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FINAL TECHNICAL REPORT FOR 2011-MU-FX-4004

Juvenile Registration and Notification Policy Effects: A Multistate Evaluation Project

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Disclaimer

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Abstract

The goals of this project were to evaluate the impact of juvenile sex offender registration and notification policies on first-time sexual offenses (general deterrence; Goal 1), sexual and violent recidivism offenses (specific deterrence; Goal 2), and on juvenile case processing including case dismissals, diversions, and plea bargains (Goal 3). Previous research has failed to support general or specific deterrent effects but has linked juvenile registration policies to unintended increases in case dismissals, diversions, and plea bargains. We aimed to replicate these findings. To date, we have addressed Goal 1 by evaluating the general deterrent effects of six states' juvenile registration policies, Oregon, Maryland, Idaho, South Carolina, Utah, and Virginia. In no case was any state's policy associated with a general deterrent effect, thus replicating the existing research. We continue to pursue the remaining goals of this project. However, in light of the fact that no published study has identified any community safety benefit of juvenile registration and that several published studies have identified unintended policy effects on juvenile case processing and on children subjected to registration and/or notification requirements, we recommend that juvenile registration and notification policies be replaced with more effective approaches to the prevention of juvenile sexual offending.

Juvenile Registration and Notification Policy Effects: A Multistate Evaluation Project

Juvenile sex offender registration and notification policies are designed to improve community safety primarily by deterring sexual recidivism or by deterring first-time sexual offenses (Letourneau & Shields, 2016; Levenson, 2009). As reviewed below, all published studies, thus far, fail to find evidence for these intended effects. Subsequently, we will demonstrate that research conducted as part of this project also failed to find any evidence that juvenile sex offender registration contributes to improved community safety by achieving the primary objective of reducing harmful sexual behavior among children.

Background

Sex offender registration was federally mandated in 1994 following passage of the Jacob Wetterling Crimes Against Children and Sexually Violent Offender Act, which expanded to include public notification after the addendum of the Megan's Law in 1996 (Logan, 2009). These Acts directed U.S. states and other jurisdictions to subject some convicted sex offenders, including children convicted as adults, to registration and notification requirements; yet these Acts were silent regarding the registration of children adjudicated delinquent for sexual offenses as minors (McPherson, 2016). Nevertheless, many states expanded registration and notification requirements to include children adjudicated delinquent for sexual offenses as minors (Pittman & Nguyen, 2011) and subsequently, the Sex Offender Registration and Notification Act (SORNA), Title 1 of the Adam Walsh Child Protection and Safety Act of 2006, mandated that all jurisdictions include certain children adjudicated as minors in their registration schemes (McPherson, 2016).

The Department of Justice's Office of Sex Offender Sentencing, Monitoring, Apprehending, Registering, and Tracking ("SMART Office") is tasked with evaluating states' and other jurisdictions' policies for their substantial implementation of SORNA requirements (Harris & Lobanov-Rostovsky, 2016; McPherson, 2016; see also H. R. 4472—12). As of 2016, 17 states were judged by the SMART office to have substantially implemented the SORNA title

while the remaining 33 states had not yet substantially implemented SORNA (SMART, 2016). More specifically, the SMART Office evaluates state policies against 15 standards that comprise SORNA requirements. Harris and Lobanov-Rostovsky (2016) found that all states except Kansas deviate from at least 1 of the 15 SORNA standards and that “failure to include certain juveniles on sex offender registries per SORNA standards” was one of the most common impediments to states receiving the “substantially implemented” designation (p. 15). McPherson (2016) noted that states’ concerns about juvenile registration requirements remain among the most contentious. This is true even though 38 states register children adjudicated for certain sexual offenses as minors (The Pew Charitable Trusts, 2015).

While potentially vexing to federal lawmakers, the variation that exists between state policies affords the opportunity to identify whether *any* policies operate as intended, by reducing juvenile sexual crimes. To date, policy evaluations from New Jersey, South Carolina, Texas, and Wisconsin all fail to find any evidence that any of these states’ policies are associated with reduced sexual recidivism rates among people whose first sexual offense was committed as a child (Caldwell & Dickinson, 2009; Caldwell, Ziemke, & Vitacco, 2008; Letourneau & Armstrong, 2008; Letourneau, Bandyopadhyay, Sinha, & Armstrong, 2009a). Likewise, policy evaluations from two studies that evaluated the federal SORNA classification system *per se* found that it fails to accurately identify children at risk of recidivism and, therefore, is unlikely to reduce recidivism (Batastini, Hunt, Present-Koller, DeMatteo, 2011; Caldwell et al., 2008).

Prior to the current study, there was only one published study evaluating the effect of juvenile registration and notification policies on commission of first-time sexual crimes by children (i.e., people under age 18). Letourneau and colleagues (Letourneau et al., 2010) employed time series analyses to evaluate the impact of South Carolina’s juvenile registration and notification policy on the likelihood that a child would be charged with a first-time sexual offense. Their results indicated no significant changes in the likelihood of charges for first-time sexual offenses between pre- and post-implementation periods.

In addition to evaluating the effects of juvenile registration and notification policies on recidivism and first-time sexual crimes, studies have also evaluated unintended policy effects on juvenile case processing. Letourneau and colleagues found that juvenile registration was associated with unintended effects on juvenile case processing, including increased likelihood of diverting/dismissing juvenile sexual offense cases (Letourneau, Bandyopadhyay, Sinha, & Armstrong, 2009b) and dramatically increased likelihood of plea bargaining juvenile sexual offense charges to nonsexual charges (Letourneau, Armstrong, Bandyopadhyay, & Sinha, 2013). These patterns were not replicated with other juvenile violent offending, supporting the conclusion that these case processing changes occurred in response to registration policies.

Research has also begun to document harmful effects associated with juvenile registration and notification. For example, Harris and colleagues surveyed 265 therapists from across the U.S. who provide treatment services to youth who have sexually offended (Harris, Walfield, Shields, & Letourneau, 2016). Therapists overwhelmingly perceived negative consequences associated with juvenile registration requirements across the domains of child mental health, harassment and unfair treatment, school problems, and living instability. These investigators recently published a survey evaluation that compared registered and nonregistered children, all of whom were in treatment for sexually harmful and/or illegal behaviors (Letourneau, Harris, Shields, Walfield, Ruzicka, Buckman, Kahn, & Nair, in press). This survey included 256 children between the ages of 12 and 17, of whom 29% were subjected to some form of registration and/or notification requirements. Compared to nonregistered children, registered children were four times more likely to report having attempted suicide in the past 30 days and five times more likely to report having been approached by an adult for sex in the past year, among other harmful outcomes.

In sum, there are no published findings that support any positive effects of juvenile registration or notification, including any positive effects on community safety. Moreover, new studies have linked registration and notification policies with egregiously harmful outcomes to

children. The results that we present from our study, below, are consistent with these findings and lend support to efforts to replace juvenile sex offender registration and notification requirements with more effective prevention and treatment efforts.

Research Questions and Objectives

In general, the present study was designed to replicate findings from published studies on the effects of juvenile registration policies including several studies published by Letourneau (PI) and colleagues evaluating South Carolina's policy. More specifically, we had three principal goals for this study: to evaluate the general deterrent effects of juvenile registration policies on first-time sexual crimes (Goal 1), to evaluate the specific deterrent effects of these policies on sexual recidivism (Goal 2), and to evaluate the effects of these policies on judicial case processing of juvenile sexual offense cases (Goal 3). For each goal, we developed specific hypotheses. Table 1 presents each goal and each related hypothesis. We intended to utilize state-level juvenile and adult justice data from Maryland, Oklahoma, and Texas to replicate the South Carolina findings.

Goal 1: Hypotheses regarding general deterrence. Goal 1 was to examine whether juvenile registration exerts a general deterrent effect on first-time juvenile sex offending. To date, research has not supported a general deterrent effect for an offense-based juvenile registration policy (Letourneau, Bandyopadhyay et al., 2010). Moreover, legal policies are less likely to deter juveniles than adults due to differences in maturity, impulsivity, and awareness of legal consequences (Sampson & Cohen, 1988; Wikström, 2008). Thus, we hypothesized that state implementation of juvenile registration policies would have no effect on trends in first-time juvenile sex offense cases (Hypothesis 1). Additionally, we hypothesize that specific policy characteristics (which vary by state; e.g., duration of registration requirements) would not be associated with deterrence (Hypothesis 2).

