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THE UNIVERSITY AT ALBANY

SCHOOL OF CRIMINAL JUSTICE

THE DISSERTATION SUBMITTED BY

Kelly M. Socia

UNDER THE TITLE

Residence Restriction Legislation, Sex Crime Rates, and the Spatial Distribution of Sex Offender Residences

has been read by the undersigned. It is hereby recommended for acceptance to the faculty of the University in partial fulfillment of the requirements for the degree of Doctor of Philosophy.

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RESIDENCE RESTRICTION LEGISLATION, SEX CRIME RATES, AND THE SPATIAL DISTRIBUTION OF SEX OFFENDER RESIDENCES

by

Kelly M. Socia

A Dissertation

Submitted to the University at Albany, State University of New York

in Partial Fulfillment of

the Requirements for the Degree of

Doctor of Philosophy

School of Criminal Justice

2011

This project was supported by Award No. 2010-IJ-CX-0004, awarded by the National Institute of Justice, Office of Justice Programs, U.S. Department of Justice. Further, data for the New York State Sex Offender Registry was supplied by the New York State Division of Criminal Justice Services. The opinions, findings, and conclusions or recommendations expressed in this publication are those of the author and do not necessarily reflect those of either the Department of Justice or the New York State Division of Criminal Justice Services.

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TABLE OF CONTENTS

ACKNOWLEDGEMENTS

Firstly, I acknowledge and thank my committee members (Drs. Levenson, Lizotte, Messner, Pogarsky, and Stamatel), for their insightful comments, questions, and feedback as this study grew from a brief concept paper into a full dissertation. Without the support of each of them, I would not have been able to complete the dissertation and finally become a doctor (though not the kind that helps people).[1] I would like to thank Dr. Lizotte in particular for agreeing to chair my committee, for providing frank and excellent guidance on getting through the dissertation process, and for his encouragement along the way. Perhaps most especially, I would like to thank Dr. Stamatel for agreeing to mentor me years ago when I was a smart-ass first year student who wanted to study a topic that no faculty members were really interested in. I appreciated the time she took to mentor me, her 'tough love' approach (sometimes), the 'academics don't have a sense of humor' talk she gave me when I had THE BEST idea for a paper title, and her friendship.

I would be remiss if I didn't specifically acknowledge and thank Drs. Apel and McDowall for their statistical and methodological advice over the years. I know that I'm a much better quantitative researcher because of my interactions with each of them. Without their sage advice on a wide-range of topics, my dissertation would likely consist wholly of cross-tab tables and stick-figure drawings.

I would like to thank rest of the faculty and staff of the School of Criminal Justice. The support they've provided me over the years has made my life much, much easier. As such, I formally issue a blanket apology for bugging each of you about various things related to classes, events, and funding (especially to Arlene deGonzague in relation

[1] Well, technically I guess I could have lost the support of *one* committee member and still been okay, since I only needed 4 out of 5 'yes' votes to pass. But that's beside the point, and is moot anyway.

to grant funding issues and Alyssa Worden for various off-the-record questions). I would also like to thank Kathleen Maguire for hiring me on the Sourcebook project and for being an awesome boss, and the other Sourcebook members for being great colleagues. The support Sourcebook provided me was pivotal to finishing my dissertation, and its family atmosphere made life much more enjoyable than it otherwise would have been.

I am pleased to acknowledge and thank the National Institute of Justice for their financial support of my dissertation research. I would also like to acknowledge the Division of Criminal Justice Services, particularly Dave van Alstyne, for the various datasets that they were kind enough to provide for my project. Additionally, the New York State Office of Sex Offender Management was kind enough to help facilitate the acquisition of the (publically available, totally-should-be-subject-to-freedom-of-information-laws-but-it-is-not) New York Sex Offender Registry database from DCJS, so thank you to them.

Drs. Naomi Freeman and Jeffrey Sandler were kind enough to include me on early projects involving the efficacy and consequences of sex offender legislation. Because of them, I was able to springboard into the topic of sex offender legislation and reentry research. They have been great colleagues and friends over the last five years.

A special thank you to William Goodman, who helped critique my personal statement to SUNY Albany. Before his help, the statement essentially read "Let me into grad school, because I want to save my wife from dangerous people!" After Will's help, the statement was obviously much better, since SUNY Albany was somehow tricked into accepting me.[2] As Will said shortly thereafter, it was like he was literally carrying me into grad school on his back. Therefore, I'm appreciative for that help and for his

[2] However, in rereading what I actually submitted to SUNY Albany, it's still pretty horrible.

friendship (and apparently for his ridiculously strong back muscles). I am also grateful to Dr. Jack Cook, Dr. Samuel C. McQuade III, and the late Dr. Thomas Castellano for writing letters of recommendation to help get me into the SUNY Albany program and for encouraging my interest in criminal justice while at Rochester Institute of Technology.

