is, a negative relationship between the HO measures (i.e. HOA, HOI, and HOPM) and crime rates would be obscured without the economic controls. Data for unemployment was provided from the Florida Bureau of Economic and Business Research (BEBR) on computer disk. Personal income data was obtained from the Bureau of Economic Analysis (1999).

Previous aggregate level research has also indicated a relationship between age structure, punishment levels, and crime rates. Therefore, two age-structure variables are also included: percent of the male population that is male and age 15 to 24 years, and that is male and age 25 to 34 years (constructed from a Division of Economic and Demographic Research data disk). These age groups have been linked to higher crime rates as well as higher imprisonment rates and arrest rates in a given area (Sagi and Wellford, 1968; Cohen and Land, 1987; Marvell and Moody, 1995). In 1993, 68.7 percent of those arrested for index crimes were in the 15-34 age cohort (Federal Bureau of Investigation, 1993:227-228). The legal system may take trends in age groups with high crime rates into account when making sentencing decisions, and target those in the high crime age group with more punitive sentences. As discussed above, failing

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personal income by the consumer price index (CPI). Price deflators for 1992 were supplied by the Bureau of Economic Analysis.

to control for variables that have a direct positive effect on both HO sentencing and crime rates could suppress the negative impact of HO sentencing on crime rates.

Other control variables which are typically included in aggregate-level crime studies (e.g. percent black, poverty, population density) are not included in the present study because there is little variation within counties over time and thus, would be highly collinear with the county dummies.

Lagged Impact of Habitual Offender Law on Crime Rates

As discussed in Chapters 1 and 4, the impact of the HO law must be lagged. More specifically, the impact of the HOPM variable on crime levels is probably distributed unequally over time. Given the exploratory nature of this analysis one cannot tell *a priori* which lag is the most appropriate for each crime type. It is possible that the HOPM variable will take more time to show an effect for more serious crime types (e.g. robbery) due to the lengthy nature of prison terms already imposed for these crimes even without use of the HO provision (see Table 8). For example, the average time-served by non-HOs for violent crime ranges from 2.2 to 14 years. For property crimes, the average time-served for non-HOs varies from 1.2 to 1.8 years. Overall, the average time-served by non-HOs was 2.5 years (Florida Department of Corrections, 1998).

The present study concentrates on one to six year lags of the HOPM variable. The one to six-year lags should be long enough to estimate the crime-reduction impact of HO sentencing. If one to six-year lags of the HOPM variable fail to show any pattern of significant crime reductions then one can conclude that the HO law does not reduce crime, except perhaps for homicide and sex-related offenses, since even non-HO sentences for these offenses are so long, additional prison time for HOs might not show up for a decade or more. Also, It is worth noting that all of the crime regressions are estimated with an HOPM measure reflecting extra HO months imposed on all HOs in a given county, regardless of crime type committed. This was based on the fact that "habitual" or "career" criminals often commit varying types of offenses and offense-specific HOPM measures would underestimate the impact of the HO law on crime if for example, those "habitualized" for drug offenses were also committing burglaries, robberies, and other property offenses to support their habits. As Gottfredson (1999) comments:

> There is some evidence for consistency in offending, but the overwhelming weight of evidence is that offenders are quite versatile in their choices of crimes. Offenders tend to be opportunistic "generalists" rather than "specialists" in, say, burglary or robbery, and there is no evidence that such "patterning" increases as a "criminal career" progresses.

Thus, the HO law is only considered effective as a crime reduction tool if coefficients for the lagged HOPM variables (for all crime types) is consistently in the negative direction and at least a few of the HOPM coefficients are negative and significant. If, for example, only a few lags of the HOPM measure are in the negative direction and even fewer are significant and negative then this should not be considered supportive of the HO law-crime efficacy hypothesis. Given the large number of hypothesis tests performed on each crime type (6), one might expect at least a few of the lagged HOPM coefficients to be in the negative direction and significant as a matter of chance alone.

Simultaneity Problems

One of the biggest problems with prior aggregate level deterrence/incapacitation research is the inability of researchers to adequately address simultaneity problems (Marvell and Moody, 1996; Blumstein et al., 1978) With respect to the HOA and HOI-crime relationship, simultaneity is clearly possible because prosecutors may respond to crime problems by charging more offenders under the HO provision. Such a situation would bias estimates for the prison variables in a positive direction and counteract any negative effect of HO sentencing on crime.

Recent research by Marvell and Moody (1994) suggests, however, that crime rates have little or no impact on prison populations. Applying the Granger causality test to pooled state data for over nineteen years the authors found that crime rates did not affect short-term prison population growth. Also, it is unlikely that crime rates have a substantial impact on prison population given their vastly divergent trends. As Marvell and Moody (1996:626) note:

> prison population and crime rate trends are very different; the former are increasing at a tremendous rate, while most crime rates are steady or declining. This would be unlikely if crime rates affect prison populations.

In the present case, however, it is not possible to rule out the possibility that crime trends affect HOA and HOI levels. Thus, two separate Granger causality tests are conducted to determine whether causation exists between HO levels (i.e. HOA and HOI levels) and UCR crimes.

The Granger test is an econometric procedure which allows researchers to explore whether causation exists between two variables and to determine the causal direction, if any (Marvell and Moody, 1996). As Marvell and Moody (1994:122) note,

> "the fundamental notion underlying the test is that if X causes, Y, then lagged values of X will be significant in a regression of Y on its own lagged values and lagged values of X. If Y causes X, then lagged values of Y should be significant in a similar regression of X on its own history and the history of Y."

It is possible, however, that the Granger causality underestimates contemporaneous causation because the test only includes lagged effects of the principal independent variable of interest (Madalla, 1992; Marvell and Moody, 1994, 1996). This can be especially problematic if the only causation that exists between the IV and DV is instantaneous (same-year). If this were the case then the Granger test results would not be significant and, thus, would fail to indicate causation. However, the Granger tests are conducted in levels, and thus, would probably indicate causation because if there was a current-year impact of the IV on the DV then one would expect the one-year lag to also be significant due to the serial correlation between current-year and one-year lagged values of the independent variables. As Marvell and Moody (1994:123) note, "contemporaneous causation must imply lagged causation." For a more in-depth discussion of the Granger causality test see Granger (1969) and Marvell and Moody (1994, 1996).

Following Marvell and Moody (1994) the Granger test was conducted using two lags of IVs and DVs. Adding a third lag to the Granger tests did not alter the findings reported here. The initial Granger test was conducted to determine if any causation exists between HOA levels and crime rates. In the first regression, HOA levels are regressed on lags of itself and on lagged crime rates. If the lagged crime rates are jointly significant, as determined by an *F* test, crime Granger-causes HOA levels. In the second regression, crime rates are regressed on lags of itself and on lags of HOA levels. If the lagged HOA variables are jointly significant, HOA levels Granger-cause crime. A second test was conducted to determine if any causation exists between HOI levels and UCR crimes. The results of the Granger tests are presented in Chapter 6. The means and standard deviations for the variables used in the crime regressions are reported in Table 10.

CHAPTER 6

FINDINGS

Initial Results

Crime Rates and HO Admission Rates: The Granger causality test results for the impact of crime levels on HOA rates are presented in Table 11. The results give no evidence that crime levels increase HOA rates. The *F* value for the two lags of total crime is far from significant in the HOA regression. Similar results were obtained for the remaining UCR crime types. All of the *F* values are non-significant, including individual property crime types which account for a large percentage of HO admissions. Robbery does have an impact at the first lag, perhaps because robbery is considered to be an offense which is often committed by "career" or "habitual" criminals, and prosecutors may take robbery trends into consideration when making HO charging decisions.

Crime Levels and HO Incarceration Rates: The results of the Granger test for the impact of crime levels on HOI rates indicate that only property crimes have an impact on HOI rates. This is somewhat inconsistent with the earlier findings concerning the impact of crime levels on HOA rates. The lack of significant impacts of property crime types on HO admission rates (Table 11) suggests that property crime trends probably have a modest impact (as seen below) on HO charging decisions, and it may take several years for property crime trends to result in the accumulation of HOs in prison. The impact does not occur until the second lag for burglary, and larceny, probably due to the time required to gather crime data, disseminate it among criminal justice officials, appropriate funds, and build more prison beds. Motor vehicle theft has an impact at the first lag, however, there is no theoretical reason why motor vehicle theft would have a faster impact on HRI levels than burglary and larceny.

As discussed above, the coefficients may be interpreted as elasticities due to the natural log transformation. However, as Marvell and Moody (1996:629) note, "the coefficients on the lagged IVs understate the full impact because much of it eventually comes through the lagged dependent variable." Thus, to estimate the impact of the property crime variables on HOI rates, it is necessary to add the coefficients for the property crime variables and then divide by the reciprocal of one minus the coefficient on the lagged dependent variables (Marvell and Moody, 1996). For example, the elasticity for burglary is .28 [(.055+.095)/(1-.558+.088)]. The elasticities for larceny, and motor-vehicle theft are .54, and .18, respectively. That is, each 10 percent increase in burglary, larceny, and motor-vehicle theft rates, leads to a 2.8, 5.4, and 1.8 percent increase in HOI rates, respectively.

With respect to the violent crime types, the *F* values are all non-significant. In fact, none of the coefficients for the lagged violent crime types are significant at the .05 level. Taken as a whole, the major findings of Tables 11 and 12 is that prosecutors respond to property crime trends more than violent crime trends when deciding which types of offenders to target with the HO statute, probably because "career" or "habitual" criminals commit more property offenses than violent offenses. These results are consistent with those reported in Table 5 which revealed that over one third (38.9 percent) of HOs are in prison for property related crimes.

HO Admission Rates and Crime Levels: Table 13 presents the Granger results for the impact of lagged HOA rates on all eight components of the crime index. This allows one to estimate the short-term impact of locking-up HOs on crime levels. As noted above, this is important because if locking up HOs not reduce crime over the short-term then a long-term impact is probably unlikely. The results in Table 13 give no indication that HOA rates reduce crime levels over the short-term. The HOA lags are all insignificant, with the exception of the two-year lagged HOA variable for robbery. In fact, the lagged HOA rate variables actually produce more positive coefficients (10) than negative coefficients (6). Any consistent impact of locking up HOs on crime should produce a significant negative coefficient, especially given the large number of degrees of freedom (848).

HO Incarceration Rates and Crime Levels: The results presented in Table 14 provide more evidence that sentencing offenders under the HO statute does little to reduce crime over the short-term. The *F* values are non-significant for all crime types, including property crimes which account for the majority of HOs in prison. The coefficients for the lagged HOI variables are far from significant, and they do not always have the expected negative sign. Of the 16 lagged HOI variables entered into the crime regressions, 11 are positive and 5 negative. A possible exception is found in the regression for rape, where the first-year HOI lag is significant and negative. This lag means that the accumulation of HOs over the short-term reduces rape, but does not imply that the HO law is responsible for this reduction because the vast majority of these offenders would have sentenced to prison even if the HO law did not exist. This is especially true for sex-related offenses, such as rape, where non-HOs served or are expected to serve an average of 6.7 years for sex-related crimes (Table 9). Likewise, the two-year lagged HOA robbery variable in Table 13 should not be considered evidence of the HO laws impact given that robbery offenders sentenced under determinate sentencing policies (1983-1993) or the sentencing guidelines (1994 to present) served or can expect to serve an average of 3.3 years for robbery. Thus, except for possibly rape and robbery, one can rule out a short-term impact of HO rates on crime levels. Likely explanations for these findings were discussed in Chapter 3. Finally, the results of the estimations presented in Tables 13 and 14 should not be considered evidence of the HO laws effectiveness or ineffectiveness, as one would have expected most offenders with extensive criminal histories (i.e. HOs) to serve at least a few years in prison. Accordingly, the analysis now focuses on the extra amount of time that individuals receive as a result of the HO law (HOPM variables) and how that extra prison time affects crime levels.