Goal 2: Hypotheses regarding specific deterrence. Goal 2 was to examine whether juvenile registration exerts a specific deterrent effect on juvenile sexual or violent recidivism. To

date, research has not supported a specific deterrent effect for juvenile registration policies (Caldwell et al., 2008; Letourneau & Armstrong, 2008; Letourneau et al., 2009b). Research has indicated that registered youth are more likely to acquire new nonviolent offense charges (Letourneau & Armstrong, 2008; Letourneau et al., 2009b). Therefore, we hypothesized that juvenile registration policies would not influence juvenile sexual or violent recidivism (Hypothesis 3). Additionally, we hypothesize that specific policy characteristics would not influence sexual or violent recidivism (Hypothesis 4). We further hypothesize that youth registration status would be associated with increased risk of nonviolent charges (Hypothesis 5) and that more severe policy characteristics will further increase the risk for nonviolent charges (Hypothesis 6).

Goal 3: Hypotheses regarding judicial case processing. Goal 3 was to examine whether juvenile registration policies influence judicial case processing of juvenile sexual offense cases. To date, research has supported policy effects on case processing outcomes. Specifically, studies found that juvenile sexual offense cases were more likely to be dismissed or diverted following implementation of a juvenile registration policy (Letourneau et al., 2009a) and cases that were prosecuted were more likely to be pled to nonsexual offense charges following policy implementation (Letourneau, Armstrong, Bandyopadhyay, & Sinha, 2013). These effects were specifically attributed to the perceived harshness of the specific policy under study (i.e., South Carolina's policy requires lifetime registration and is based on the adjudication charge and not additional risk assessment). Thus, we hypothesized that Texas' policy, which is offense-based and thus might be perceived by judicial actors as unduly harsh will also be associated with increased likelihood of juvenile sex offense cases being dismissed, diverted or pled to nonsexual offenses (Hypothesis 7). Alternatively, we hypothesized that Maryland and Oklahoma's policies which are risk-based and might be perceived by judicial actors as fair will not be associated with changes in likelihood of these outcomes (Hypothesis 8).

Research Challenges

In subsequent sections, we detail our study methods, analytic strategies, and findings. However, it is important to acknowledge, at the outset, that our study suffered several unexpected set-backs that limited progress on addressing the three study goals within the study timeframe. As we describe in greater detail below, it took longer than anticipated to obtain data. Worse still, not one of the three original states that pledged to support this study provided all of the requisite data. Additionally, one of our original co-investigators, Dr. Mark Chaffin, died unexpectedly in August 2015. Apart from the abrupt loss of a dear friend, which was bad enough, his death impeded our ability to obtain *any* data from Oklahoma, one of the three original states. Lastly, our data analyst, Mr. Geoff Kahn, was accepted into a doctoral program (with our sincere congratulations) and had to step away from this project in July 2016. Despite immediately posting for the position, it took nearly a year to hire our new analyst, Dr. Reshmi Nair, who joined us in April 2017. Dr. Nair is a mathematician who has proven to be a remarkable analyst and we are moving much more quickly now that she has joined our team.

Because we encountered serious difficulties in obtaining the requisite data to support this project, we added a fourth state, Oregon to help address all study goals and we also obtained National Incident Based Reporting System (NIBRS) data to address one of the study goals. Of note, we were recently approved by the California Department of Justice to obtain data from that state and will do so in early 2018 to facilitate completion of all goals.

In the context of these limitations, we have not yet achieved several of our aims. We have most fully addressed Goal 1 and are currently focused on Goal 2, after which we will address Goal 3. This study officially ended in September 2017. However, our work will not end. We will continue to press states for promised data and to obtain relevant data from new states (e.g., California). Dr. Letourneau (PI) began this program of research evaluating juvenile registration policies in 2003 and she and her colleagues will continue pursuing the current study's goals until each has been fully addressed.

Study Methods and Analytic Techniques

In this section, we provide a general overview of data sources and analytic techniques and then provide specific information on the methods and analytic techniques for the two completed sub-studies, both of which assessed the general deterrent effects of juvenile registration policies. We refer to these as General Deterrence Study 1 and General Deterrence Study 2.

General Overview of Methods and Data Sources

To achieve Goals 1-3 required three essential types of information including (1) family/juvenile court sexual offense charges and adjudications for all cases in a given state encompassing several years before and several years following implementation of state registration policies (e.g., if a registration policy was enacted in 2000 then we required data from several years before and several years after 2000); (2) all prior and subsequent charges and adjudications of any offense type from family/juvenile and from adult (criminal) court for each youth with a juvenile sexual offense adjudication; and (3) the registration status of each youth with a juvenile sexual offense adjudication. For comparison analyses, we required similar information on children with nonsexual offense charges and adjudications.

All project procedures were approved by the PI's Institutional Review Board. Prior to conducting this project, we obtained letters of support from juvenile justice state agencies in Maryland, Oklahoma, and Texas. Initial data requests were made in early 2013, immediately after funding for this project was released. Despite the letters of support and numerous efforts, obtaining the requisite data proved remarkably difficult and still remains an ongoing process. Given these difficulties, we expanded our data collection efforts to obtain data from Oregon, and to utilize NIBRS data from Idaho, South Carolina, Utah, and Virginia. We describe the types of data we have (and have not) received from each state or source.

Maryland. In January 2014 the Maryland Department of Juvenile provided information on nearly 12,000 juvenile justice cases including all sexual offense charges and adjudications

(approximately 6,000 cases) and a random sample of nonsexual offense charges and adjudications (approximately 6,000 cases) that occurred between 2002 and 2014. Maryland enacted its juvenile registration policy in 2010. Maryland did not provide individual youth-level registration information (i.e., we do not know which of the children adjudicated for sex crimes were subjected to registration). Nor (despite numerous requests and a separate memorandum of understanding) have we received linked adult records, necessary for assessing recidivism. Thus, at present, the Maryland data can inform analyses only pertaining to policy effects on first-time sexual offenses (Goal 1; Hypotheses 1 & 2) and on judicial case processing (Goal 3, Hypotheses 7 & 8). We continue to press for the linked adult records.

Oklahoma. In June 2015, the Oklahoma Office of Management and Enterprise Services (OMES) Authority provided a document necessary to link juvenile and adult records for approximately 56,000 cases to Dr. Mark Chaffin, who had worked in Oklahoma for much of his career and had strong ties to this and other relevant state agencies. However, this linking document does not actually include any juvenile and criminal records (i.e., it only includes data to link such records). Dr. Chaffin made numerous requests for the data prior to his untimely death in 2016. We continued to reach out to the Oklahoma state agencies that originally promised the data but have not yet received any data from Oklahoma that can inform any analyses for any of the study's goals. We will continue to press OMES for the promised data.

Texas. In November 2015 the Texas Juvenile Justice Department provided data on approximately 49,000 juvenile justice cases including nearly 11,000 juvenile sexual offense cases, 30,000 juvenile assault cases, and nearly 8,000 juvenile robbery cases that occurred between 2005 and 2013. These included linked adult records but do not include any information on individual youth-level registration status. Unfortunately, Texas implemented its juvenile registration policy in 1999; thus, all of the data we obtained from Texas post-dates the Texas juvenile registration policy. Repeated attempts to obtain data from before 1999 and to obtain youth-level registration status have been to no avail. Consequently, the Texas data

cannot inform any analyses pertaining to any of the project goals. However, the Texas data do include cases before and after a substantial change to the Texas juvenile sex crime policy, one that exempts from prosecution teenagers engaging in consenting sexual relationships with peers (aka a “Romeo and Juliet” clause). We plan to evaluate the impact of that policy change on first-time juvenile sexual offenses.

Oregon. In February 2014 the Oregon Youth Authority provided information on more than 56,000 juvenile justice cases including all sexual offense charges and adjudications (approximately 13,000) and all assault and robbery cases (approximately 43,000) that occurred between 1991 and 2010. Oregon enacted its juvenile registration policy in 1995. All juvenile cases were linked to adult records. Oregon did not provide individual youth-level registration information (i.e., we do not know which of the children adjudicated for sexual crimes were subjected to registration). These data can inform analyses pertaining to policy effects on first-time sexual offenses (Goal 1; Hypotheses 1 & 2), sexual recidivism (Goal 2; Hypotheses 3-6) and on juvenile case processing (Goal 3; Hypotheses 7-9).

In addition to state-level data, we also obtained NIBRS data from four states on juvenile crime reports involving an alleged offender less than 18-years old. We used these data to evaluate Goal 1, Hypotheses 1 and 2, regarding juvenile registration policy effects on first-time sexual offenses committed by children under age 18 (Sandler et al., in press). While it is impossible to distinguish recidivism events from first-time sexual crimes within NIBRS data sets, research reveals that the vast majority of juvenile sexual crimes are committed by first-time offenders (Caldwell, 2016; Caldwell, 2007). Thus, even though the analyses included all sexual crime reports (first-time and recidivism events), the current study can be thought of as an analysis largely of initial or first-time sexual crime reports, bearing in mind that a small few will actually be recidivism offenses.