I must acknowledge the Men's Academic Pursuit (MAP) group for their camaraderie and for many great times over the past few years.[3] You're friendship has helped sustain my pursuit of the dissertation, and your slow-but-steady financial support (one poker game at a time) has helped me to afford the 'good' beer that individuals of stature and sophistication drink. Thank you also to my many other friends in (and associated with) the School of Criminal Justice, including Lisa Sacco who was kind enough to share her apartment with my wife, myself, and our three cats.[4] I'll hopefully someday be in a position to repay everyone's friendship and kindness with amazing employment and/or co-authorship opportunities (though I wouldn't count on it).

Dr. Matthew Pate has been one of my closest friends during my graduate career, and I am thankful to him and his family for the kindness, encouragement, and friendship they've provided me throughout this process. It is because of him (and Kiyonaga Sensei), that I am theoretically able to defend myself from an onslaught of up to four samurai, assuming the proper equipment is worn by all and they attack with staggered timing as practiced in the kata. There is no one else I'd depend on more in the event of a zombie apocalypse than Dr. Matthew Pate and his extensive firearm skills (though Ráchael Powers and Chris Dum are close seconds).

[3] The other regular members of the MAP group include Christopher Dum, Dennis Gabriels, Robert Norris, Matthew Phillips, Jeremy Shifton, and Andrew Wheeler.

[4] And no, Lisa, you cannot keep Kismit when we move out, no matter how much you think he loves you squeezing the bejesus out of him with your 'Lenny' hugs.

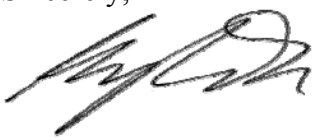
My family has supported me (emotionally, financially, and otherwise) longer than anyone else in my life, and instilled in me the value of education, and so I'm especially grateful to my mother, Kim Socia, my father, Kelly Socia (Sr.), and my sister, Sarah Socia, for their love and support throughout the years. I also thank them for hiding from me the disappointment they likely felt when I told them I was going to be a professor and not one of the people they've seen on the CSI TV shows.

Most importantly, I thank my wife, Kristen Socia, for too many things to list here. However, a small sample of such things would include: Not killing me when I told her I wanted to leave my awesome and well-paid job at the Pentagon to live on a hobo salary for five years while getting my Ph.D., listening to my friends and I talk, incessantly, about issues related to classes, school, and research, being the best wife I've ever had (population n = 1), and for loving me unconditionally.[5] Finally, thank you to Kristen's parents, Diane and Heidi, for also supporting me in my pursuit of this degree, and for not killing me when I told them we were moving to New Mexico after graduation.

There still remains a large list of individuals that I've not been able to thank here due to considerations involving space and carpal tunnel syndrome. These individuals have been very helpful (and kind) to me over the years, and I'm grateful for their mentoring, friendship, and encouragement. I'm sure each of you knows who you are.

In closing, thank you all for being a very significant part of my life.[6]

Sincerely,



Kelly M. Socia

[5] Or, at least loving me to the extent where I've yet to come across one of those 'conditions,' if they exist.
[6] $p < .05$ (two tailed)

ABSTRACT

Residence restrictions are one of the most recent, and most controversial, public policies seeking to protect community members from registered sex offenders (RSOs) reentering society following incarceration. Residence restriction policies prohibit RSOs from living within a given distance of certain places where children might gather (e.g., schools, daycares, parks, and playgrounds). In doing so, the expectation is that RSOs will have a harder time finding and approaching young children whom they can sexually assault, thus driving sexual recidivism rates down. These policies, first passed in 1995 at the state level and in 2005 at the county and local level, have become extremely popular throughout the United States, but without proof that they are effective. To date, the research on these policies has been extremely limited, and has largely focused on the unintended consequences that these policies cause for RSOs, typically as a result of reduced housing options.

This study addresses this lack of research by examining four issues: 1) the characteristics of counties passing these policies, 2) the efficacy of county residence restrictions to reduce sex crime rates in New York State, 3) whether these policies are associated with the spatial distribution (i.e., clustering or dispersion) of RSO residences in upstate New York neighborhoods, and 4) whether this spatial distribution is in turn associated with differences in county-level recidivistic sex crime rates. In doing so, this study draws on a number of diverse literatures, including the diffusion of policy innovations, incapacitation and deterrence theories, reentry and rehabilitation research, and the conceptualization and measurement of the spatial distribution of ex-offender residences.