Florida's HO Law and Crime Rates: The analysis now turns to the central question of this study. Does incarcerating offenders for extended periods of time reduce crime? If so, does the impact vary across crime types, sample sizes, or variable configurations? If the answer to all of these questions is consistently no, then it is safe to conclude that Florida's HO law does not reduce crime.

The first group of regressions presented in Table 15 estimate the average impact of Florida's HO law on eight different categories of crime. The results of separate crime regressions reported in Table 15 includes lagged HOPM variables, with only one HOPM lag included at a time, and all of the control variables discussed above (HOA rates, HOI rates, specific control variables, and proxy variables), but no lagged dependent variables (see Appendix B for the full set of crime regressions with the one-year lag HOPM variable).

MTS regressions with the first six lags of the HOPM variable give no consistent indication that Florida's HO law reduces crime. The coefficients for the HOPM lags are far from significant, and there is no consistent pattern among the algebraic signs of the lagged HOPM coefficients for any crime type. Of the 48 lags entered into the crime regressions, 18 are in the negative direction, and only 2 are significant and negative at the .05 level (1-tailed). Although the two-year HOPM lag is significant and negative for robbery and the six-year HOPM lag is significant and negative for homicide, there is no sound logical or theoretical reasons to attribute associations to a crime-reducing effect of the HO law. First, non-habitual offenders were already serving an average of 3.3 and 14 years for robbery and homicide, respectively even though they did not receive HO sentences (see Table 9). Second, as noted above in Tables 13 and 14, both HOA and HOI rates are non-significant in regressions for homicide, and robbery while excluding the HOPM variables, and it is reasonable to assume that if there is no short-term impact, there is probably no long-term impact because HOs are likely to be nearer their highest crime committing rates in the first couple of years they are incarcerated. Finally, one would expect a few of the 48 HOPM coefficients to be significant and in the negative direction as a result of chance alone due to the large number of hypothesis tests performed. Re-running the specifications shown in Table 15 with lagged dependent variables in order to correct for possible autocorrelation produced similar coefficients for the lagged HOPM variables, though the *t*-ratios were usually much smaller (Table 16).

Some might argue that the simultaneous inclusion of HOI and HOPM rates in the crime models is problematic because the HOI and HOPM variables overlap somewhat. That is, a certain number of HOs included in the HOI measure are serving the extended portion of their sentences due to HO sentencing, and thus, in many instances are also accounting for any deterrent/selective incapacitative effects of the HO law on crime rates. For example, a HO sentenced to prison for robbery would be included in the HOI measure for approximately 15 years, and would begin to account for any deterrent/selective incapacitative effects of the HO law after 3 years (see Table 9). Consequently, the simultaneous inclusion of the HOPM and HOI variables could lead to collinearity if the HOI measure is partially accounting for some of the crime-reduction effects of the HO law. This could result in inflated standard errors for the lagged HOPM variables, and bias hypothesis tests in favor of the null hypothesis. To address this problem the crime rate models were re-estimated without the HOI measure (Table 17). The HOPM measure still showed no positive effect on any of the crime rates.

Robustness Checks

Florida's HO Law and Crime Rates Using Different Variable Configurations: The first set of robustness checks compare the results in Table 15 with the results when using different variable structures. First, the analyses in Table 15 were rerun when the variables are not logged (Table 18). While the number of significant coefficients increased dramatically (2 to 20), there is little evidence that increases in HOPM levels leads to reduced crime levels because nearly half of these coefficients are significant positive (9). The reason for so many significant coefficients is probably the excessive impact of several HOPM outliers.

Second, as discussed above, crime regressions are conducted in levels because the ADF test revealed that the HO and crime rates are stationary in levels over the past seventeen years and, thus, first-differencing variables is not necessary. In any event, crime regressions are re-estimated with first-differenced variables. First-differencing has little impact on the crime regressions (Table 19), and the results are virtually identical to the regressions in levels (Table 15).

Third, Table 20 uses non-weighted regressions. Using non-weighted regressions actually produces more statistically significant positive (5) than negative (3) HOPM coefficients. The reason is probably due to the influence of small counties. Fourth, the regressions in Table 15 are re-estimated with a quadratic trend (linear trend squared) while controlling for the effects of the linear trend. The quadratic term is a proxy control for factors that are changing at a rate even faster than those picked up the linear trend (Marvell and Moody, 1996). Adding the quadratic trend does little to the crime regressions, except that the six-year HOPM variable is significant and positive for rape and the five-year lag is significant and negative for assault (Table 21).

Finally, the regressions in Table 15 are conducted with data starting in 1988 because this is the year the HO law amendment became effective. While this shortens the length of the time-series considerably, it is possible that the crime-reduction effects of the HO law are masked due to the inclusion of years when the HO law was used rather infrequently. Deleting years 1981 to 1987 produces results similar to those obtained with the entire time-series (Table 22). This suggests that the results reported in Table 15 are rather consistent over time.

Florida's HO Law and Crime Rates Dropping Groups of Control Variables: As discussed above, one of the main advantages of the MTS design is the ability to enter proxy variables to mitigate omitted-variable bias. Thus, crime regressions are compared to the results reported in Table 15 while dropping groups of control variables to determine what impact, if any, this has on the main results. Specifically, crime regressions are re-run while dropping the specific control variables (Table 23), year dummies (Table 24), county trends (Table 25), and year dummies and county trends (Table 26). The results changed little for most crime regressions, except that when the year and county trend dummies are dropped, there are 7 negative and significant HOPM coefficients and none significant positive. It should be stressed, however, that these coefficients are probably biased because of the exclusion of the year and county trend variables.

<u>Florida's HO Law in High, Medium, and Low Populated Counties:</u> As discussed earlier, the estimates presented above are <u>averages</u> across the counties, and the impact of Florida's HO law may vary across counties due to, for example, differences in criminal opportunities (Zimring and Hawkins, 1995). For example, while low population counties tend to have lower HOA, HOI, and HOPM rates than large urban counties, the removal of HOs from these counties may produce a greater crime-reduction impact because there is less chance that incapacitation will produce substitution. That is, lower populated counties will probably have a smaller pool of potential offenders to replace those already imprisoned because HOs are more likely to reside in large urban counties where criminal opportunities are greater and the risk of apprehension and punishment is lowered. If so, increases in the application of the HO law should be more efficacious in low populated counties, as opposed to medium and large urban counties. In regressions similar to those reported in Table 15, each of the crime regressions is re-estimated with counties divided into the following three population groups (population based on 1989—midpoint in time-series): counties with a population greater than 100,000 (n=27), counties with a population between 25,000 to 100,000 (n=20), and counties with populations less than

25,000 (n=20). A listing of the counties included in each population group is provided in Appendix C. The results of the regressions for the three groups of counties are presented in Tables 27 to 29.

The results in Table 27 give no indication that Florida's HO law has a crime-reduction impact in medium populated counties. In fact, only one of the 48 HOPM lags are significant and negative in the crime regressions (1 year lag-robbery), and one is actually significant in the positive direction (2 year lag-homicide). Likewise, the results for low population counties (Table 28) do not support the hypothesis that imprisoning HOs for prolonged periods of time reduces crime. While the majority of HOPM lags are in the negative direction (30 out of 48) only two are significant and negative, balanced by two others that are positive and significant.

Contrary to theoretical expectations, the only support for the HO law-crime hypothesis was found in the most populous counties (Table 29). Thirty-three of the 48 HOPM lags entered into the crime regressions have negative coefficients, 6 are significant negative and 3 positive to the .05 level. It is difficult to claim, however, that the HO law has a significant impact on crime rates in the most populous counties because there is no clear theoretical reason why the HO law would only impact crime levels in high population counties, and not others. Also, some of the findings that the HO law appears to increase or reduce crime has nothing to do with the HO law, because one would expect that about two significant results are due to chance alone, and one cannot tell which these are. Nevertheless, there is some <u>weak</u> empirical support for the HO law effectiveness hypothesis in high populated counties, but the reasons for the significant results remain ambiguous.

Overall, the results presented in Tables 15-29 for the lagged HOPM variables suggests that (1) on balance incarcerating offenders for extended periods of time does little statewide to reduce violent or property crime levels and (2) no lag shows consistent effects across crime types or different model specifications or samples (with the possible exception of high populated counties), and the few lags that are significant are not consistent with any theory of deterrent or incapacitative effects.

Florida's HO Law and Crime Using an HO Law Dummy Variable:

Finally, crime regressions were re-estimated with a HO law dummy variable to further assess the robustness of the results presented above. The HO law dummy variable is scored as 1 after the effective date of the 1988 HO law amendment. Since the law went into effect on October 1, 1988, the value of this variable for 1988 is the portion of the year remaining, 0.25. Regressions were also re-estimated with the HO law dummy variable lagged one, two, and three years to determine if the HO law had any delayed deterrent/incapacitative impact on crime levels.

With the possible exception of burglary, the coefficients for the current-year, one-year lag, two-year lag, and three-year lag HO law dummy variables do not provide consistent support for the hypothesis that Florida's HO law reduces crime (Table 30). While the majority of coefficients are in the negative direction, only 6 are significant and negative at the .05 level, and 1 is significant in the positive direction. The significant and negative coefficients for burglary is probably due to the deletion of year dummy variables, a problem mitigated with the alternative HO measures used above.[38] Crime regressions were also estimated with three short-term variables in order to estimate any initial deterrent effects of the HO law on crime levels. If offenders come to the conclusion that the HO law is not being enforced or does not result in longer prison terms (i.e. judges circumvent the laws) then any initial deterrent effects of the HO law may have only been short-term (Marvell and Moody, 1995). The three short-term variables are scored zero except for the first two, three, and fours year of the HO law, respectively. The results revealed no evidence of short-term impacts on the crime variables (Table 31). In fact, only 5 out of the 24 short-term variables entered into the crime regressions have negative coefficients, none are significant negative and 4 are positive. Finally, a distributed lag variable was created in order to capture any strong immediate effects which eventually tapered off. Following Marvell and Moody (1995), the variable is constructed by taking the logged sum of the basic HO law dummy variable, plus the three short-term variables, plus one. The distributed lag variable is then lagged one year. The coefficients for the distributed lag variables are from significant (2 are actually positive) and give no indication of an initial deterrent effect which eventually tapered off over time (Table 31). In all, the analyses with dummy

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[38] Since Florida's HO law effected all counties at the same point in time (October 1, 1988), the inclusion of a habitual dummy variable leads to perfect collinearity with the year unit effects, which are a necessary feature of the "fixed-effect" model. Deleting the year dummies (unless significant as a group) causes the estimates of the remaining variables to be biased (Pindyck and Rubinfeld, 1991; Marvell and Moody, 1995).

variables (Tables 30 and 31) are generally consistent with the earlier findings concerning the immediate and delayed impact of Florida's HO law on crime rates.