From the overall NIBRS data file, states were selected for analysis that had NIBRS-certified agencies¹ for at least an 8-year analytic period consisting of (a) at least four years of NIBRS data available prior to the enactment of a given state's juvenile registration policy and, (b) at least four years of NIBRS data available following the enactment of that policy. The four states that met these parameters were Idaho, South Carolina, Utah, and Virginia, giving the study reasonable variation in terms of state geography, size, population and juvenile registration policy characteristics. We included eight different types of crime data in the count of sexual crimes (e.g., forcible rape), seven in the count of nonsexual violent crimes (e.g., aggravated assault), 24 in the count of property crimes (e.g., shoplifting), and five in the count of drug and alcohol crimes (e.g., drunkenness).

Idaho. NIBRS data from Idaho included approximately 104,000 reports on juvenile crimes including approximately reports on nearly 2700 sexual offenses, 23,000 nonsexual violent offenses, 66,000 property offenses, and 13,000 drug offenses that occurred between 1992 and 2012. Idaho enacted its juvenile registration law in 1998. NIBRS data do not include youth-level registration information and not linked so identifying recidivism events is not possible. These data can inform analyses only pertaining to policy effects on first-time sexual offenses (Goal 1; Hypotheses 1 & 2).

South Carolina. NIBRS data from South Carolina included approximately 322,000 reports on juvenile crimes including nearly 9,000 sexual offenses, 131,000 nonsexual violent offenses, 150,000 property offenses, and 31,000 drug offenses that occurred between 1992 and 2012. South Carolina enacted its juvenile registration policy in 1995. NIBRS data do not include youth-level registration information and not linked so identifying recidivism events is not

¹ For more information about the NIBRS certification process, refer to: <https://www.fbi.gov/about-us/cjis/ucr/processes-and-procedures-of-nibrs-certification>

possible. These data can inform analyses only pertaining to policy effects on first-time sexual offenses (Goal 1; Hypotheses 1 & 2).

Utah. NIBRS data from Utah included approximately 118,000 reports on juvenile crimes, including approximately 4300 juvenile sexual offenses, 24,000 nonsexual violent offenses, 74,000 property offenses, and 16,500 drug offenses that occurred between 1993 and 2012. Utah enacted its juvenile registration policy in 2006. NIBRS data do not include youth-level registration information and not linked so identifying recidivism events is not possible. These data can inform analyses only pertaining to policy effects on first-time sexual offenses (Goal 1; Hypotheses 1 & 2).

Virginia. NIBRS data from Virginia included approximately 312,000 reports on juvenile crimes, including approximately 7,100 sexual offenses, 102,000 nonsexual violent offenses, 295,000 property offenses, and 35,000 drug offenses that occurred between 1994 and 2012. Virginia implemented its juvenile registration policy in 2005. NIBRS data do not include youth-level registration information and not linked so identifying recidivism events is not possible. These data can inform analyses only pertaining to policy effects on first-time sexual offenses (Goal 1; Hypotheses 1 & 2).

Within each of the four NIBRS study states, only those agencies that were certified NIBRS reporters at the start of their state's study period were included in the study, in order to obtain more stable data, lending itself well to time series analyses. Data from all qualifying agencies within each state were converted into monthly state averages for each type of crime report. These averages were per agency for each month. The decision to use per agency averages (as opposed to simply monthly counts) was an attempt to address missing data. Within the four states included in the current analysis, many of the agencies missed reporting data for at least one month. These missing monthly data points result in shocks to a monthly count series that can impede modeling. Using a series of per agency averages, however, reduces the impact of those shocks, making the series less susceptible to influence from

missing data. The unit of analysis for the study, therefore, was the average number of reports each agency in a given state received in a given month.

General Overview of Analytic Techniques

With the data we obtained thus far, we will be able to address all three study goals. We will not, however, be able to address all of the proposed hypotheses. Specifically, the available data do not permit comparing state policies with respect to their impact on recidivism. This is because as we only have a single state (Oregon) that provided sufficient recidivism data (i.e., linked juvenile and adult records that span pre- and post-policy implementation periods). Moreover, some hypotheses required individual youth-level registration status, which we did not obtain from any state. Thus, we cannot address Hypothesis 4, 5 or 6, which require pre- and post-policy implementation recidivism data from multiple states and/or individual youth-level registration status.

Analytic techniques for Goal 1: General deterrence effects. For this goal, we hypothesized that state's implementation of juvenile registration policies would not be associated with reduced first-time sexual crimes (Hypothesis 1) and that specific policy characteristics would not be associated with general deterrence (Hypothesis 2). In two separate papers (Letourneau et al., under review; Sandler et al., in press), we tested these hypotheses with a series of autoregressive integrated moving average (ARIMA) interrupted time-series models (McDowall, McCleary, Meidinger, & Hay, 1980). ARIMA analysis was selected for its ability to model the autocorrelation almost always found in time series data. ARIMA analyses were conducted separately for each state around that state's date of initial registration enactment. For each ARIMA analysis, autocorrelation in the series was identified through examination of the series' autocorrelation function (ACF) and partial autocorrelation function (PACF), after which the autocorrelation will be modeled and, therefore, removed from the analysis (Box & Jenkins, 1976). A dichotomous intervention variable coded "zero" for all months

prior to enactment of the state's registration law and coded "one" for all months following enactment of the registration law were entered into the models.

Analytic techniques for Goal 2: Specific deterrent effects. For this goal we hypothesized that state's implementation of juvenile registration policies would not influence juvenile sexual or nonsexual violent recidivism rates (Hypothesis 3). We are using survival analysis to evaluate policy effects on recidivism (Blosfeld, Hamerle, & Meyer, 1989; Kalbfleisch & Prentice, 2002). Survival analytic methods analyze the time to an event of interest, which can vary between cases, while incorporating and controlling for the effects of fixed explanatory variables. Analyses will separately model policy effects on new charges and new adjudications. Both juvenile and adult recidivism events are needed to contribute to the recidivism analyses, which we presently have only for Oregon. Preliminary results indicate that, as hypothesized, Oregon's juvenile registration policy is not associated with sexual or nonsexual violent recidivism (supporting Hypothesis 3). We are re-running these analyses prior to writing them up for publication. And we continue to press for adult records from Maryland to facilitate addressing the remaining hypotheses for Goal 2.

Analytic techniques for Goal 3: Judicial processing of juvenile sexual offense cases. To investigate whether policies exert effects on judicial case processing outcomes including case dismissal, diversion, and plea bargaining (Hypotheses 7 & 8), two analytical techniques will be used: ARIMA analysis, and generalized estimating equation (GEE) methods. Data from Oregon and Maryland will inform these analyses. As described earlier, ARIMA analysis models patterns in data over time and identifies significant effects of interventions, in this case the enactment of a juvenile sex offender registration law, by identifying pre- and post-intervention changes in the data series. ARIMA modeling will be used to examine the *rates* of three case processing outcomes: (a) those juveniles who were initially charged with sexual offenses proceeding to prosecution versus being dismissed or diverted, (b) those juveniles who were charged with sexual offenses plea bargaining to non-sex offenses versus retaining sexual

offense charges, and (c) those juveniles who retained sexual offense charges being adjudicated delinquent (guilty) of sexual offense charges versus being found not guilty.

An alternative strategy to investigate the effects of registration and notification policies on juvenile justice decision-making is to examine the *likelihood* that a particular decision gets made. GEE models, which are extensions of the general linear model, are able to incorporate repeated (i.e., correlated) observations by estimating and adjusting for the amount of correlation within the datasets (Ghisletta & Spini, 2004; Liang & Zeger, 1986). Separate GEE analyses will be conducted for each of the three case processing outcomes. Each analysis will include a dichotomous variable coded zero for all months prior to enactment and coded one for all months following enactment and the regression coefficient for this variable will determine whether enactment influenced the likelihood of a given outcome. As with the survival analyses, GEE models will control for possible influences of youth-level characteristics (age at initial arrest, race, prior criminal history) and if necessary, state-level characteristics. Comparison ARIMA and GEE analyses will examine judicial case processing outcomes for youth charged with non-sexual violent offenses.