Results indicate that political competition is very influential in passing a county residence restriction and that a nearby residence restriction may dissuade others from passing their own policies. Further, while these restrictions do not reduce recidivistic sex crimes, they may generally deter some individuals who are not yet RSOs from sexually victimizing adults. Finally, results indicate that while a residence restriction is in some cases associated with the within and between-neighborhood spatial distribution of RSOs, there is no indirect effect on recidivistic sex crime rates.

CHAPTER 1: INTRODUCTION

Each year over a half-million individuals are released from incarceration into communities throughout the United States (Anderson-Facile, 2009; T. Hughes & Wilson, 2003). Many of these ex-offenders will eventually return to jail or prison after committing new crimes or violating the conditions of parole. A number of criminal justice policies have been implemented in an attempt to influence the reentry of these ex-offenders. Some of these policies are rehabilitative in nature, such as provisions funding drug and alcohol treatment programs, occupational and academic training, and housing and employment assistance for ex-offenders. Other reentry policies are meant to protect the public from these ex-offenders' future crimes, typically through incapacitative or deterrence measures such as electronic monitoring, loitering restrictions, mandatory curfews, random drug testing, and periodic contact with parole officers.

In addition to policies that apply to the general population of ex-offenders, a number of reentry policies have recently been applied specifically to individuals convicted of sex offenses. These policies include mandatory public registration, community notification, temporary or lifetime electronic monitoring, chemical castration, and residence restrictions (see Bonnar-Kidd, 2010; Button, DeMichele, & Payne, 2009; Cohen & Jeglic, 2007; Center for Sex Offender Management (CSOM), 2007; 2008; Farkas & Stichman, 2002; Finn, 1997; Fitzgerald, 1990; Flack, 2005; IACP, 2008; Levenson, 2009; Levenson & D'Amora, 2007; Matson & Lieb, 1997; Socia & Stamatel, 2010; Towers, 2007; Wetterling & Wright, 2009; Winick, 1998).[7] Meant more for public

[7] While civil commitment may be considered another type of recent sex offender specific incapacitative policy, as it provides for the extended removal of certain sex offenders from the community and placement into a secured treatment facility, it results in long term incapacitation outside of the community (similar to a three-strikes provision), rather than incapacitating these sex offenders *within* the community.

protection rather than rehabilitation, these policies attempt to reduce sex crime rates through either incapacitative measures (e.g., electronic monitoring, chemical castration, residence restrictions), or specific deterrence measures (e.g., public registration, community notification). Some of these policies, such as community notification, have also been found to be a potential source of general deterrence for sex offenses (see CSOM, 2008; Prescott & Rockoff, 2008).

Residence Restriction Policies

Residence restrictions are one of the most recent, and most controversial, of these public policies seeking to protect community members from registered sex offenders (RSOs). Residence restriction policies prohibit RSOs from living within a given distance of certain places where children might gather (e.g., schools, daycares, parks, and playgrounds).[8] In doing so, the expectation is that RSOs will have a harder time finding and approaching young children they could sexually assault, thus driving sexual recidivism rates (and the rates of recidivistic sex crimes) down. As such, these policies attempt to incapacitate RSOs (via residential limitations and/or relocation) from interacting with children at public places during their daily routines.[9]

First passed in 1995 at the state level and in 2005 at the county and local level, residence restriction have become extremely popular throughout the United States, but without proof that they are effective.[10] In fact, only a single study has explored the types of jurisdictions passing these policies at the state level (e.g., Meloy, Miller, & Curtis,

[8] These are sometimes referred to collectively as 'child congregation locations.'
[9] This also assumes that the 'suitable targets' of RSOs are children who gather around public locations such as schools and daycares. While this assumption does not appear to be supported by existing literature (e.g. Duwe, Donnay, & Tewksbury, 2008; Zandbergen, Levenson, & Hart, 2010), contradicting either this assumption or the use of routine activities theory to justify these policies is not the focus of the current study (see Tewksbury, Mustaine, & Stengel, 2008; J. T. Walker, Golden, & VanHouton, 2001).
[10] For the purposes of this study, "local-level" refers only to sub-county jurisdictions such as towns and cities.

2008), and none has explored their passage at either the county or local level. Thus, it is unclear what types of jurisdictions have already passed these restrictions and what types of jurisdictions are most likely to pass these restrictions in the future. Without this early exploratory research, the passage of residence restrictions as an incapacitative criminal justice policy cannot be analyzed or compared to broader, more established literatures, such as those detailing the diffusion of policy innovations (see Rogers, 2003).