CHAPTER 7

SUMMARY, DISCUSSION, AND CONCLUSION

Summary

These results generally do not support the hypothesis that selectively incarcerating offenders designated as "habitual" for extended periods of time reduces county level crime rates. Of the 720 crime regressions estimated, no HOPM lags show consistent effects across crime types, varying model specifications, or samples. The few lags that are significant and negative (48) are not consistent with any theory of deterrent or incapacitative effects and are usually balanced by positive, significant associations (24). Note that one would expect 35 significant coefficients just on the basis of chance alone, and it is impossible to determine which ones these are. Likewise, the results presented above also suggest that locking up HOs over the short-term does not appear to lower crime levels. None of the *F* values for the two-lagged HOA or HOI variables were significant and negative for any crime type at the .05 level. Rather, it appears that judges and prosecutors take into account property crime trends when making HO sentencing decisions. Finally, despite the problems associated with the use of dummy variables, the present study tested the robustness of the results obtained with the HOPM variables by also including various configurations of a HO law dummy variable (Tables 30 and 31). The resulting estimates confirmed the results obtained with the lagged HOPM variables. The HO law dummy variables were almost never negative and significant (with the possible exception of burglary), indicating that the short and long-term effects of Florida's HO law on crime rates apparently did not depend on the HO measured used, at least not based on the HO measures used here (i.e. HOA, HOI, and HOPM rates). Overall, the present findings are <u>completely consistent</u> with the findings of previous MDSE studies which generally find little or no support for the MDSE law-crime efficacy hypothesis.

Unlike previous research on MDSE laws which have relied solely on the immediate or short-term effects of MDSE sentencing on crime, the present study avoided the problems associated with the use of dummy variables by including more nuanced measures to assess the impact of HO sentencing on crime levels. Specifically, this study attempted to measure the extra amount of prison time imposed on offenders specifically attributable to HO designation as opposed to the immediate and short-term deterrent/incapacitative effects that occur even in the absence of such provisions. This strategy should prove useful for future evaluations of MDSE laws on crime. Unlike previous research on MDSE laws, the present study also emphasized the importance of robustness checks: Crime models were estimated with different samples, different variations of the regressions, and various configurations of a HO law dummy variable. Finally, the present study limited omitted-variable bias by adding proxies that control for differences between counties, yearly statewide trends, and trends in counties that differ from statewide trends.

Discussion

Why does Florida's HO law appear, with few possible exceptions to have no impact on any of the serious crime types that frequently involve "habitual offenders", or "career criminals"? Theories that might explain the lack of a deterrent or selective incapacitative impact of HO sentencing on crime were discussed extensively in Chapter 3. Perhaps the most likely reason for the lack of an impact of Florida's HO law on crime has to deal with the age of HOs at the time of their admission. As noted in Chapter 3, the bulk of "criminal career" research suggests that offending rates begin to decline substantially as offenders enter their 30s. Considering that the average age of a HO at the time of admission was 30, it should not be all that surprising that incarcerating offenders past the age they are "at risk" to commit crimes does <u>not</u> lead to substantial reductions in crime rates. It may be that HOs were once high frequency offenders, but the CJ system identifies them as such only after they are well past their high-rate years, too late to get much crime-control impact. In fact, if older offenders are taking up space that could have been used for more active younger offenders, there could be a net loss of incapacitative effect as a result of the HO law. That is, retaining prisoners into their older ages (30s and 40s), robs the prison system of spaces that could otherwise be devoted to offenders in their 20s, who are likelier to be closer to the peaks of their criminal careers.

Another possible explanation for the lack of an impact of Florida's HO law on crime rates deals with prediction. Unfortunately, the CJ system is rather poor at predicting future criminal behavior. This is especially problematic for HO laws because they only target offenders with extended criminal histories. As just noted, however, most offenders do not accumulate extensive criminal histories until they are in their late 20s or early 30s, a time when it is too late to get much crime reduction impact. This means that the CJ system should probably place less emphasis on an offenders criminal history and place more emphasis on the age of the offender when making HO charging decisions. For example, a 20-year old robbery offender with 2 priors is probably at greater risk of committing future criminal acts than a 30 year-old robbery offender with 4 or 5 priors, all other things being equal. Unfortunately, the 30-year old is more likely than the 20-year old to be sentenced under Florida's HO law given his/her extended criminal history. As a result, the 30-year old will be incarcerated until he/she is 45, while the younger more criminally active 20-year old is released from prison at age 23. Thus, while it is probably true that a small group of high-rate offenders account for a disproportionate number of crimes, it will be difficult for selective incapacitative strategies, such as HO laws, to reduce crime rates if prior record continues to be the main prediction tool of future criminal behavior.

Another factor possibly affecting the impact of Florida's HO law on crime rates was the implementation of numerous early-release programs in the late 80s and early 90s to alleviate prison overcrowding in Florida's prison system. These early-release programs resulted in substantial decreases in the percentage of sentences served for many offenders, and may have compromised the impact of HO sentencing on crime rates. Finally, it is possible that replacement and co-offending effects cancelled out any selective incapacitative effects of HO sentencing on crime rates. This is important because it suggests that MDSE laws are inherently limited as crime-reduction tools if those targeted by "sentencing enhancement" laws are simply replaced by new young recruits. The implication for the crime rate is that the beneficial effects of incapacitation could be canceled out or, worse, the replacement effect could outweigh the beneficial effects of incapacitation and produce a net increase in crime--if, for example, older HOs are replaced by younger offender(s).

Some might argue that the time period covered is not long enough to adequately estimate the impact of HO sentencing on the more serious crime types and it is possible that the added incapacitative effects of the HO law may still show up, once additional years of data can be analyzed. Thus, it is possible that even longer lags are needed to discover any HO law effects, in light of how long even non-HO sentences are, especially for murder and rape. Offenders sentenced as HOs even back in late 1988, may still not be serving the extra prison years that are attributable to HO designation for the more serious offenses. However, there are two reasons for believing there is no delayed impact of HO sentencing on the more serious crime types. First, the results presented in Table 13 and 14 revealed no short-term deterrent/incapacitative effects in the homicide and rape regressions, and HOs are most likely to be nearer their highest crime committing rates in the first couple of years they are incarcerated. Coupled with the fact that the average age of a HO at the time of admission is 30, it is unlikely that HOs would be more criminally active in their late 30s or early to mid 40s compared to their level of criminal activity at the time of admission to prison.

Conclusions

In sum, this research found little evidence that Florida's HO laws had made any serious impact on the reduction of crime in the state of Florida. Additionally, previous research has suggested that the increased application of Florida's HO law over the past decade may have had some adverse consequences, such as a huge financial cost (see Bales and Dees, 1992), and some indication of racial disparity in the application of the HO law to the disadvantage of African-Americans (EDR, 1992; Crawford et al., 1997). In light of these facts, it might be appropriate to re-evaluate the use of the current HO statute. This is particularly true because, given the extensive prior records of most habitual offenders, almost all of these offenders would receive lengthy prison terms under the sentencing guidelines, and current statutes require offenders to serve at least 85 percent of their imposed sentences.

TABLE 1

Summary of Florida's Habitual Offender Law

Effective Date	Triggering Event	Gain-time Provisions[a]				Sentencing Provisions
		BGT[b]	IGT[c]	PC[d]	MGT[e]	
775.084 Subsequent Felony Offenders January 1, 1972	Felony conviction and prior felony conviction, and the present crime occurring less than 5 years from the defendant's release on parole or otherwise from a sentence imposed as a result of such prior conviction.	YES	YES	NO	NO	1st degree felony conviction, life. 2nd degree felony conviction, a term of years not to exceed 30. 3rd felony conviction, a term of years not to exceed 10 (see note f below).
775.084 Habitual Felony Offenders October 1, 1988	Felony conviction and two or more prior felony convictions, and the present crime occurring less than 5 years from the defendant's release on parole or otherwise from a sentence imposed as a result of such prior conviction.	NO	YES	NO	NO	Same as 1972 statute
775.084 Habitual Violent Felony Offenders October 1, 1988	One or more felony convictions and a prior conviction for a specified violent felony within the time frame specified in the habitual felony offender statute. The specified nature of the current offense is irrelevant. The specified prior offenses include: arson, sexual battery, robbery, kidnapping, aggravated child abuse, aggravated assault, murder, manslaughter, unlawful throwing, placing, or discharging of a destructive device or bomb, or armed burglary.	NO	YES	NO	NO	1st degree felony conviction, life and not eligible for release for 15 years. 2nd degree felony conviction, a term of years not to exceed 30, and not eligible for release for 10 years. 3rd degree felony conviction, a term of years not to exceed 10 and not eligible for release for 5 years.

Notes to Table 1:
(Source: Bureau of Sentence Structure, Florida Department of Corrections, July 1997)

a. BGT=Basic gain-time; IGT=Incentive gain-time; PCs=Provisional credits; MGT=Meritorious gain-time. For offenses committed on or after October 1, 1995, the gain-time statute was amended to prohibit the awarding of any gain-time that would result in an offender being released prior to serving 85 percent of the sentence imposed.

b. Basic gain-time was awarded at a fixed rate based on the term of the sentence and the date of offense. Basic gain-time typically reduced the inmates sentence by one-third upon entering prison. Basic gain-time was eliminated for all inmates with offenses committed on or after January 1, 1994.

c. Incentive gain-time is awarded to inmates based upon work evaluations, program participation, and inmate behavioral adjustment. Awards are made on a monthly basis as earned, and is dependent upon the date the crime was committed. Habitual violent felony offenders can earn incentive gain-time during the mandatory portion of the sentence, however, the inmate must serve the time specified by the mandatory term before release from prison. After October 1, 1995 the awarding of incentive gain-time for all inmates was limited up to 10 days per month until such time as the tentative release date is the same as that date which is equal to 85 percent of the sentence imposed and thereafter, no further incentive gain-time can be earned.

d. PC programs statutorily defined inmates as eligible or ineligible to receive early-release credits. PC programs were implemented to maintain the prison population within legal capacity limits under Costello v. Singletary. Early-release credits for all inmates was discontinued in December, 1994.

e. Meritorious Gain-time may be awarded to inmate for an outstanding deed performed law. The law currently allows for a maximum award of 60 days.

f. Prior to 1988 the law required a separate judicial hearing to determine if sentencing an offender under the Habitual offender law was necessary for the protection of the public. A preponderance of the evidence was required to habitualize the offender (this requirement was eliminated in the 1988 amendment to the Habitual Offender statute).