Specific Methods and Analytic Strategies for General Deterrence Sub-studies

Methods: General Deterrence Study 1

In this first study (Sandler, Letourneau, Vandiver, Shields, & Chaffin, in press) we used autoregressive modeling to compare the monthly average of sexual offense reports lodged against children (i.e., under age 18) prior to and following juvenile registration policy implementation in each of four states. This is the only study, to our knowledge, that relies on reports rather than official charges or adjudications to assess juvenile registration policy impact. Reports are distinct from, and precede, formal charges and this is relevant because in other research PI Letourneau and colleagues have demonstrated that juvenile registration policies appear to influence charging and adjudication decisions (e.g., Letourneau, Bandyopadhyay, Sinha, D., & Armstrong, 2009b). By virtue of preceding formal law enforcement and prosecution

procedures, reports may provide a “cleaner” picture of the impact of juvenile registration on the primary prevention of adolescent sexual offending behaviors.

As described more fully next, we utilized time series analyses to evaluate the impact of four state juvenile registration policies, including those of Idaho, South Carolina, Utah, and Virginia, to test our hypothesis that registration policies would have no general deterrent effect (Hypothesis 1) and that specific characteristics of state registration policies (which vary widely) would not impact this result (Hypothesis 2).

The current study used publicly-available NIBRS data. NIBRS was established by the Federal Bureau of Investigation as an improvement to the Uniform Crime Reports (UCR), and NIBRS provides detailed reports of each criminal incident known to law enforcement (Addington, 2009). NIBRS data, therefore, have several analytic advantages over UCR data, as UCR data are only presented in aggregate, while NIBRS includes incident-level data (including information on the offender[s]) for each known crime. Furthermore, NIBRS includes data on a wide range of offenses, while UCR data are limited to a small number of specific offenses. This latter advantage is particularly important for analyses of sexual offending, as many sexual offenses in the UCR are aggregated into the “other sexual offense” category, with no level of detail available on the individual offenses and no way to separate out specific sexual offense types. NIBRS is also advantageous over UCR, as NIBRS includes all offenses associated with each incident, rather than implementing the hierarchy rule, which includes only the most serious offense.

Within each of the four study states, only those agencies that were certified NIBRS reporters at the start of their state’s study period were included in the study, in order to obtain more stable data, lending itself well to time series analyses (discussed more fully below). That is, including only those agencies that were certified NIBRS reporters at the start of the study period eliminated the possibility of one or more agencies contributing data to the series midway

through the study period, which could be mistaken for changes in the series as a result of JSORN enactment.

Finally, data from all qualifying agencies within each state were converted into monthly state averages for each type of crime report. These averages were per agency for each month. The decision to use per agency averages (as opposed to simply monthly counts) was an attempt to address missing data. Within the four states included in the current analysis, many of the agencies missed reporting data for at least one month. These missing monthly data points result in shocks to a monthly count series that can impede modeling. Using a series of per agency averages, however, reduces the impact of those shocks, making the series less susceptible to influence from missing data. The unit of analysis for the study, therefore, was the average number of reports each agency in a given state received in a given month.

Juveniles reported for sexual crimes. Eight different offenses were included in the count of juveniles reported for sexual crimes. These include forcible rape (NIBRS code 111), forcible sodomy (code 112), sexual assault with an object (code 113), forcible fondling (code 114), incest (code 361), statutory rape (code 362), human trafficking (but not commercial sexual acts; code 641), and peeping tom (code 908).

Juveniles reported for nonsexual crimes (for comparison). In addition to the series of juveniles reported for sexual crimes, three types of nonsexual juvenile crime reports were also modeled: (a) interpersonal (nonsexual), (b) property, and (c) drug and alcohol. As the enactment of JSORN laws should not have impacted any of these series, they were modeled for comparison and control. That is, there may have been other factors (e.g., increased media attention to juvenile-perpetrated crime) that occurred around the time of JSORN enactment and that influenced rates of reports for both sexual and nonsexual crimes. Modeling these nonsexual crime report series, therefore, contextualized the findings of the sexual crime report series analyses.

For example, if the results show a significant decrease in rates of juveniles reported for sexual crimes following the enactment of a JSORN law, while rates of juveniles reported for nonsexual crimes remained unchanged across the same time period, that combination of findings would lend support to the interpretation that the JSORN policy influenced the decline of juvenile sexual crime reports. Alternatively, if the results show a significant decrease in rates of juveniles reported for both sexual and nonsexual crimes following the enactment of a JSORN law, then other unmeasured factors might have contributed to the observed drops.

Interpersonal (nonsexual) reports. Seven different types of crime were included in the count of juveniles reported for interpersonal (nonsexual) crimes. These include murder/non-negligent manslaughter (NIBRS code 91), negligent manslaughter (code 92), kidnaping/abduction (code 100), aggravated assault (code 131), simple assault (code 132), intimidation (code 133), and human trafficking - involuntary servitude (code 642).

Property crime reports. Twenty-four different types of crime were included in the count of juveniles reported for property crimes, including robbery (NIBRS code 120), arson (code 200), extortion/blackmail (code 210), burglary/breaking and entering (code 220), pocket-picking (code 231), purse-snatching (code 232), shoplifting (code 233), theft from building (code 234), theft from coin-operated machine or device (code 235), theft from motor vehicle (code 236), theft of motor vehicle parts/accessories (code 237), all other larceny (code 238), motor vehicle theft (code 240), counterfeiting/forgery (code 250), false pretenses/swindle/confidence game (code 261), credit card/ATM fraud (code 262), impersonation (code 263), welfare fraud (code 264), wire fraud (code 265), embezzlement (code 270), stolen property offenses (code 280), destruction/damage/vandalism of property (code 290), bribery (code 510), and bad checks (code 901).

Drug and alcohol crime reports. Five different types of crime were included in the count of juveniles reported for drug and alcohol crimes, including drug/narcotic violations

(NIBRS code 351), drug equipment violations (code 352), DUI (code 904), drunkenness (code 905), and liquor law violations (code 907).

Analytic Strategy: General Deterrence Study 1

Univariate Box-Jenkins interrupted autoregressive integrated moving average (ARIMA) analyses were used to test the effect of JSORN enactment on all 16 crime report series (i.e., four crime report series per state). This analytic approach was selected due to its ability to model the autocorrelation commonly present in time series data (McDowall, McCleary, Meidinger, & Hay, 1980). That is, although the analysis in its basic form is a comparison of the monthly crime report series before the enactment of JSORN versus after the enactment of JSORN, simply using ordinary least squares regression (OLS) to conduct the comparison would be unreliable. Specifically, using OLS on autocorrelated data increases the likelihood that the analysis will return a false-positive result (i.e., find a significant change when there was in fact none; McDowall et al., 1980). ARIMA time series analyses are an improvement over OLS when analyzing time series data, therefore, as ARIMA models remove the influence of the autocorrelation from the analyses.

The Box-Jenkins approach to ARIMA analyses involves a three-step process: (a) identification, (b) estimation, and (c) diagnosis (Box & Jenkins, 1976). In the identification step, the autocorrelation process or processes (autoregressive, integrated, moving average, or some combination thereof) at work in the data are identified by examining the autocorrelation function (ACF; or correlogram) and partial autocorrelation function (PACF) for the series. Once the autocorrelation process(es) has been identified, the second analytic step, estimation, involves estimating a model in an attempt to remove the autocorrelation. Following this estimation, the possible presence of residual autocorrelation in the data is investigated in the third step, diagnosis, through examination of the ACF, PACF, and Ljung-Box Q-statistics for the estimated model. If there is no residual autocorrelation, the model is deemed to fit the data whereas

residual autocorrelation suggests the model is deemed to not fit the data, which requires the identification, estimation, and diagnosis steps are repeated.

Methods: General Deterrence Study 2

The purpose of this study was to determine whether the rates of children being charged or adjudicated for a first-time sexual offense declined following implementation of juvenile registration and notification policies relative to pre-policy periods. To increase confidence that any findings would pertain specifically to registration and notification policies (versus other juvenile or criminal justice policies), we conducted comparison analyses of physical assault and robbery charges and adjudications during the same time periods. The pattern of results across the primary and comparison analyses can aid in the interpretation of findings. For example, if results indicated a reduction in charges or adjudications associated with policy enactment (i.e., a general deterrent effect), that effect should be specific to the prevention of first-time sexual offenses and should not include first-time robbery or assault offenses that, while serious, do not typically trigger registration and notification requirements.

As described more fully below, we utilized time series analyses to evaluate the impact of two state juvenile registration policies, including those of Maryland and Oregon, to test our hypothesis that registration policies would have no general deterrent effect (Hypothesis 1) and that specific characteristics of state policies would not impact this result (Hypothesis 2).