Perhaps more concerning is that only a few studies have analyzed whether residence restrictions have (or would have) reduced recidivistic sex crime rates as intended (e.g., Blood, Watson, & Stageberg, 2008; Duwe, et al., 2008; Minnesota Department of Corrections (MNDOC), 2007; see also Zandbergen, et al., 2010). These studies find little support that these policies are effective at preventing the types of sex crimes targeted by the restrictions, but have largely limited their analyses to sex crimes committed by previously convicted RSOs against child victims. As such, no research has explored whether the incapacitative relocation inherent in these policies has affected sex crimes committed by RSOs against adult victims, and even the literature regarding RSOs and child victims is still in its early stages.

Further, no research has specifically explored whether residence restriction policies are effective at deterring sex crimes committed by individuals who are *not* directly subject to these policies (i.e., exploring the general deterrence of non-recidivistic sex crimes). This is an important consideration for the evidence-based support of these policies, particularly if they do not show any incapacitative effects (see more generally Pawson, 2002, 2006). Additionally, because of the numerous unintended consequences that these policies generate for RSOs returning to or already living in the community (see

Bonnar-Kidd, 2010; Levenson, 2009; Soule & Earl, 2001), a general deterrence effect seems plausible.

One such unintended consequence involves the spatial implications residence restrictions can cause for RSOs seeking housing when returning to or relocating within a community. Relatively more research has explored the spatial implications of residence restrictions as they relate to theoretical or actual RSO residences (e.g., Barnes, Dukes, Tewksbury, & De Troye, 2009; Berenson & Appelbaum, in press; Chajewski & Mercado, 2009; Grubesic, Mack, & Murray, 2007; Grubesic & Murray, 2008; Grubesic, Murray, & Mack, 2008; L. A. Hughes & Burchfield, 2008; Levenson, 2008; Levenson & Cotter, 2005; Mulford, Wilson, & Parmley, 2009; Red-Bird, 2009; Socia, in press; J. T. Walker, et al., 2001; Youstin & Nobles, 2009; Zandbergen & Hart, 2006, 2009a, 2009b; Zgoba, Levenson, & McKee, 2009). This research generally finds that residence restrictions may force RSOs to seek housing in rural areas (see Berenson & Appelbaum, in press; Casady, 2009; Morgan, 2008; Socia, in press; Youstin & Nobles, 2009; Zandbergen & Hart, 2006, 2009a, 2009b), which may hinder their ability to find available and affordable housing (Socia, in press).

However, the majority of these studies were based on theoretical residence restrictions and housing options and/or did not measure whether existing restrictions had led RSOs to actually cluster into a limited number of neighborhoods offering unrestricted housing (Levenson & Cotter, 2005; Socia, in press; but see Berenson & Appelbaum, in press; Morgan, 2008), or alternatively whether RSOs were forced out of the most restricted areas and became dispersed evenly among the remaining (less restricted) neighborhoods. Whether RSOs are actually 'clustering' in certain neighborhoods can

have important consequences for residents living in jurisdictions that have passed residence restrictions, both in terms of increased fear and (potentially) increased risks of sexual assault. Residents in spatially proximate jurisdictions may also experience these consequences, particularly if the number of RSO residences increase in these neighborhoods as RSOs are kept out (or forced out) of other nearby jurisdictions due to a residence restriction policy.

Finally, research on the relationship between RSO housing, spatial clustering, and sex crime rates is still in its early stages (see Duwe, et al., 2008; MNDOC, 2007; Zandbergen, et al., 2010). If residence restrictions *are* affecting the spatial distribution of RSOs, as either increased clustering or increased dispersion, the next step would be to examine whether these spatial distributions are related to recidivistic sex crime rates. If this is the case, then residence restriction policies may be indirectly affecting recidivistic sex crime rates *in addition* to any direct effects resulting from incapacitation.

Overall, further research on the passage and consequences of residence restriction policies could provide important contributions to a growing field that draws on elements of the diffusion of policy innovations, research on the efficacy of criminal justice policies, incapacitation and deterrence theories, and spatial criminology. Further, it would provide more evidence as to whether these policies are empirically justified, and the extent that their unintended consequences are affecting sex crime rates.