TABLE 2

Annual Habitual Offender Admissions to the Florida Department of Corrections: 1980-1997

Year	Annual Admissions	Total Admissions	Percent of All Admissions
1980	58	9,219	0.63
1981	55	11,392	0.48
1982	86	14,360	0.60
1983	86	13,526	0.64
1984	83	12,552	0.66
1985	102	15,838	0.64
1986	102	19,932	0.51
1987	50	26,733	0.19
1988	115	35,449	0.32
1989	1,097	44,119	2.49
1990	2,635	41,531	6.34
1991	2,902	34,479	8.42
1992	3,033	33,016	9.19
1993	2,341	28,546	8.20
1994	2,108	24,348	8.66
1995	2,359	21,033	11.22
1996	2,697	21,516	12.53
1997	3,031	22,427	13.51
Total	22,960	430,016	5.34

Source: Florida Department of Corrections, Bureau of Research and Data Analysis

Table 3

Comparison of Length of Prison Sentences and Time Served for Habitual Offenders and Non-Habitual Offenders: Admissions from October, 1988 to June, 1998

	Habituals	Non-Habituals
Sentence Length		
2 years or less	6.0%	23.7%
2 to LT 5 years	21.9%	49.0%
5 years to LT 10 years	28.2%	17.4%
10 years to LT 30 years	30.8%	7.9%
30 or more years	13.2%	2.0%
Average	12.5 years	4.9 years
Time Served in Prison		
1 year or less	5.1%	63.4%
+1 to 2 years	13.9%	16.1%
+2 to 5 years	34.5%	12.7%
+5 to 10 years	21.4%	4.4%
+10 o 20 years	13.8%	1.4%
20 years	11.2%	1.7%
Average	9.6 years	2.5 years

Source: Florida Department of Corrections, Bureau of Research and Data Analysis

TABLE 4

Inmates Serving Sentences Imposed Under the Habitual Offender Law: FY1988-89 to FY 1996-97

End of Fiscal Year June 30th	Number of Inmates	Annual Increase	Annual Percentage Increase	Total Prison Population	Percent of All Inmates
1989	585	-	-	38,059	1.5
1990	2,343	+1,765	+305.4	42,733	5.5
1991	4,818	+2,475	+105.6	46,233	10.4
1992	6,832	+2,014	+41.8	47,012	14.5
1993	8,202	+1,370	+20.1	50,603	16.2
1994	7,883	-1,122	-12.5	56,052	14.1
1995	8,426	+543	+6.9	61,992	13.6
1996	9,511	+1,085	+12.9	64,333	14.8
1997	10,381	+870	+9.1	64,713	16.0

Source: Florida Department of Corrections, Bureau of Research and Data Analysis

TABLE 5

Profile of Inmates Sentenced Under Florida's Habitual Offender Law, 1989-1997

	N	%
Total Intake	22,203	100.0
Sex		
Male	21,504	96.9
Female	699	3.1
Age		
Under 20	373	1.7
20-29	9,573	43.1
30-39	9,232	41.6
40-49	2,546	11.5
50+	477	2.1
Race/Ethnicity		
Black	15,713	70.8
White	6,232	28.1
Latin	213	1.0
Other	45	0.2
Primary Offense Type		
Person	6,836	30.8
Property	8,630	38.9
Drugs	5,042	22.7
Other	1,693	7.6
Prior Prison Commitments		
None	2,240	10.1
1	5,304	23.9
2	5,900	26.6
3	4,350	19.6
4	2,403	10.8

5+	2,006	9.0
Sentence Length		
5 years or less	8,591	38.7
5-10 years	6,280	28.2
10-15 years	2,301	10.4
15-30 years	3,062	13.8
30-50 years	565	2.2
50+	283	1.3
Life	1,087	4.9
Death	34	0.2
Average Time-Served by Crime Type (in years)		
Murder/Manslaughter	28.2	
Sexual/Lewd Behavior	19.7	
Robbery	15.1	
Violent, Other	10.6	
Burglary	8.5	
Property Theft/Fraud	5.0	
Drugs	6.0	
Weapons	6.5	
Other	5.9	

Source: Florida Department of Corrections, Bureau of Research and Data Analysis

TABLE 6

Profile of Sentences for Florida's Habitual Offenders, 1989-1997

Offense	Number of Offenses	%	Cum %
Murder 1st	357	1.6	1.6
Murder 2nd	287	1.3	2.9
Murder 3rd	17	0.1	3.0
Homicide, Other	14	0.1	3.1
Manslaughter	58	0.3	3.4
Violent, Other	44	0.2	3.6
Robbery without Weapon	1,548	7.0	10.6
Robbery with Weapon	1,946	8.8	19.4
Home Invasion, Robbery	15	0.1	19.5
Aggravated Assault	201	0.9	20.4
Assault/Battery, Other	17	0.1	20.5
Aggravated Battery	657	3.0	23.5
Assault/Battery on L.E.O.	417	1.9	25.4
Aggravated Stalking	13	0.1	25.5
Capital Sexual Battery	59	0.3	25.8
Life Sexual Battery	167	0.8	26.6
1st Degree Sexual Battery	75	0.3	26.9
2nd Degree Sexual Battery	90	0.4	27.3
Abuse of Children	14	0.1	27.4
Car Jacking	80	0.4	27.8
Arson	98	0.4	28.2
Kidnapping	316	1.4	29.6
Burglary, Structure	2,169	9.8	39.4
Burglary/Trespass, Other	44	0.2	39.6
Burglary with Assault	491	2.2	41.8
Burglary, Armed	612	2.8	44.6
Burglary, Dwelling	2,834	12.8	57.4
Grand Theft	603	2.7	60.1
Auto-Theft	549	2.5	62.6
Stolen Property	742	3.3	65.9
Fraudulent Practices	191	0.9	66.8
Forgery/Counterfeiting	204	0.9	67.7

Other Theft/Property Damage	167	0.8	68.5
Worthless Checks	24	0.1	68.6
Drug Possession	1,190	5.4	74.0
Drug Sale/Manufacture	3,614	16.3	90.3
Drug Trafficking	238	1.1	91.4
Racketeering	10	.05	91.5
Escape	449	2.0	93.5
Weapons Offense	1,026	4.6	98.1
Lewd/Lascivious Behavior	158	0.7	98.8
Other Offense	394	1.8	100.6

Source: Florida Department of Corrections, Bureau of Research and Data Analysis

TABLE 7

Summary of Studies on the Effect of Mandatory Sentencing Laws on Crime Rates

Study	Research Design[a]	Time Period	Sample	Problems Associated with Study[b] 1	2	3	4	Mandatory Sentencing Law Effective?[c]
Habitual Offender Laws								
Stolzenberg and D'Alessio (1997)	ITS	85-95	132 mo., 10 largest cities in California	X		X		NO
Males, MacAllair, and Taqi-Eddin (1999)	B-A	91-97	7 yrs., 12 largest counties in California	X		X		NO
Firearm Sentence Enhancement Laws								
Beha (1977)	ITS	72-77	63 mo., Boston	X	-	X		MIXED
Deutsch and Alt (1977)	ITS	66-75	142 mo., Boston	X	-	X	X	MIXED
Hay and McCleary (1979)	ITS	66-76	156 mo., Boston	X	-	X	X	NO
Deutsch (1981)	ITS			X	-	X		YES
Pierce and Bowers (1981)	B-A & ITS	74-76	3 yrs., 36 mo., Boston	X	-	X	X	MIXED
Loftin and McDowall (1981, 1983)	ITS	67-79[e]	156 mo., Detroit	X	-	X		NO
Loftin and McDowall (1984)	ITS	67-80	168 mo., Tampa, Miami, Jacksonville	X	-	X	X	NO
McPheters et al. (1984)	ITS			X	-	X		YES
McDowall et al. (1992)	ITS[d]	see[f]	Pooled results for 6 city-specific case studies	X	-	X		MIXED
Kleck and Patterson (1993)	CX	80	170 cities					MIXED
Marvell and Moody (1995)	MTS	71-93	1,150 yr./states		X			NO

Notes to Table 7

a. B-A= Before-After percentage data analysis. CX= cross-section. ITS= interrupted time-series. MTS= multiple time-series.

b. Key to Table 1 (Problem codes for mandatory/discretionary sentencing enhancement law studies were adopted from Kleck (1991:Ch.10;1995: 36 *Table 4*). Problem codes are as follows: X indicates problem existed, blank indicates no problem, (-) indicates problem is an inherent property of time series studies, and (Z) indicates partial presence of problem, or problem inadequately addressed.

c. "Mandatory Law Effective?" means "Did mandatory sentencing law appear to significantly reduce total rates of violence or crime?"

d. Time period covered for McDowall et al. (1992) study of various FSE laws included the following: Florida (robbery 1968-80; homicide 1968- 1978) Detroit (robbery and assault (1967-79; homicides 1969-78) Pittsburgh and Philadelphia (robbery and assault 1978-84; homicide 1970-1984).

e. Time period covered for Loftin et al.'s (1981,1983) studies on Michigan's FSE law included the following: gun and non-gun homicides (1969-1978); Gun and other weapon robberies (1967-1979 and 1975-1979); gun and non-gun assaults (1967-1979).

f. McDowall et al. (1992) pooled together results for six interrupted time-series analyses in order to estimate the combined effect of these laws on homicides, robberies, and assaults.

KEY TO TABLE 7

Problem Codes:

1. The number of control variables was equal to or less than two.
2. State level of analysis used, rather than city or county.
3. Studied just one specific law; little generalizability.
4. Included no control jurisdictions, or inappropriate control jurisdictions; inappropriate crime control series.

TABLE 8

Extra Number of Prison Months Imposed on Habitual Offenders in Florida for all Crime Types: 1989-1997

Year	All Crime Types
1989 (n=44,053)	102.79
1990 (n=41,488)	98.09
1991 (n=23,023)	89.26
1992 (n=33,012)	79.88
1993 (n=28,537)	75.31
1994 (n=24,344)	81.08
1995 (n=21,029)	75.46
1996 (n=21,512)	68.83
1997 (n=22,421)	67.08
Average (n=271,015)	81.98

Notes—The OLS parameter estimates for the HO law dummy variable are listed in the second column. The coefficients estimate the average number of extra prison months imposed on offenders due to HO sentencing.

Table 9
Comparison of Length of Time Served (in Years) for Habitual Offenders and Non-Habitual Offenders by Crime Type: Admissions from October, 1988 to June, 1998

Offense	Habituals	Non-Habituals
Homicide	28.2	14.0
Sexual Offense	19.7	6.7
Robbery	15.1	3.3
Other violent	10.6	2.2
Burglary	8.5	1.8
Theft/Forgery	5.0	1.2
Drugs	6.0	1.3
Weapons	6.5	1.4
Other	5.9	1.5
All Crime Types	9.6	2.5

Source: Florida Department of Corrections, Bureau of Research and Data Analysis
Note: The length of time-served for those still incarcerated was calculated by taking the amount of gain-time earned over the past 12 months and applying that amount to the remaining portion of the offenders sentence.

TABLE 10
Variables Used in the Multivariate Aggregate Level Analysis: Means and Standard Deviations

Variable	Mean		Standard Deviation	
Target Independent Variables (defined per 100,000 county population):	Unlogged	Logged	Unlogged	Logged
Extra HO Months (HOPM)	628.81	3.88	1011.30	3.20
Habitual Offender Admission Rate (HOA)	7.66	1.32	11.97	1.31
Habitual Offender Incarceration Rate (HOI)	20.36	1.87	30.89	1.67
Non-Habitual Offender Incarceration Rate (NHOI)	313.93	5.62	171.28	.507
Population Characteristics:				
Percent Males 15-24	7.49	1.98	2.01	.262
Percent Males 25-34	7.59	1.99	2.20	.248
Percent Unemployed	6.91	1.87	2.51	.358
Real per capita income data (in real 1992 dollars)	16432.58	9.67	4850.09	.273
Crime rates are defined per 100,000 people:				
Total Crime Rate	5037.08	8.30	2683.46	.886
Homicide	8.34	1.89	7.30	.936
Rape	39.82	3.35	26.53	1.09
Robbery	134.31	4.30	141.37	1.37
Assault	469.69	5.93	253.86	.848
Burglary	1355.31	6.98	707.70	.928
Larceny	2729.13	7.64	1518.30	.981
Auto-Theft	301.85	5.31	275.43	1.11

Source: Florida Department of Corrections—Admissions File; Florida Department of Law Enforcement—UCR Computer Disk.