Policy characteristics: Maryland. In Maryland, juvenile registration was implemented in 2010 and is applied to children adjudicated in juvenile court for first- or second-degree rape or first- or second-degree sexual assault (Cherry & Smallwood, 2010; MD Criminal Procedure Code § 11-704.1). Child and adult registrants are classified based on their crime of adjudication within one of the three tiers established in SORNA, with higher tier levels associated with those deemed most at risk to recidivate and therefore, have a greater registration burden (e.g., children in Tier III must update their information every three months compared to Tier II, which requires updated information every six months and Tier I which requires updated information

annually). Registration requirements terminate at the end of juvenile court jurisdiction over the child unless the court extends registration. Access to the juvenile registry is restricted to law enforcement, and children adjudicated in juvenile court are not subjected to broad online community notification. Data that informed analyses for this study were obtained from the Maryland Department of Juvenile Services, and include all juvenile justice cases involving sexual offense charges or adjudications from January 2002 to September 2013. We also obtained a random sample of 10% of all cases involving non-sexual offense charges or convictions during the same period, from which we extracted physical assault and robbery cases for use in comparison analyses.

Policy characteristics: Oregon. In 1995, Oregon expanded registration and notification to children adjudicated delinquent for sexual offenses (Oregon State Police, n.d.).² As with adult registrants, children were subjected to lifetime registration. Some children, however, were permitted to petition for removal from the registry depending on certain case features (Pittman & Nguyen, 2011). Data that informed analyses in the present study were obtained from the Oregon Youth Authority and included all juvenile sexual offense charge or adjudication cases from January 1991 to December 2010. Oregon also provided data on all juvenile physical assault and robbery charges and adjudications during this time period for comparison analyses. Of note, in 2015 Oregon substantially amended its juvenile registration laws such that children adjudicated for sexual offenses as minors undergo a separate hearing to determine whether they will be required to register (McFarlane & McKechnie, 2016). This change does not affect the current study because our data extend only through 2010.

² The sexual offenses in Oregon include the following: contributing to the sexual delinquency of a minor; encouraging child sex abuse 1, 2, and 3; harassment touch intimate part; incest; indecent exposure; luring a minor; possess child sex material 1 and 2; possession of materials depicting sexually explicit conduct of a child; private indecency; and public indecency. All of the crimes include related “attempt to commit” and “conspiracy to commit” charges. Prostitution was excluded from all analyses.

All data from both states were de-identified and assigned random identification numbers prior to being obtained by the investigative team for analysis. These numbers permitted us to identify a child's first charge or first adjudication for a given type of offense within the time frame of the records retrieval (i.e., 2002-2013 for Maryland and 1991-2010 for Oregon). The first author's university institutional review board approved this study.

Samples. We obtained limited information on the children whose records informed this study, including age at charge (adjudication), gender, and race. We did not have access to information on other child characteristics (e.g., ethnicity) or on family or victim characteristics. (We present the demographic data by offense type and by state under Findings, below). We restricted analyses to cases involving youthful defendants ages 8 to 18 years. Average ages ranged between 14 and 16 years, and tended to be younger for children charged or adjudicated for sexual offenses than children charged or adjudicated for physical assault or robbery offenses (we did not control for these or other differences, as each analysis was run separately by state and by offense type). Most children whose cases informed analyses were boys, particularly the children charged or convicted of sexual offenses. The majority of children was Black in the Maryland dataset and White in the Oregon dataset, findings that are consistent with adult arrest data for each state (Oregon Uniform Crime Reporting, 2016; Williams, 2016). In both state samples, most children had incurred a single offense type (i.e., sexual, assault, or robbery) and thus contributed just one charge case and/or one adjudication case to the analyses. However, we retained data from children with more than one offense type (e.g., a physical assault charge and a robbery charge) because analyses were conducted separately by offense type.

Analytic Strategy: General Deterrence Study 2

The analytic strategy was designed to assess whether the trend in the number of first-time sexual offense charges or adjudications changed after juvenile registration and notification policies were implemented, relative to pre-policy periods. For analytic purposes, we designated as "intervention dates" January 2010 and January 1995, the initial months of policy

implementation for Maryland and Oregon, respectively. For analyses involving charges, the first sexual offense charge was identified and retained, regardless of whether or not the charge resulted in adjudication. Similarly, for the adjudication analyses, the first sexual offense adjudication was identified and retained, regardless of any prior charges. In cases where multiple charges or adjudications stemmed from a single incident, only the most serious charge or adjudication was retained. First-time charges or first-time adjudications were then counted and aggregated by month. This same process was followed for physical assault and robbery offense charges and adjudications.

We used interrupted auto-regressive integrated moving average (ARIMA) models to evaluate whether trends in the monthly number of first-time charges or first-time adjudications for sexual offenses in Maryland or Oregon changed following the implementation of juvenile registration policy implementation (again, identified as intervention dates of January 2010 and January 1995, respectively). ARIMA is a well-established analytical tool for assessing the effects of legislative action (see generally: Chamlin & Krajewski, 2016; Stockwell et al., 2017). ARIMA models intrinsically account for the effects of maturation (i.e., naturally-occurring changes that unfold over time); therefore, multivariable adjustments are generally not necessary, and the approach is relatively robust (Hamilton, 1994; McCleary & Hay, 1980; McDowell, McCleary, Meidinger, & Hay 1980). ARIMA models are also advantageous over other analytical tools that simply compare pre- and post-intervention scores in that ARIMA models do not assume an abrupt change, but consider various alternative patterns, such as a gradual effect that levels off or various other possible patterns.

ARIMA models may, however, be susceptible to temporal confounding by discreet events that occur in close proximity to the intervention of interest (e.g., a change in juvenile justice practice that occurred during the same year as registration policy implementation). To partially address this limitation, we also modeled physical assault and robbery offense data in comparison analyses. Thus, for example, if we identified declines in the trends of first-time

sexual *and* nonsexual offense charges during a post-policy period, relative to a pre-policy period, this would suggest that other factors besides (or in addition to) registration were influential.

In the ARIMA analysis, we first identified an adequate white noise model. To develop the white noise model, auto-correlation (i.e., systematic variations that occur over time) and stationarity (i.e., consistency) were assessed using the Ljung-Box chi-square statistic and the Augmented Dickey-Fuller test, respectively. After a white noise model was attained, a dichotomous intervention variable was introduced that identified whether the month in which the first-time charge (adjudication) occurred was prior to or after the policy intervention date. Statistical analyses were performed using SAS 9.4 (SAS Institute Inc., 2013).

Findings

In this section, we provide a brief review of study results for each of our three overarching goals. We then provide detailed results for our two completed studies that focus on general deterrence.

As previously noted, we have made the most progress on addressing Goal 1, Hypotheses 1 and 2, with two completed studies. We have begun conducting recidivism analyses with the Oregon data, which will permit addressing Goal 2, Hypothesis 3 and 5, and we continue to press for the promised adult recidivism records from Maryland which would permit addressing Goal 2, Hypothesis 4. We have sufficient data to begin addressing Goal 3, Hypotheses 7 & 8.

Findings for Goal 1: General Deterrence Effects

As noted, we have completed two studies evaluating juvenile registration policy effects on general deterrence of first-time sexual offenses by children under age 18. Briefly, and as described more fully in the next section, results from both studies indicated no significant changes in the likelihood of reports for first-time sexual offenses or of charges or adjudications for first-time sexual offenses between pre- and post-implementation periods for any of the six

states whose data informed these two studies. This supports our hypothesis that registration policies would have no general deterrent effect (Hypothesis 1). We further hypothesized that specific characteristics of state registration policies (which vary widely) would not impact this result (Hypothesis 2). Because the effect of each state's policy was nonsignificant, we could not formally test this hypothesis. However, Hypothesis 2 is indirectly supported by the fact that none of the six states' policies influenced first-time sexual offenses by adolescents, despite wide variations among those policies.

Findings for Goal 2: Specific Deterrent Effects

To date, only Oregon has provided the requisite data to address Goal 2. We have completed a preliminary analysis of the effect of Oregon's juvenile registration policy on sexual and nonsexual violent recidivism using survival analyses. We are presently replicating the analyses and will provide them in an update once they have been completed.

Findings for Goal 3: Judicial processing of juvenile sexual offense cases.

We have not yet conducted the analyses needed to evaluate this goal. The data we have in-hand from Maryland and Oregon will inform planned analyses addressing this goal and, specifically, Hypotheses 7 and 8.

Findings for General Deterrence Studies 1 and 2

Findings: General Deterrence Study 1

Recall that our first study evaluating juvenile registration policy effects on general deterrence of adolescent sexual offending (Sandler et al., in press) was informed by NIBRS data on reports of adolescent sexual offenses from four states. Table 2 presents the number of qualifying agencies and the number of months of data modeled for each state. We did not obtain descriptive information on the youth whose NIBRS cases informed this study, apart from the fact that they were identified in the NIBRS data as being under age 18 at the time of the report. We utilized ARIMA analyses to assess whether juvenile registration policy

implementation was associated in reductions of reports for adolescent sexual offenses. We also conducted comparison analyses on reports of non-sexual offenses.

The first main column of Table 3 presents the monthly mean number of reports for juvenile sexual, nonsexual interpersonal, property, and drug-related reports. Of the four states included in the study, South Carolina averaged the highest monthly number of juveniles reported for these crimes with 1,371.6 ($SD = 352.4$), followed by Virginia (1,062.8, $SD = 619.7$), Utah (249.7, $SD = 85.0$), and Idaho (237.5, $SD = 64.4$). Virginia averaged the highest monthly number of juveniles reported for these crimes with 1,755.8 ($SD = 892.2$), followed by South Carolina (1,564.4, $SD = 357.1$), Utah (769.3, $SD = 198.3$), and Idaho (684.2, $SD = 245.9$). Virginia also averaged the highest monthly number of juveniles reported for these crimes with 365.1 ($SD = 191.8$), followed by South Carolina (329.7, $SD = 112.8$), Utah (172.5, $SD = 62.3$), and Idaho (141.1, $SD = 44.8$).

The second main column of Table 3 presents the final ARIMA models for each of the series modeled. As can be seen, 14 of the 16 series for which modeling was attempted resulted in models free from autocorrelation that were stable and, therefore, interpretable. The two series for which no stable, autocorrelation-free model could be obtained were the interpersonal (nonsexual) crime report series for South Carolina and the interpersonal (nonsexual) crime report series for Utah. As such, it was not possible to test and interpret the impact of JSORN enactment on the number of juveniles reported for interpersonal (nonsexual) crimes in either South Carolina or Utah. It was possible, however, to test and interpret the impact of JSORN enactment on the other 14 crime report series.

Findings: Juveniles reported for sexual crimes. As can be seen in Table 3 of the four states included in the study, South Carolina averaged the highest monthly number of juveniles reported for these types of crime with 92.7 ($SD = 19.0$), followed by Virginia (73.8, $SD = 39.7$),

Utah (45.8, $SD = 17.2$), and Idaho (28.3, $SD = 8.2$).³ As can be seen in the first row for each state in Table 3, when the intervention variable (marking the enactment of JSORN policies) for each state was added to the respective model for each state, none of the coefficients for the variables (which show the impact of JSORN enactment) reached statistical significance (all p 's ≥ 0.18). Thus, in none of the study states did the enactment of JSORN policy significantly impact the per agency monthly average of juveniles reported for sexual crimes.

Findings: Juveniles reported for nonsexual crimes. As can be seen in Table 3, and as expected, JSORN enactment did not significantly impact the monthly number of juveniles reported per agency in any of the four study states for any of the three types of nonsexual crime (all p 's ≥ 0.07). Thus, when combined with the results above, it does not appear that the enactment of a JSORN law impacted the monthly per agency average of juveniles reported for any of the crime types that were modeled.

Findings: General Deterrence Study 2

Descriptive information on study cases. Recall that our second study evaluating juvenile registration policy effects on general deterrence of adolescent sexual offending (Letourneau, Shields, Nair, Kahn, Sandler, & Vandiver, manuscript under review) was informed by state juvenile justice data from Maryland and Oregon. We obtained limited information on the children whose records informed this study, including age at charge (adjudication), gender, and race. We did not have access to information on other child characteristics (e.g., ethnicity) or on family or victim characteristics. We present the demographic data by offense type and by state in Table 4. We restricted analyses to cases involving youthful defendants ages 8 to 18 years. Average ages ranged between 14 and 16 years, and tended to be younger for children

³ Virginia and South Carolina have much larger youth populations than Utah and Idaho (see http://www.ojdp.gov/ojstatbb/ezapop/asp/comparison_selection.asp) and thus it is not surprising that, for juvenile sexual crimes and nonsexual crimes, the highest monthly averages are always for Virginia and South Carolina and the lowest are for Utah and Idaho.

charged or adjudicated for sexual offenses than children charged or adjudicated for physical assault or robbery offenses (we did not control for these or other differences, as each analysis was run separately by state and by offense type). Most children whose cases informed analyses were boys, particularly the children charged or convicted of sexual offenses. The majority of children was Black in the Maryland dataset and White in the Oregon dataset, findings that are consistent with adult arrest data for each state (Oregon Uniform Crime Reporting, 2016; Williams, 2016). In both state samples, most children had incurred a single offense type (i.e., sexual, assault, or robbery) and thus contributed just one charge case and/or one adjudication case to the analyses. However, we retained data from children with more than one offense type (e.g., a physical assault charge and a robbery charge) because analyses were conducted separately by offense type.

Again, recall that we utilized autoregressive modeling to compare the monthly average of first-time sexual offense charges and the monthly average of first-time sexual offense adjudications across pre- and post-policy implementation years for Maryland and for Oregon. The average monthly count for sexual, physical assault, and robbery offense charges and adjudications are provided for Maryland and Oregon in Tables 5 and 6, respectively. Results of the ARIMA analyses, including model structure and coefficients for the policy variable, are shown in Table 7. Results are presented separately by state, by outcome (charge or adjudication), and by offense type (sexual, assault, or robbery), for a total of 12 analyses. A stationary white noise model was attained for each analysis except for the analysis of Oregon first-time sexual offense charges. In this case, and as described more fully below, results from two models that approached white noise and were the best possible fit to the data are presented in Table 7.

Findings for sexual offense charges: Maryland. Using data from Maryland, we analyzed 5,657 first-time sexual offense charges over 141 months. Of these, 4,385 charges occurred in the 96-month pre-policy period and 1,272 in the 45-month post-policy period. As

depicted in Figure 1, declines in the raw number of juvenile sexual offense charges predated implementation of Maryland's juvenile registration policy. Implementation of the Maryland juvenile registration policy in 2010 was not associated with a significant change in trends of the monthly number of first-time sexual offense charges (Table 7).

Findings for sexual offense adjudications: Maryland. We analyzed 1,631 first-time sexual offense adjudications of which 1,279 adjudications occurred in the pre-policy period and 352 in the post-policy period. As depicted in Figure 1, declines in the raw number of juvenile sexual offense adjudications predate implementation of Maryland's juvenile registration policy. Again, implementation of Maryland's juvenile registration policy was not associated with a significant change in trends of the monthly number of first-time sexual offense adjudications (Table 7).

Comparison analyses: Maryland. Comparison analyses indicated that trends in the monthly number of first-time assault or robbery charges or adjudications did not change significantly between the pre- and post-policy periods (Table 7).

Findings for sexual offense charges: Oregon. Using data from Oregon, we analyzed 13,279 first-time sexual offense charges over 240 months of which 2,598 occurred in the 48-month pre-policy period and 10,681 in the 192-month post-policy period. As depicted in Figure 2, the raw number of juvenile sexual offense charges increased somewhat after policy implementation before beginning to decline in 2002. Implementation of the Oregon juvenile registration policy in 1995 was not associated with a significant change in trends of the monthly number of first sexual offense charges in either of the models tested (Table 7). Again, neither model met the formal definition of white noise so some correlation between months remained (autocorrelation at lag 6 could not be removed); nevertheless, the policy variable was nonsignificant in both models.

Findings for sexual offense adjudications: Oregon. We also analyzed 5,451 first-time sexual offense adjudications, of which 1,036 occurred in the pre-policy period and 4,415 in the

post-policy period. As depicted in Figure 2, the raw number of juvenile sexual offense adjudications increased somewhat following policy implementation, before beginning to decline in 2004. Implementation of the Oregon juvenile registration policy in 1995 was not associated with a significant change in trends of the monthly number of first-time sexual offense adjudications (Table 7).

Findings for comparison analyses: Oregon. Comparison analyses indicated that the monthly number of first-time assault or robbery charges and adjudications did not change significantly between pre- and post-policy periods (Table 7).

Summary for Findings from General Deterrence Studies 1 and 2

The results from both of these general deterrence studies are consistent with the only other study that, to our knowledge, also evaluated the impact of a state juvenile registration policy on general deterrence of first time sexual crimes by children (Letourneau et al., 2010). In combination, these studies evaluate the juvenile registration policies of six states on three outcomes: reports, charges, and adjudications pertaining to first-time adolescent sexual offenses. None of the six juvenile registration policies exerted a general deterrent effect on any of these important outcome indicators. This supports Hypothesis 1. Furthermore, collectively these findings support Hypothesis 2. That specific policy characteristics do not account for the overall failure of juvenile registration and notification policies to improve community safety via deterring first-time sexual offenses among children. Recommendations include replacing juvenile registration policies with more effective prevention and intervention practices.