TABLE 11

Granger Analyses of Impact of Individual Crime Types on HO Admission Rates, 1983-1997

	One-Year Lag		Two-Year Lag		F Value	
	b	*t*-ratio	b	*t*-ratio	Value	Probability Level
Total Crime	-.003	-.04	.045	.58	.17	.84
Homicide	-.007	-.19	**.059**	1.69	1.44	.24
Rape	.042	1.00	.015	.36	.61	.54
Robbery	**.104**	2.09	.001	.02	2.18	.11
Assault	-.005	-.07	.013	.20	.02	.98
Burglary	-.041	.67	-.025	-.37	.26	.77
Larceny	.012	.18	.028	.39	.12	.89
Auto-Theft	.064	1.24	.052	.95	1.48	.23

Note--This table summarizes regressions in which the dependent variable is HOA rates. The data start in 1981 but two years are lost due to lagged IVs and DVs. While only the results for the lags of crimes are reported, all the control variables are the same as those used in Appendix C. The two columns below each of the first two columns are the coefficients and absolute values of the t-statistics and the F value is for the two lags. All the regressions use weighted least squares where the weighting is the square root of each county's population. Coefficients in bold are significant at the .05 level (1-tail test).

TABLE 12

Granger Analyses of Impact of Individual Crime Types on HO Incarceration Rates, 1983-1997

	One-Year Lag		Two-Year Lag		F Value	
	b	*t*-ratio	b	*t*-ratio	Value	Probability Level
Total Crime	.055	1.23	.073	1.53	2.45	.09
Homicide	-.003	-.15	-.033	-1.52	1.18	.31
Rape	.004	.14	.016	.60	.20	.82
Robbery	.024	.77	.049	1.49	1.41	.25
Assault	.028	.70	.027	.67	.62	.54
Burglary	.055	1.47	**.095**	2.27	4.25	.01
Larceny	.047	1.10	**.088**	1.97	3.37	.03
Auto-Theft	**.072**	2.27	.023	.67	3.21	.04

Note--This table summarizes regressions in which the dependent variable is HOI rates. The data start in 1981but two years are lost due to lagged IVs and DVs. While only the results for the lags of crimes are reported, all the control variables are the same as those used in Appendix C. The two columns below each of the first two columns are the coefficients and absolute values of the t-statistics and the F value is for the two lags. All the regressions use weighted least squares where the weighting is the square root of each county's population. Coefficients in bold are significant at the .05 level (1-tail test).

TABLE 13

Granger Analyses of Impact of HO Admission Rates on Crime Levels, 1983-1997

	Coefficients on HOA Rates Lagged One and Two-Years					
	One-Year Lag		Two-Year Lag		F Value	
Dependent Variable	b	*t*-ratio	b	*t*-ratio	Value	Probability Level
Total Crime	.011	.60	.015	.84	.49	.61
Homicide	-.019	-.50	.017	.49	.27	.77
Rape	.016	.49	-.026	-.89	.56	.57
Robbery	-.009	-.33	**-.051**	-2.02	2.06	.13
Assault	.025	1.26	.027	1.47	1.73	.18
Burglary	.001	.023	.001	.021	.00	.99
Larceny	-.006	-.27	.011	.58	.22	.80
Auto-Theft	.014	.55	-.016	-.66	.40	.67

Note--This table summarizes regressions in when regressing different crime types on HOA rates lagged one and two-years. The data start in 1981 but two years are lost due to lagged IVs and DVs. While only the results for the lags of HOA rates are reported, all the control variables are the same as those used in Appendix C. The two columns below each of the first two columns are the coefficients and absolute values of the t-statistics and the F value is for the two lags. All the regressions use weighted least squares where the weighting is the square root of each county's population. Coefficients in bold are significant at the .05 level (1-tail test).

TABLE 14

Granger Analyses of Impact of HO Incarceration Rates on Crime Levels, 1983-1997

	Coefficients on HOI Rates Lagged One and Two-Years					
	One-Year Lag		Two-Year Lag		F Value	
Dependent Variable	b	*t*-ratio	b	*t*-ratio	Value	Probability Level
Total Crime	.003	.10	.012	.48	.20	.82
Homicide	.049	1.03	-.007	-.14	.63	.54
Rape	**-.077**	-1.87	.028	.70	1.79	.17
Robbery	-.021	-.59	.035	1.00	.50	.60
Assault	.024	.93	-.010	-.38	.44	.64
Burglary	-.021	-.72	.031	1.08	.60	.55
Larceny	.010	.35	.009	.35	.26	.77
Auto-Theft	.020	.59	.017	.52	.63	.53

Note--This table summarizes regressions in when regressing different crime types on HOI rates lagged one and two-years. The data start in 1981 but two years are lost due to lagged IVs and DVs. While only the results for the lags of HOA rates are reported, all the control variables are the same as those used in Appendix C. The two columns below each of the first two columns are the coefficients and absolute values of the t-statistics and the F value is for the two lags. All the regressions use weighted least squares where the weighting is the square root of each county's population. Coefficients in bold are significant at the .05 level (1-tail test).

TABLE 15
Estimated Impact of Florida's Habitual Offender Law on Crime Rates: County-Level Cross-Sectional Time-Series Evidence

Target Independent Variable: Extra HO prison months per 100,000 county population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.006	.883	-.010	-.811	-.006	-.703	.009	.882
HOPM, 2 year lag	.005	.836	.009	.791	**-.016**	-1.88	-.010	-.965
HOPM, 3 year lag	.001	.229	.005	.432	.002	.301	.002	.202
HOPM, 4 year lag	.002	.364	-.009	-.772	-.004	-.452	-.011	-1.14
HOPM, 5 year lag	-.003	-.453	-.003	-.258	.007	.812	-.016	-1.58
HOPM, 6 year lag	.0003	.037	**-.028**	-2.39	.004	.422	.017	1.68

TABLE 15 (Continued)
Estimated Impact of Florida's Habitual Offender Law on Crime Rates: County-Level Cross-Sectional Time-Series Evidence

Target Independent Variable: Extra HO prison months per 100,000 county population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.006	.831	.004	.479	.002	.357	.007	.780
HOPM, 2 year lag	.006	.991	.003	.362	.004	.661	-.005	-.539
HOPM, 3 year lag	.003	.408	-.006	-.893	.004	.593	-.0004	-.054
HOPM, 4 year lag	.004	.595	.002	.327	.003	.495	.007	1.00
HOPM, 5 year lag	-.004	-.658	.004	.473	-.005	-.634	.004	.585
HOPM, 6 year lag	-.004	-.554	.002	.232	-.001	-.083	-.001	-.061

Note: This table summarizes crime regressions in which the HOPM variables are lagged one to six years. Only one lagged HOPM variable is included in any one crime model. The remaining results for the crime regressions are reported in Appendix C. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

TABLE 16

Regressions reported in Table 15 re-estimated with Lagged Dependent Variables

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.003	.52	-.011	-.87	-.004	-.38	.006	.53
HOPM, 2 year lag	.002	.36	.016	1.35	**-.015**	-1.77	-.009	-.82
HOPM, 3 year lag	.005	.82	.005	.47	.004	.50	.005	.52
HOPM, 4 year lag	.00004	.01	-.008	-.68	-.005	-.54	-.011	-1.17
HOPM, 5 year lag	.0001	.01	-.001	-.007	.007	.84	-.014	-1.50
HOPM, 6 year lag	-.001	-.14	**-.028**	-2.39	.003	.30	**.018**	1.86

TABLE 16 (Continued)

Regressions reported in Table 15 re-estimated with Lagged Dependent Variables

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.003	.42	.001	.21	-.002	-.28	.003	.31
HOPM, 2 year lag	.006	.98	.002	.32	.003	.40	-.007	-.88
HOPM, 3 year lag	.003	.44	-.006	-.82	.006	1.04	.004	.51
HOPM, 4 year lag	.003	.47	.003	.41	.0002	.03	.006	.78
HOPM, 5 year lag	-.004	-.67	.004	.52	-.002	-.24	.004	.45
HOPM, 6 year lag	-.003	-.46	.001	.11	-.001	-.09	-.001	-.16

Note: This table is the result of regressions that are the same in Table 15 except that lagged dependent variable are included in the crime regressions in order to correct for autocorrelation. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

TABLE 17

Regressions reported in Table 15 re-estimated without Habitual Offender Incarceration Rates

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.003	.461	-.004	-.363	-.008	-1.02	.006	.661
HOPM, 2 year lag	.003	.588	.010	.887	**-.017**	-2.04	-.009	-.912
HOPM, 3 year lag	.0003	.056	.007	-1.01	.001	.128	.002	.157
HOPM, 4 year lag	.002	.352	-.009	-.766	-.004	-.464	-.011	-1.14
HOPM, 5 year lag	-.002	-.255	-.004	-.381	.008	.988	-.015	-1.52
HOPM, 6 year lag	.002	.302	**-.029**	-2.51	.006	.733	**.018**	1.77

TABLE 17 (Continued)

Regressions reported in Table 15 re-estimated without Habitual Offender Incarceration Rates

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	-.001	-.083	.001	.182	.003	.481	.008	1.01
HOPM, 2 year lag	.004	.595	.001	.164	.005	.748	-.001	-.135
HOPM, 3 year lag	.001	.200	-.007	-1.04	.004	.587	.001	.159
HOPM, 4 year lag	.004	.587	.002	.315	.003	.492	.009	1.01
HOPM, 5 year lag	-.003	-.524	.005	.669	-.004	-.535	.004	.434
HOPM, 6 year lag	-.002	-.315	.004	.517	.0004	.060	-.002	-.189

Note: This table is the result of regressions that are the same in Table 15 except that the HOI variable is dropped from the crime regressions. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. All crime regressions use weighting where the weighting is the square root of each county's population. Coefficients in bold are significant at the .05 level (1-tail test).