Conclusions

The results of our two studies that evaluated the effects of juvenile registration policies on general deterrence failed to find any evidence of such an effect. Our analyses evaluated policies from six different states on three different outcomes, including reported sexual offenses, first-time sexual offense charges, and first-time sexual offense adjudications among children under age 18. None of the findings indicated that rates of these outcomes were significantly

different during the post-policy implementation period, relative to pre-policy periods. Our results replicate findings from a study that evaluated South Carolina's juvenile registration and notification policy effects on first-time charges and adjudications (Letourneau et al., 2010). Based on the strength of the available evidence, we conclude that juvenile registration and notification policies fail to improve community safety through a general deterrent effect on youth sexual offending. These findings are also commensurate with the broader literature, finding no evidence that JSORN policies significantly improve public safety in any measurable way (Batastini et al., 2011; Caldwell & Dickinson, 2009; Caldwell et al., 2008; Letourneau & Armstrong, 2008; Letourneau et al., 2009a). To date, the only identified effects of juvenile registration policies are unintended effects on juvenile case processing, including increases in diversions, dismissals, and plea bargains of juvenile sexual offense cases (Letourneau et al., 2013; Letourneau et al., 2009b; Letourneau et al., 2009a) and remarkably harmful effects on children, such as increased risk for attempted suicide and for being approached by adults for sex, among other negative outcomes (Chaffin, 2008; Harris, Walfield, Shields, & Letourneau, 2016; Human Rights Watch, 2013).

Discussion

We believe the two studies that we have completed substantively address the question of whether juvenile registration policies improve community safety via a general deterrent effect on the harmful and/or illegal sexual behavior of children. They do not. We acknowledge that, as with all studies, ours involved some limitations. First, both studies relied upon data from official sources (i.e., NIBRS and juvenile justice charges and adjudications). Therefore, only crimes reported to law enforcement were included in the analyses. This is a limitation, as many sexual offenses go unreported to authorities (Maxfield, Weiler, & Widom, 2000). In an attempt to minimize the impact of this limitation, the current study modeled juvenile sexual crime reports and charges regardless of final outcomes (i.e., we did not limit the first two outcomes to those cases ultimately adjudicated delinquent for sexual crimes). A limitation of our first study

(Sandler et al., in press), which relied upon NIBRS data is that the data series for the four states did not include all law enforcement agencies within each state, as the process for reporting to NIBRS is still voluntary and developing (Addington, 2009). Restricting the analyses to only those agencies that were reporting throughout the entire study period, however, resulted in much more stable series and more easily-interpretable results. Also with respect to our first study, it was impossible to separate first-time sexual offense reports in NIBRS from sexual recidivism reports. This limitation is somewhat mitigated by research demonstrating that the vast majority of sexual crime reports (particularly juvenile sexual crime reports) are of first-time offenders. A limitation of our second study (Letourneau et al., manuscript under review) is that we were unable to fit a white noise model to the Oregon first-time sexual offense charge data. The resulting autocorrelation between the monthly charge counts detracts from confidence in the results of this one outcome. However, the similarity of findings for both charges and adjudications across both states, and in alignment with previously reported findings from other states increases confidences in the validity and in the generalizability of those findings.

Implications for Policy, Practice, and Future Research

State juvenile registration and notification policies vary along a number of dimensions, including the specific offenses that trigger registration, the duration and frequency of registration, and the degree to which a child's status as a registered sex offender is released to the public (Pittman & Nguyen, 2011). State policies also vary regarding the extent with which they comply with federal juvenile registration standards (SMART, 2016). Despite these and other variations, findings from the present studies and prior research are entirely consistent: there is no evidence that subjecting children to sex offender registration and notification procedures deters other children from engaging in harmful or illegal sexual behavior. Moreover, the consistency of findings across six states with different juvenile registration and notification systems (Idaho, Maryland, Oregon, South Carolina, Utah, and Virginia) and three different outcomes (sexual offense reports, charges, and adjudications) suggests that the failure of

juvenile registration policies to deter first-time sexual crimes is robust to policy variations. This robustness of findings supports two additional conclusions. First, that these findings will generalize to the remaining state policies that have not yet been evaluated. And, second, that merely revising juvenile registration and notification policies will not result in different outcomes, given the consistency of results across a wide variety of systems, locations, and demographic populations.

As noted, we have not yet completed analyzes evaluating policy effects on juvenile sexual recidivism. However, as noted earlier, all available research on that issue finds that juvenile registration and notification policies also fail to deter sexual or violent recidivism. While we will continue to address the goals of this project and analyze Oregon data (and, we hope, data from Maryland and California, if we obtain the promised adult recidivism records), the available evidence suggests that juvenile registration policies do nothing to improve community safety. Moreover, these policies have been associated, in previous research, with unintended consequences on juvenile case processing and with harmful effects on the children who are subjected to registration and/or notification requirements. As such, **we recommend that juvenile registration and notification policies be replaced with more effective approaches to the prevention of juvenile sexual offending.** For example, empirically rigorous research has shown that children's harmful and illegal sexual behaviors can be effectively prevented via evidence-based universal prevention programs including Shifting Boundaries (Taylor, Stein, Mumford, & Woods, 2013) and Safe Dates (Foshee et al., 2005), via evidence-based treatment models including Multisystemic Therapy for Problem Sexual Behaviors (Dopp, Borduin, Rothman, & Letourneau, 2016; Letourneau, Henggeler et al., 2009) and via evidence based approaches that include parent-focused behavioral skills training for addressing younger child problem sexual behavior (Carpentier, Silovsky, & Chaffin, 2010; St. Amand, Bard, & Silovsky, 2008). Replacing ineffective and wasteful juvenile registration and

notification policies with effective evidence-based practices would achieve the important goal of preventing child sexual abuse.

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Table 1. Project goals and hypotheses.

Goals	Hypotheses
Goal 1. Evaluate juvenile registration policy effects on general deterrence of juvenile sexual offenses	<ol style="list-style-type: none"> 1. Juvenile registration policies will have no impact on first-time sexual offenses committed by children under age 18. 2. Specific policy characteristics (e.g., whether registration is lifetime or for a briefer duration) will have no impact on first-time sexual offenses committed by children.
Goal 2. Evaluate juvenile registration policy effects on specific deterrence of juvenile sexual offenses	<ol style="list-style-type: none"> 3. Registration policies will have no effect on sexual or violent recidivism 4. Specific policy characteristics (e.g., whether registration is lifetime or for a briefer duration) will have no impact on sexual or violent reoffending. 5. Registration policies will be associated with increases in nonviolent charges against registered children. 6. Harsher policy characteristics will be associated with increases in nonviolent charges against registered children.
Goal 3. Evaluate juvenile registration policy effects on processing of juvenile sexual offenses cases.	<ol style="list-style-type: none"> 7. Offense-based policies (i.e., that do not consider risk) will be associated with increases in dismissal/diversion and plea bargains of juvenile sexual offense cases. 8. Risk-based policies will not be associated with changes in dismissal/diversion/plea bargains of juvenile sexual offense cases.

Table 2. General Deterrence Study 1: Characteristics of states included in the study.

State	Month of JSOR Enactment	Month of NIBRS Certification Cut Off ^a	Number of Qualifying Agencies (% of Total) ^b	Months of NIBRS Data Modeled
Idaho	July 1998	July 1993	110 (86.6%)	252
South Carolina	January 1995	January 1991	324 (63.8%)	252
Utah	July 2006	July 2001	65 (69.9%)	240
Virginia	July 2005	July 2000	389 (92.0%)	228

^aThe year by which an agency needed to be listed as NIBRS certified for the agency to be included in the analyses.

^bSource: FBI, 2012.

Table 3. *General Deterrence Study 1: Findings from ARIMA analyses.*

	Juveniles Reported Monthly Mean (SD)	ARIMA Model	JSORN Coefficient	<i>p</i>
Idaho				
Sexual	28.3 (8.2)	(0,1,1)(0,1,1) ₁₂	-0.02	0.78
Nonsexual personal	237.5 (64.4)	(0,1,1)(0,1,1) ₁₂	-0.36	0.14
Property	684.2 (245.9)	(0,1,1)(0,1,1) ₁₂	-0.36	0.69
Drug	141.1 (44.8)	(0,1,1)(0,1,1) ₁₂	-0.33	0.08
South Carolina				
Sexual	92.7 (19.0)	(0,1,1)(1,0,0) ₁₂	-0.03	0.42
Nonsexual personal	1,371.6 (352.4)	Could not be cleanly modeled	--	--
Property	1,564.4 (357.1)	(0,1,1)(1,0,1) ₁₂	-0.13	0.07
Drug	329.7 (112.8)	(0,1,1)(0,1,1) ₁₂	-0.16	0.35
Utah				
Sexual	45.8 (17.2)	(0,1,1)(0,1,1) ₁₂	-0.12	0.18
Nonsexual personal	249.7 (85.0)	Could not be cleanly modeled	--	--
Property	769.3 (198.3)	(0,1,1)(0,1,1) ₁₂	0.40	0.94
Drug	172.5 (62.3)	(0,1,1)(0,1,1) ₁₂	-0.06	0.92
Virginia				
Sexual	73.8 (39.7)	(0,1,1)(1,0,0) ₁₂	0.04	0.30
Nonsexual personal	1,062.8 (619.7)	(0,1,1)(1,0,1) ₁₂	-0.11	0.76
Property	1,755.8 (892.2)	(0,1,1)(1,0,1) ₁₂	-0.08	0.90
Drug	365.1 (191.8)	(0,1,1)(1,0,0) ₁₂	0.01	0.94

Table 4. General Deterrence Study 2: Characteristics of Maryland and Oregon samples of children charged or adjudicated as minors of sexual, assault, or robbery crimes.

Characteristic	All Crimes		Sexual Crimes		Assault Crimes		Robbery Crimes	
	Charges	Adjudicated	Charges	Adjudicated	Charges	Adjudicated	Charges	Adjudicated
Number								
Maryland	10,960	2,671	5,657	1,631	3,451	617	2,241	445
Oregon	56,391	24,427	13,279	5,451	42,728	17,761	4,571	2,168
% Male								
Maryland	86.6	92.4	94.4	97.2	70.6	78.3	92.8	94.4
Oregon	74.1	77.4	91.8	95.8	69.1	71.5	84.7	84.9
Age, Mean (SD)								
Maryland	14.9 (2.0)	14.9 (1.8)	14.3 (2.1)	14.4 (1.8)	15.2 (1.7)	15.5 (1.5)	15.8 (1.4)	15.9 (1.2)
Oregon	14.8 (2.1)	14.7 (1.7)	14.2 (2.1)	14.3 (1.8)	14.5 (2.0)	14.7 (1.7)	15.2 (1.7)	15.3 (1.6)
% White								
Maryland	29.5	31.3	39.0	41.4	26.7	22.5	8.3	5.8
Oregon	71.8	71.3	76.2	78.1	71.0	70.1	58.3	61.4
% Black								
Maryland	65.5	62.6	55.2	51.3	69.1	72.6	88.0	91.0
Oregon	7.8	6.5	6.0	4.0	8.0	6.4	20.4	15.7
% Other race								
Maryland	5.0	6.1	5.8	7.3	4.3	4.9	3.7	3.2
Oregon	20.4	22.3	17.8	18.0	20.9	23.5	21.4	22.9
% Single offense type								
Maryland	96.6	99.2	95.5	99.2	91.1	97.1	91.0	97.1
Oregon	92.9	96.1	81.5	91.2	91.0	94.9	59.0	76.9

Table 5. General Deterrence Study 2: Average monthly counts per year of first charge and adjudication by offense type in Maryland.

Pre-Policy Years						
Year	Sexual Crimes		Assault Crimes		Robbery Crimes	
	Charge	Adjudication	Charge	Adjudication	Charge	Adjudication
2002	57	19	27	4	12	2
2003	52	15	32	6	13	2
2004	51	14	27	4	12	2
2005	44	12	31	5	15	4
2006	46	14	27	4	21	3
2007	44	13	27	5	21	3
2008	36	10	26	6	22	4
2009	37	10	24	4	19	4
Post-Policy Years						
Year	Sexual Crimes		Assault Crimes		Robbery Crimes	
	Charge	Adjudication	Charge	Adjudication	Charge	Adjudication
2010	30	9	23	5	16	4
2011	33	10	20	4	13	3
2012	29	8	13	4	14	3
2013 ^a	20 ^b	3 ^b	14 ^b	3 ^b	12 ^b	3 ^b

a. 2013 data obtained only through September.

b. Average monthly count based on data through September.

Table 6. General Deterrence Study 2: average monthly counts per year of first charge and adjudication by offense type in Oregon.

Pre-Policy Years						
Year	Sexual Crimes		Assault Crimes		Robbery Crimes	
	Charge	Adjudication	Charge	Adjudication	Charge	Adjudication
1991	46	19	96	26	13	4
1992	56	22	119	35	18	7
1993	58	23	169	52	20	9
1994	56	23	207	62	25	10
Post-Policy Years						
Year	Sexual Crimes		Assault Crimes		Robbery Crimes	
	Charge	Adjudication	Charge	Adjudication	Charge	Adjudication
1995	54	21	230	79	30	12
1996	57	23	247	83	27	12
1997	60	25	234	79	24	12
1998	63	27	227	86	22	12
1999	61	25	209	95	20	11
2000	59	25	202	94	20	12
2001	67	27	190	86	16	8
2002	59	26	175	84	16	9
2003	62	27	177	86	16	7
2004	59	26	165	80	19	10
2005	57	24	167	82	15	7
2006	48	20	176	89	17	7
2007	49	19	161	81	19	10
2008	50	20	150	75	16	9
2009	44	16	133	66	15	8
2010	42	15	127	62	14	7

Table 7. General Deterrence Study 2: ARIMA model results for the effect of juvenile registration and notification policy enactment.

Maryland Results			
Outcome	Model Structure	Policy Variable Coefficient	p-value for Policy Variable
Sex offense charge	(0,0,0)x(0,1,1) ₁₂ +c	-0.76	0.773
Sex offense adjudication	(0,1,1)	-0.48	0.835
Assault charge	(0,1,1)x(0,1,1) ₁₂	-0.01	0.997
Assault adjudication	(1,0,1)+c	-0.53	0.348
Robbery charge	(0,1,1)	-3.43	0.226
Robbery adjudication	(1,0,0)+c	0.08	0.854
Oregon Results			
Outcome	Model Structure	Policy Variable Coefficient	p-value for Policy Variable
Sex offense charge ^a	(0,1,1)x(0,1,1) ₁₂ +c	-6.46	0.200
Sex offense charge ^a	(0,1,1)x(1,0,0) ₁₂	-1.62	0.708
Sex offense adjudication	(0,1,1)x(1,0,0) ₁₂	-1.04	0.653
Assault charge	(0,1,1)x(0,1,1) ₁₂	8.34	0.520
Assault adjudication	(0,1,1)x(1,0,1) ₁₂	11.76	0.056
Robbery charge	(1,0,1)+c	4.73	0.165
Robbery adjudication	(1,1,1)	3.30	0.064

a. Sexual offense charges were modeled twice in Oregon, because a white noise model was not obtained. Instead, the two best fitting models are both included for comparison.

Figure 1. General Deterrence Study 2: Juvenile sexual offense charges and adjudications in Maryland, January 2002-September 2013.

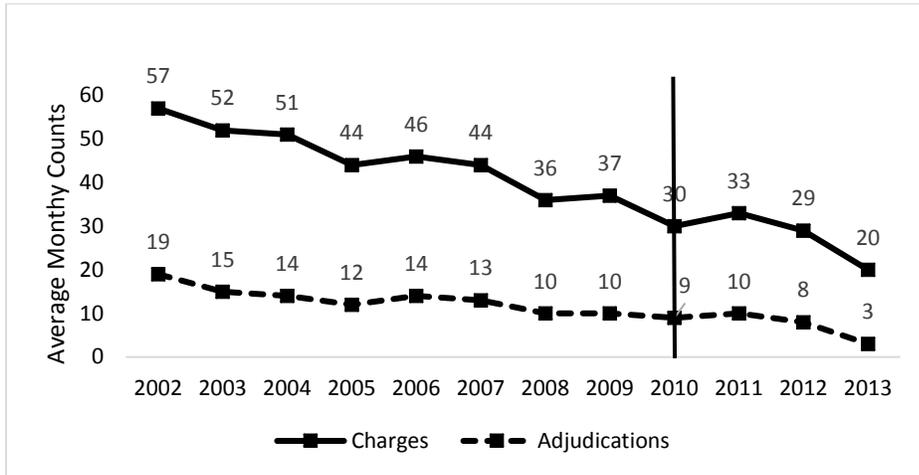


Figure 2. General Deterrence Study 2: Juvenile sexual offense charges and adjudications in Oregon, January 1991-December 2010.