Table 18

Re-running the Regressions Reported in Table 15 with Variables Not Logged

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.037	.734	.0004	1.48	**.006**	1.70	-.0002	-.253
HOPM, 2 year lag	**.099**	1.95	.0005	1.65	**.006**	1.73	-.0005	-.524
HOPM, 3 year lag	.077	1.61	.0001	.194	**.009**	2.76	.00004	.045
HOPM, 4 year lag	-.014	-.337	**-.001**	-2.19	**.006**	2.15	-.001	-.873
HOPM, 5 year lag	**-.095**	-2.38	-.0003	-1.12	-.002	-.731	**-.002**	-2.79
HOPM, 6 year lag	**-.090**	-2.18	**-.001**	-2.78	-.004	-1.61	-.0001	-.116

Table 18 (Continued)

Re-running the Regressions Reported in Table 15 with Variables Not Logged

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.005	.653	.016	.919	.028	.961	-.0003	-.027
HOPM, 2 year lag	.007	1.01	.029	1.65	**.054**	1.85	.012	1.25
HOPM, 3 year lag	.002	.269	.007	.417	.045	1.63	**.018**	2.03
HOPM, 4 year lag	-.006	-.987	-.020	-1.29	-.004	-.150	-.0004	-.057
HOPM, 5 year lag	**-.014**	-2.42	**-.037**	-2.63	-.046	-1.92	-.010	-1.48
HOPM, 6 year lag	-.013	-2.17	**-.032**	-2.30	-.037	-1.50	**-.015**	-2.29

Note: This table is the result of regressions that are the same in Table 15 except that the variables are not logged. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

Table 19
Re-running the Regressions Reported in Table 15 with Variables First-Differenced

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.009	1.01	.027	1.37	.021	1.49	.022	1.38
HOPM, 2 year lag	.003	.57	-.015	-1.23	-.0001	-.02	.001	.11
HOPM, 3 year lag	-.0004	-.09	.010	.97	**-.016**	-2.15	-.014	-1.58
HOPM, 4 year lag	.002	.31	.006	.62	**.014**	1.88	.012	1.40
HOPM, 5 year lag	-.001	-.23	-.011	-1.01	-.012	-1.57	-.006	-.71
HOPM, 6 year lag	-.002	-.41	.011	.99	.005	.71	-.015	-1.70

Table 19

Re-running the Regressions Reported in Table 15 with Variables First-Differenced

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.014	1.42	.012	1.16	.008	.87	.004	.35
HOPM, 2 year lag	.001	.11	.002	.32	.001	.15	.010	1.37
HOPM, 3 year lag	.001	.12	.002	.30	-.001	-.10	-.009	-1.37
HOPM, 4 year lag	.00004	.01	-.005	-.90	.003	.67	.002	.34
HOPM, 5 year lag	.003	.54	-.001	-.22	-.001	-.21	.002	.27
HOPM, 6 year lag	-.004	-.77	.004	.55	-.003	-.57	-.001	-.14

Note: This table is the result of regressions that are the same in Table 15 except that the data are first-differenced (first-differencing cancels out the unit dummies). The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

Table 20
Re-running the Regressions Reported in Table 15 without Weighting the Regressions

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	**.019**	2.06	-.007	-.48	-.008	-.65	.014	1.06
HOPM, 2 year lag	**.015**	1.65	.008	.54	**-.024**	-2.08	-.017	-1.28
HOPM, 3 year lag	-.0004	-.04	-.005	-.37	.013	1.14	.002	.13
HOPM, 4 year lag	.002	.16	-.020	-1.37	-.007	-.55	-.017	-1.23
HOPM, 5 year lag	-.011	-1.01	.010	.68	.011	.90	**-.032**	-2.28
HOPM, 6 year lag	-.001	-.06	**-.031**	-1.95	.008	.65	**.030**	2.09

Table 20

Re-running the Regressions Reported in Table 15 without Weighting the Regressions

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	**.018**	1.86	.015	1.37	.012	1.30	**.031**	2.65
HOPM, 2 year lag	**.017**	1.94	.008	.81	.011	1.15	-.003	-.23
HOPM, 3 year lag	.0002	.02	-.014	-1.34	.002	.21	-.006	-.56
HOPM, 4 year lag	.003	.30	-.001	-.08	.003	.32	.005	.41
HOPM, 5 year lag	-.012	-1.21	.005	.43	-.011	-1.02	.001	.05
HOPM, 6 year lag	-.008	-.73	.007	.55	-.003	-.22	-.002	-.16

Note: This table is the result of regressions that are the same in Table 15 except that the regressions are not weighted. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

Table 21
Re-running the Regressions Reported in Table 15 with Quadratic Trends Variables

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.007	1.17	-.010	-.835	.0004	.043	.005	.446
HOPM, 2 year lag	.005	.752	.011	.887	**-.014**	-1.70	-.010	-.994
HOPM, 3 year lag	.0002	.048	.010	.878	.001	.157	.001	.121
HOPM, 4 year lag	-.003	-.442	-.004	-.328	-.013	-1.54	-.013	-1.29
HOPM, 5 year lag	-.010	-1.40	.00002	.001	-.003	-.341	-.016	-1.55
HOPM, 6 year lag	-.001	-.186	**-.026**	-2.14	-.001	-.105	**.018**	1.71

Table 21
Re-running the Regressions Reported in Table 15 with Quadratic Trend Variables

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.006	.920	.007	1.03	.004	.593	.006	.674
HOPM, 2 year lag	.005	.772	.002	.365	.004	.563	-.005	-.613
HOPM, 3 year lag	-.000004	-.001	-.008	-1.10	.003	.469	-.001	-.089
HOPM, 4 year lag	-.0002	-.028	-.004	-.594	-.002	-.332	.004	.529
HOPM, 5 year lag	**-.012**	-1.85	-.003	-.380	-.011	-1.52	-.003	-.308
HOPM, 6 year lag	-.008	-1.18	-.001	-.136	-.002	-.220	-.004	-.413

Note: This table is the result of regressions that are the same to Table 15 except that quadratic trends variables are added to the regressions. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

Table 22
Re-running the Regressions Reported in Table 15 with Data Starting in 1988

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.009	.90	-.012	-.71	.009	.70	.006	.40
HOPM, 2 year lag	.006	.66	.016	1.04	-.015	-1.33	-.021	-1.56
HOPM, 3 year lag	-.001	-.12	.006	.40	-.001	-.09	-.008	-.67
HOPM, 4 year lag	-.0001	-.01	-.012	-.89	-.010	-1.02	-.016	-1.32
HOPM, 5 year lag	-.009	-1.16	-.002	-.16	-.001	-.07	-.013	-1.16
HOPM, 6 year lag	-.003	-.39	**-.029**	-2.26	.005	.57	**.020**	1.77

Table 22

Re-running the Regressions Reported in Table 15 with Data Starting in 1988

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.008	.82	.013	1.13	.003	.24	.009	.66
HOPM, 2 year lag	.009	1.01	.004	.36	.005	.48	-.014	-1.19
HOPM, 3 year lag	-.004	-.54	-.010	-1.03	.002	.21	-.004	-.36
HOPM, 4 year lag	.008	1.01	-.004	-.39	-.0002	-.02	.009	.87
HOPM, 5 year lag	**-.012**	1.67	-.0003	-.03	-.011	-1.33	.002	.23
HOPM, 6 year lag	-.004	-.58	-.003	-.29	-.005	-.61	-.007	-.67

Note: This table is the result of regressions that are the same in Table 15 except that the data starts in 1988. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

Table 23

Re-running the Regressions Reported in Table 15 with No Specific Control Variables

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.002	.285	-.005	-.417	-.007	-.939	.005	.494
HOPM, 2 year lag	.002	.345	.011	.960	**-.016**	-2.00	-.010	-1.11
HOPM, 3 year lag	-.001	-.156	.008	.712	.001	.153	.001	.135
HOPM, 4 year lag	.001	.129	-.007	-.637	-.004	-.494	-.011	-1.13
HOPM, 5 year lag	-.002	-.317	-.003	-.298	.008	.912	-.015	-1.53
HOPM, 6 year lag	.002	.332	**-.030**	-2.56	.006	.764	**.019**	1.89

Table 23

Re-running the Regressions Reported in Table 15 with No Specific Control Variables

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	-.001	-.111	.001	.104	.002	.261	.009	1.10
HOPM, 2 year lag	.003	-.467	-.0004	-.065	.003	.469	-.003	-.407
HOPM, 3 year lag	.0002	.040	-.009	-1.25	.002	.308	-.003	-.306
HOPM, 4 year lag	.003	.489	.001	.072	.001	.192	.003	.352
HOPM, 5 year lag	-.003	-.521	.005	.590	-.004	-.605	.0001	.007
HOPM, 6 year lag	-.002	-.245	.005	.626	.001	.090	-.002	-.234

Note: This table is the result of regressions that are the same in Table 15 except that the specific control variables are dropped from the crime regressions. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios Coefficients in bold are significant at the .05 level (1-tail test).

Table 24

Re-running the Regressions Reported in Table 15 without Year Dummies

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.004	.683	-.004	-.370	.001	.136	.009	.902
HOPM, 2 year lag	.005	.916	.008	.786	-.011	-1.37	-.006	-.685
HOPM, 3 year lag	-.001	-.148	-.002	-.228	-.002	-.295	.001	.091
HOPM, 4 year lag	.0001	.014	-.017	-1.72	-.011	-1.44	-.011	-1.24
HOPM, 5 year lag	-.003	-.525	-.009	-.883	-.001	-.086	**-.016**	-1.82
HOPM, 6 year lag	.002	.320	**-.025**	-2.38	.002	.323	.015	1.63

Table 24 (Continued)

Re-running the Regressions Reported in Table 15 without Year Dummies

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.008	1.18	.003	.365	-.0003	-.053	.002	.178
HOPM, 2 year lag	.007	1.17	.003	.377	.004	.722	-.005	-.591
HOPM, 3 year lag	.0004	.062	-.009	-1.39	.001	.213	-.001	-.150
HOPM, 4 year lag	.001	.262	-.003	-.442	.001	.229	.007	.966
HOPM, 5 year lag	-.005	-.782	-.002	-.225	-.001	-.197	.005	.605
HOPM, 6 year lag	-.001	-.141	.003	.388	.004	.544	.002	.264

Note: This table is the result of regressions that are the same in Table 15 except that the year dummies are dropped from the crime regressions. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

Table 25
Re-running the Regressions Reported in Table 15 without County Trend Variables

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.004	.738	-.007	-.587	-.013	-1.56	.016	1.53
HOPM, 2 year lag	.004	.638	.012	1.07	**-.018**	-2.15	-.007	-.737
HOPM, 3 year lag	.002	.364	.008	.707	.0003	.036	.002	.220
HOPM, 4 year lag	.002	.295	-.011	-.940	-.005	-.667	-.012	-1.19
HOPM, 5 year lag	-.002	-.321	-.004	-.336	.004	.493	**-.018**	-1.84
HOPM, 6 year lag	-.002	-.362	**-.025**	-2.15	.001	.094	.007	.699

Table 25 (Continued)

Re-running the Regressions Reported in Table 15 without County Trend Variables

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	-.002	-.246	.005	.734	.004	.621	-.014	-1.58
HOPM, 2 year lag	.002	.290	.005	.641	.004	.576	**-.018**	-2.21
HOPM, 3 year lag	-.0001	-.022	-.002	-.324	.004	.599	-.011	-1.35
HOPM, 4 year lag	.002	.256	.005	.639	.001	.221	-.003	-.334
HOPM, 5 year lag	-.006	-.932	.007	.912	-.005	-.698	-.005	-.538
HOPM, 6 year lag	-.008	-1.18	.002	.283	-.005	-.645	-.008	-.950

Note: This table is the result of regressions that are the same in Table 15 except that the county trend variables are dropped from the crime regressions. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

Table 26

Re-running the Regressions Reported in Table 15 without Year and County Trend Variables

Target Independent Variable: Extra HO prison months per 100,000 population	Total Crime		Homicide		Robbery		Rape	
	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.004	.663	-.003	-.273	-.007	-.812	.015	1.50
HOPM, 2 year lag	.002	.281	.004	.394	**-.019**	-2.34	.0001	.015
HOPM, 3 year lag	-.002	-.431	-.008	-.798	-.007	-1.28	.008	.893
HOPM, 4 year lag	-.002	-.351	**-.023**	-2.48	**-.015**	-2.19	-.003	-.703
HOPM, 5 year lag	-.003	-.602	**-.016**	-1.64	-.007	-1.10	-.006	-1.40
HOPM, 6 year lag	-.0001	-.016	**-.025**	-2.57	-.003	-.499	.008	.937

Table 26 (Continued)

Re-running the Regressions Reported in Table 15 without Year and County Trend Variables

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	-.00005	-.007	.005	.699	.003	.512	**-.018**	-2.14
HOPM, 2 year lag	.004	.607	-.001	-.119	.002	.327	**-.021**	-2.58
HOPM, 3 year lag	.001	.158	-.010	-1.60	-.001	-.211	-.012	-1.66
HOPM, 4 year lag	.002	.383	-.005	-.795	-.002	-.363	-.003	-.364
HOPM, 5 year lag	-.004	-.746	-.002	-.294	-.003	-.542	-.004	-.517
HOPM, 6 year lag	-.002	-.419	.002	.243	-.0001	-.015	-.005	-.674

Note: This table is the result of regressions that are the same in Table 15 except that the year and county trend variables are dropped from the crime regressions. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

TABLE 27

Estimated Impact of Florida's Habitual Offender Law on Crime Rates: Sample Where County Population is Greater than 100,000

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	-.001	-.251	-.016	-1.43	-.003	-.516	**.017**	2.14
HOPM, 2 year lag	-.002	-.649	.001	.089	**-.014**	-2.27	.0001	.017
HOPM, 3 year lag	.0001	.047	**.018**	1.70	-.008	-1.43	**.013**	1.76
HOPM, 4 year lag	-.004	-1.63	.004	.407	-.004	-.763	-.002	-.276
HOPM, 5 year lag	**-.005**	-2.04	**-.028**	-2.67	.004	.758	-.001	-.103
HOPM, 6 year lag	-.001	-.419	**-.034**	-3.26	.005	1.01	.008	1.19

TABLE 27 (Continued)
Estimated Impact of Florida's Habitual Offender Law on Crime Rates: Sample Where County Population is Greater than 100,000

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	-.0002	-.034	-.002	-.452	.001	.313	-.011	-1.64
HOPM, 2 year lag	-.0002	-.037	-.003	-.707	-.00002	-.007	-.006	-.906
HOPM, 3 year lag	.004	.856	-.005	-1.61	.001	.235	-.001	-.198
HOPM, 4 year lag	.001	.264	-.001	-.365	**-.006**	-2.16	-.001	-.086
HOPM, 5 year lag	-.002	-.439	-.004	-1.32	**-.008**	-2.88	-.002	-.293
HOPM, 6 year lag	.001	.344	-.003	-1.13	-.001	-.196	-.002	-.318

Note: This table is the result of regressions that are the same in Table 15 except that only counties with populations greater than 100,000 in 1989 are included in the crime regressions. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

TABLE 28

Estimated Impact of Florida's Habitual Offender Law on Crime Rates: Sample Where County Population is Between 25,000 to 99,999

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	-.005	-1.00	-.010	-.436	**-.020**	-1.92	.003	.210
HOPM, 2 year lag	.004	.917	**.050**	2.40	-.001	-.143	-.006	-.411
HOPM, 3 year lag	.004	.925	.011	.491	.002	.249	-.014	-.872
HOPM, 4 year lag	.007	1.39	-.010	-.472	.012	1.20	-.007	-.401
HOPM, 5 year lag	.004	.877	.009	.390	.011	1.10	-.017	-1.03
HOPM, 6 year lag	.006	1.08	-.003	-.120	.006	.544	-.003	-.195

TABLE 28 (Continued)
Estimated Impact of Florida's Habitual Offender Law on Crime Rates: Sample Where County Population is Between 25,000 to 99,999

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.009	1.12	-.003	-.576	-.011	-1.64	-.009	-1.03
HOPM, 2 year lag	.004	.553	.006	.988	.007	1.09	.0001	.007
HOPM, 3 year lag	.006	.757	.005	.885	.007	1.13	.001	.166
HOPM, 4 year lag	.005	.563	.007	1.32	.006	.955	.015	1.65
HOPM, 5 year lag	.004	.438	.002	.264	.005	.739	.007	.791
HOPM, 6 year lag	-.005	-.519	.004	.672	.009	1.30	.009	.915

Note: This table is the result of regressions that are the same in Table 15 except that only counties with populations between 25,000 to 99,999 are included in the crime regressions. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

TABLE 29

Estimated Impact of Florida's Habitual Offender Law on Crime Rates: Sample Where County Population is Less than 25,000

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.038	1.61	.005	.126	.002	.070	-.001	-.039
HOPM, 2 year lag	.008	.363	.005	.138	-.041	-1.35	-.049	-1.49
HOPM, 3 year lag	-.016	-.655	-.041	-1.16	.024	.778	.015	.418
HOPM, 4 year lag	-.021	-.731	-.042	-1.11	-.035	-1.03	-.042	-1.10
HOPM, 5 year lag	-.037	-1.21	.042	1.06	-.008	-.227	**-.080**	-2.07
HOPM, 6 year lag	-.010	-.276	-.066	-1.52	.007	.185	**.102**	2.44

TABLE 29 (Continued)
Estimated Impact of Florida's Habitual Offender Law on Crime Rates: Sample Where County Population is Less than 25,000

Target Independent Variable: Extra HO prison months per 100,000 population	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
HOPM, 1 year lag	.024	1.03	.033	1.24	.020	.799	**.086**	2.94
HOPM, 2 year lag	.017	.794	-.010	-.404	.003	.122	-.016	-.578
HOPM, 3 year lag	-.014	-.609	**-.050**	-1.79	-.015	-.595	-.038	-1.30
HOPM, 4 year lag	-.009	-.370	-.040	-1.24	-.006	-.199	-.031	-.938
HOPM, 5 year lag	-.030	-1.13	.013	.389	-.039	-1.23	-.018	-.504
HOPM, 6 year lag	-.021	-.695	.006	.153	-.018	-.493	-.043	-1.10

Note: This table is the result of regressions that are the same in Table 15 except that only counties with populations below 25,000 in 1989 are included in the crime regressions. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

TABLE 30

Re-running the Regressions Reported in Table 10 with a Habitual Offender Law Dummy Variable

Dependent Variable:	Current-Year		One-Year Lag		Two-Year Lag		Three-Year Lag	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
Total Crime	-.010	-.249	-.075	-1.62	**-.087**	-1.66	-.152	-.063
Homicide	.102	1.28	.005	.057	-.019	-.196	-.150	-1.59
Rape	-.075	-1.12	-.084	-1.11	-.058	-.687	-.013	-.163
Robbery	**.113**	1.94	.029	.436	-.030	-.405	**-.152**	-2.18
Assault	.048	1.06	.084	1.66	.007	1.27	-.008	-.157
Burglary	-.025	-.521	**-.157**	-2.87	**-.181**	-2.93	**-.154**	-2.52
Larceny	-.055	-1.30	**-.082**	-1.67	-.041	-.737	-.038	-.691
Auto-Theft	.038	.656	-.074	-1.41	-.102	-1.41	-.109	-1.54

Note--This table summarizes regressions in which the habitual law dummy variables are current year, one-year, two-year, and three-year lagged. While not all of the coefficient estimates are reported, all the control variables are the same as those used in Table 22, excluding lagged HOPM variables, HOA rates, HOI rates, and year dummies. The two columns below each column are the coefficients and absolute values of the t-statistics. Coefficients in bold are significant at the .05 level (1-tail test).

TABLE 31

Re-running the Regressions Reported in Table 24 with Short-Term and Distributed Habitual Offender Law Dummy Variables

Dependent Variable:	2-Year Short-Term		3-Year Short-Term		4-Year Short-Term		Distributed Lag	
	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
Total Crime	.010	.328	.001	.040	.020	.757	.017	.853
Homicide	.042	.713	**.089**	1.68	.084	1.63	.065	1.63
Rape	-.001	-.023	-.017	-.391	.006	.137	-.018	-.544
Robbery	.060	1.40	**.090**	2.33	**.126**	3.33	.105	3.65
Assault	.032	.956	.020	.661	**.056**	1.91	**.069**	3.09
Burglary	.027	.745	.016	.506	.032	1.03	.021	.896
Larceny	-.032	-1.01	-.021	-.742	-.011	-.391	-.007	-.325
Auto-Theft	.031	.716	.026	.674	.020	.532	.037	1.30

Note--This table summarizes regressions in which the habitual law dummy variables are current year, one-year, two-year, and three-year lagged. While not all of the coefficient estimates are reported, all the control variables are the same as those used in Table 22, excluding lagged HOPM variables, HOA rates, HOI rates, and year dummies. The two columns below each column are the coefficients and absolute values of the t-statistics. Coefficients in bold are significant at the .05 level (1-tail test).

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APPENDIX A

Variables Included in Habitual Offender Sentencing Disparity Analysis (N=271,015)

VARIABLE	DESCRIPTION
Individual Level Data	
HABITUAL	Denotes whether the offender was sentenced as an habitual offender. It is a constructed dichotomous variable, Habitual=1/Not Habitual =0.
PRIMOFF	The specific primary offenses were categorized into nine groups. Eight dummy variables were created with robbery as the reference category. Other categories include murder/manslaughter, sexual/lewd lascivious behavior, other violent, burglary, property, drugs, weapons, and other.
RACE	Denotes race of offender. It is a constructed dichotomous variable, Black=1/White=0.
AGE	Age of offender at admission, in years.
GENDER	Denotes gender of offender. It is a constructed dichotomous variable, Male=1/Female=0.
MARITAL	Denotes marital status of offender at time of admission. It is a constructed dichotomous variable, Married=1/Not Married=0.
PRIORS	Total number of prior prison commitments to the FDC.

APPENDIX A (Continued)

VARIABLE *Individual Level Data*	DESCRIPTION
FELCLASS	Felony class of primary offense, 4 constructed dummy variables with 1st Degree Felony as reference category. Other categories include Capital Offense, Life Offense, 2nd Degree Felony, and 3rd Degree Felony.
GUILTY	Offender plead guilty to primary offense. It is a constructed dichotomous variable, Guilty Plea=1/No Guilty Plea=0.
COUNTS	Measured as the total number of counts for which the offender was sentenced.
CIRCUIT	Denotes judicial circuit in which the offender was sentenced, 19 dummy variables with circuit 1 as reference category. Other categories include circuits 2-circuit 20)
PROBAT	Denotes whether the offender violated probation. It is a constructed dichotomous variable, Violator=1/No Violator=0.
QUALIF	Primary offense qualifier denotes whether the offender committed, attempted to commit, or conspired to commit the primary offense. It is a constructed dichotomous variable, 1=committed primary offense/attempted or conspired to commit primary offense=0.

Appendix A (Continued)
OLS Estimates of Habitual Offender Sentencing on Sentence Length (In Months)
for all Crime Types, 1989-1997 (N=271,015)

	1989		1990	
Target Independent Variables:	b	*t*-ratio	b	*t*-ratio
HABITUAL	102.79	61.82	98.09	77.42
PRIMOFF				
HOMICIDE	87.70	52.24	104.15	52.55
SEXUAL	52.22	30.78	50.54	26.72
VIOLENT	-11.15	-8.85	-10.81	-7.46
BURGLARY	-16.21	-14.96	-16.96	-13.36
PROPERTY	-16.86	-14.96	-18.31	-13.39
DRUGS	-18.86	-14.42	-18.91	-16.03
WEAPON	-21.51	-12.11	-23.02	-11.38
OTHER	-24.31	-15.49	-24.71	-12.77
PRIORS	8.98	36.30	8.35	30.87
FELCLASS				
FELCAP	393.30	96.24	366.09	87.03
FELLIF	74.43	42.23	70.77	37.25
FELSEC	-47.61	-57.06	-48.67	-49.86
FELTHIRD	-54.55	-60.83	-56.52	-53.66
COUNTS	4.23	39.01	5.75	38.67
QUALIF	2.39	1.82	7.39	4.99
GUILTY	-5.94	-9.63	-5.45	-7.87
PROBAT	-1.59	-2.75	-2.76	-4.16
RACE	-2.19	-3.98	-2.29	-3.57
MARITAL	.544	.749	1.37	1.62
AGE	.085	2.58	.228	6.15
SEX	2.99	3.39	3.51	3.44
N	44,053		41,488	
Adj. R^2	.54		.55	

Appendix A (Continued)
OLS Estimates of Habitual Offender Sentencing on Sentence Length (In Months) for all Crime Types, 1989-1997 (N=271,015)

	1991		1992	
Target Independent Variables:	b	*t*-ratio	b	*t*-ratio
HABITUAL	89.26	52.91	79.88	60.23
PRIMOFF				
HOMICIDE	106.00	39.16	87.18	37.64
SEXUAL	57.30	21.12	43.12	19.90
VIOLENT	-6.70	-3.21	-10.42	-6.13
BURGLARY	-15.92	-8.62	-16.19	-10.84
PROPERTY	-16.24	-8.11	-17.02	-10.50
DRUGS	-18.75	-10.88	-23.13	-16.50
WEAPON	-20.68	-7.05	-25.18	-10.49
OTHER	-20.24	-6.79	-28.16	-10.99
PRIORS	8.16	20.77	8.25	26.66
FELCLASS				
FELCAP	356.25	66.22	377.40	80.49
FELLIF	68.98	26.51	107.84	38.35
FELSEC	-45.63	-32.65	-52.41	-47.41
FELTHIRD	-54.45	-35.57	-63.24	-51.96
COUNTS	6.89	28.49	4.07	28.07
QUALIF	9.24	4.36	5.82	3.42
GUILTY	-7.17	-6.94	-6.72	-7.96
PROBAT	-5.40	-5.45	-3.00	-3.81
RACE	-3.55	-3.69	-3.44	-4.43
MARITAL	.714	.536	.212	.180
AGE	.260	4.86	.193	4.42
SEX	4.34	2.85	3.18	2.57
N	23,023		33,012	
Adj. R^2	.54		.54	

Appendix A (Continued)
OLS Estimates of Habitual Offender Sentencing on Sentence Length (In Months) for all Crime Types, 1989-1997 (N=271,015)

	1993		1994	
Target Independent Variables:	b	*t*-ratio	b	*t*-ratio
HABITUAL	75.32	48.95	81.08	47.25
PRIMOFF				
HOMICIDE	82.23	82.23	102.42	38.86
SEXUAL	37.06	16.51	44.42	17.89
VIOLENT	-13.90	-7.54	-8.97	-4.45
BURGLARY	-17.90	-11.10	-18.76	-10.40
PROPERTY	-15.77	-8.93	-17.25	-8.55
DRUGS	-24.32	-15.76	-21.75	-12.58
WEAPON	-24.90	-9.23	-24.40	-8.52
OTHER	-25.19	-8.82	-24.66	-8.18
PRIORS	7.94	24.08	5.03	13.58
FELCLASS				
FELCAP	380.32	80.23	365.84	79.90
FELLIF	102.80	34.74	80.47	27.12
FELSEC	-51.76	-42.39	-55.39	-41.73
FELTHIRD	-63.99	-47.45	-62.25	-41.18
COUNTS	3.80	24.44	8.31	29.92
QUALIF	6.38	3.47	11.83	5.66
GUILTY	-8.77	-9.36	-10.94	-10.41
PROBAT	-2.78	-3.21	-.150	-.153
RACE	-2.14	-2.50	-3.72	-3.88
MARITAL	-3.77	-2.39	4.42	3.04
AGE	.270	5.80	.383	7.21
SEX	5.83	4.20	5.90	3.77
N	28,537		24,344	
Adj. R^2	.53		.57	

Appendix A (Continued)
OLS Estimates of Habitual Offender Sentencing on Sentence Length (In Months) for all Crime Types, 1989-1997 (N=271,015)

	1995		1996	
Target Independent Variables:	B	*t*-ratio	b	*t*-ratio
HABITUAL	**75.46**	46.69	**68.83**	45.83
PRIMOFF				
HOMICIDE	99.11	37.38	87.81	34.31
SEXUAL	39.56	16.81	39.31	17.06
VIOLENT	-1.66	-.837	-5.40	-2.83
BURGLARY	-14.31	-7.76	-12.90	-7.22
PROPERTY	-14.34	-6.67	-19.44	-9.26
DRUGS	-19.88	-10.98	-23.81	-13.50
WEAPON	-13.42	-4.73	-15.29	-5.59
OTHER	-11.49	-3.88	-16.83	-6.11
PRIORS	3.12	8.20	2.74	7.38
FELCLASS				
FELCAP	376.49	79.41	391.96	92.28
FELLIF	72.17	22.99	74.41	23.81
FELSEC	-56.76	-42.68	-57.74	-44.51
FELTHIRD	-62.80	-39.95	-64.83	-42.70
COUNTS	6.90	23.29	6.89	23.69
QUALIF	12.61	5.85	13.16	6.42
GUILTY	-11.47	-10.49	-14.32	-13.54
PROBAT	4.05	3.96	-1.49	-1.51
RACE	-2.59	-2.56	1.04	1.07
MARITAL	.506	.358	3.81	2.78
AGE	.301	5.54	.331	6.48
SEX	5.42	3.22	4.43	2.62
N	21,029		21,512	
Adj. R^2	.56		.60	

Appendix A (Continued)
OLS Estimates of Habitual Offender Sentencing on Sentence Length (In Months) for all Crime Types, 1989-1997 (N=271,015)

	1997	
Target Independent Variables:	b	*t*-ratio
HABITUAL	67.08	43.36
PRIMOFF		
HOMICIDE	111.59	40.67
SEXUAL	46.98	19.34
VIOLENT	-.609	-.309
BURGLARY	-11.26	-6.06
PROPERTY	-19.18	-8.78
DRUGS	-24.32	-13.32
WEAPON	-8.01	-2.86
OTHER	-16.65	-5.78
PRIORS	4.11	11.00
FELCLASS		
FELCAP	366.93	74.80
FELLIF	64.69	19.29
FELSEC	-63.94	-47.13
FELTHIRD	-73.48	-46.22
COUNTS	7.07	25.71
QUALIF	17.22	8.07
GUILTY	-19.76	-17.65
PROBAT	-1.61	-1.55
RACE	.360	.350
MARITAL	.745	.522
AGE	.317	5.81
SEX	5.42	3.13
N	22,421	
Adj. R^2	.56	

Note: The two columns below each year are the coefficients and absolute values of the *t*-ratios. See top of Appendix A for a detailed description of variables. The coefficient estimates for the nineteen circuit dummies are not reported.

APPENDIX B

Full-Results for the Estimated Impact of Florida's Habitual Offender Law on Crime Rates: HOPM Variable Lagged One-Year

	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Total Crime		Homicide		Robbery		Rape	
Target Independent Variables:	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
Extra HO prison months per 100,000 population (HOPM), 1 year lag	.005	.883	-.010	-.811	-.006	-.703	.009	.882
Habitual Offender Admissions Rate (HRA)	.017	1.06	-.039	-1.23	.001	.039	.006	.231
Habitual Offender Incarceration Rate (HOI)	-.026	-1.10	.052	1.14	-.018	-.527	-.026	-.668
Specific Control Variables:								
Non-Habitual Offender Incarceration Rate (NHOI)	-.0001	-.001	-.125	-1.03	.065	.737	-.101	-.981
Percent Males 15-24	**-.701**	-2.05	.714	1.08	.122	.257	-.379	-.676
Percent Males 25-34	.372	1.36	.194	.365	-.486	-1.27	**-.939**	-2.09
Real Per-Capita Income (in 92 dollars)	-.121	-.305	-.074	-.096	-.093	-.168	-.455	-.696
Percent Unemployed	.052	.631	-.077	-.483	.021	.179	.168	1.24
N	1071		1071		1071		1071	
D.F.	916		916		916		916	
Adj. R^2	.82		.48		.82		.66	

APPENDIX B (Continued)

Full-Results for the Estimated Impact of Florida's Habitual Offender Law on Crime Rates: HOPM Variable Lagged One-Year

	Dependent Variables (Natural Logs of the Crime Rate per 100,000 People)							
	Assault		Burglary		Larceny		Auto-Theft	
Target Independent Variables:	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio	b	*t*-ratio
Extra HO prison months per 100,000 population (HOPM), 1 year lag	.006	.831	.004	.479	.002	.357	.007	.780
Habitual Offender Admissions Rate (HRA)	.023	1.27	.032	1.64	.018	1.01	**.062**	2.73
Habitual Offender Incarceration Rate (HOI)	**-.056**	-2.14	-.021	-.740	.005	.185	.011	.324
Specific Control Variables:								
Non-Habitual Offender Incarceration Rate (NHOI)	.050	.723	.063	.853	-.030	-.445	**.145**	1.67
Percent Males 15-24	-.010	-.027	-.392	-982	**-.886**	-2.45	-.554	-1.17
Percent Males 25-34	-.219	-.728	.495	1.54	**.524**	1.80	.278	.732
Real Per-Capita Income (in 92 dollars)	-.068	-.154	-.320	-.688	.034	.081	**-1.37**	-2.48
Percent Unemployed	.132	1.46	.104	1.08	.052	.593	.081	.713
N	1071		1071		1071		1071	
D.F.	916		916		916		916	
Adj. R^2	.78		.75		.83		.84	

Note: This table is the full set of results for the regressions reported in Table 15. Only the results for the one-year HOPM lag are presented. The two columns below each dependent variable are the coefficients and absolute values of the *t*-ratios. Coefficients in bold are significant at the .05 level (1-tail test).

APPENDIX C

Counties with populations greater than 100,000 in 1989:

Alachua, Bay, Brevard, Broward, Charlotte, Clay, Collier, Dade, Duval, Escambia, Hillsborough, Lake, Lee, Leon, Manatee, Marion, Okaloosa, Orange, Osceola, Palm Beach, Pasco, Pinellas, Polk, Sarasota, Seminole, St. Lucie, Volusia

Counties with populations between 25,000 to 100,000 in 1989:

Citrus, Columbia, Flagler, Gadsden, Hendry, Hernando, Highlands, Indian River, Jackson, Levy, Martin, Monroe, Nassau, Okeechobee, Putnam, Santa Rosa, St. Johns, Sumter, Suwanee, Walton

Counties with populations less than 25,000 in 1989:

Baker, Bradford, Calhoun, Desoto, Dixie, Franklin, Gilchrist, Glades, Gulf, Hamilton, Hardee, Holmes, Jefferson, Lafayette, Liberty, Madison, Taylor, Union, Wakulla, Washington