Types of Sex Crimes

When considering how a residence restriction might directly or indirectly affect sex crime rates via incapacitation, general deterrence, or some other mechanism, it is important to acknowledge that not all sex crimes are equivalent. In fact, different types

of sex crimes may be affected differently by a single residence restriction policy. In order to understand the effects of a residence restriction, these different types of sex crimes must be identified and explained.

Sex crimes may be roughly separated into four types. The first type involves those crimes committed against child victims by current RSOs (i.e., those with prior convictions for sex crimes). The second type involves sex crimes committed against adult victims also by current RSOs. These first two types can be considered recidivistic sex crimes, as they involve the sexual recidivism of RSOs. The third type involves sex crimes committed against child victims by individuals *without* any prior sex crime convictions (i.e., individuals who are not RSOs). The fourth type involves sex crimes committed against adult victims by individuals without prior sex crime convictions. The latter two types can be considered non-recidivistic sex crimes. As such, the term 'sex crime' is a multidimensional concept, and has important implications for how residence restrictions could affect each type of sex crime.

As noted earlier, residence restrictions are meant to directly reduce a very specific instance of the first type of sex crime involving RSOs and child victims. Specifically, residence restrictions are meant to reduce sex crimes committed by RSOs who are living near child congregation locations and who indentify potential targets (i.e., children) at these same public locations. This goal is presumed to be achieved through a form of incapacitation. That is, by relocating the residences of RSOs away from areas where children congregate, it is expected that these offenders will no longer be able to access pools of potential victims as easily (see Farkas & Stichman, 2002). Fortunately, these types of sex crimes are extremely rare (Duwe, et al., 2008; MNDOC, 2007).

Unfortunately, even if effective at relocating RSOs away from child congregation locations, the rarity of these sex crimes may mean that residence restrictions may not be affecting the overall rate of recidivistic sex crimes committed against children.

However, residence restrictions may be affecting recidivistic sex crimes in ways that *do not* involve RSOs' proximity to child congregation locations. For instance, recidivistic sex crimes committed against either children or adults may be reduced if these policies result in RSOs living in less populated areas (see Chajewski & Mercado, 2009; Socia, in press; Youstin & Nobles, 2009; Zandbergen & Hart, 2006; Zgoba, et al., 2009), as this may still reduce the ease in which RSOs become acquainted with and/or otherwise gain access to future victims.[11] This is particularly important, as the majority of sex offenses are committed by acquaintances or family members of the victims (Greenfield, 1997; Snyder, 2000).

Alternatively, residence restrictions may actually *increase* recidivistic sex crimes if relocating RSOs to certain neighborhoods limits their ability to access stable housing, suitable employment, supportive family members, and/or adequate treatment facilities (see Barnes, et al., 2009; Casady, 2009; CSOM, 2007, 2008; Levenson, 2008; Levenson & Cotter, 2005; MNDOC, 2003; Zandbergen & Hart, 2006). This is in part due to the reduction in stability and increase in financial and emotional stress these RSOs might experience (Levenson, 2008; Levenson & Cotter, 2005; MNDOC, 2003; Willis & Grace, 2008), which can ultimately hamper successful reentry and rehabilitation. This seems

[11] This assumes in part that offenders with prior sexual convictions involving adult victims are also subject to the residence restriction. In some instances, residence restrictions apply only to those individuals with prior sexual convictions involving child victims, only to the highest risk sex offenders, and/or only to individuals still subject to probation or parole monitoring. At the state level, most residence restriction policies apply to RSOs regardless of the age of their victim (Meloy, et al., 2008). For the purposes of this study, it is assumed that all RSOs whose information is available on a public registry (which typically excludes low risk and juvenile offenders) are subject to a residence restriction policy applying to their jurisdiction.

particularly likely if residence restrictions force RSOs to become clustered in only a few neighborhoods that offer limited housing, rather than dispersing them throughout many other (less urban) neighborhoods (see Berenson & Appelbaum, in press; Socia, in press).

Finally, it is possible that a residence restriction simply has no direct or indirect effect on the sex crimes committed by RSOs, other than displacing these individuals (and potentially their future crimes) from one neighborhood to another. If this were the case, a countywide residence restriction policy would be unlikely to affect the recidivistic sex crime rate unless the policy resulted in displacing many RSOs outside of the county.

In any event, despite the different ways that residence restrictions might ultimately affect recidivistic sex crimes, little is known about whether or how this may be the case. As such, hypothesizing that the policy is *not* affecting recidivistic sex crime rates (i.e., testing a null hypothesis of no effect) seems most appropriate when studying this issue, at least until research evidence indicates otherwise. This course of action seems particularly appropriate given the current lack of research supporting these policies (see Harris & Lurigio, 2010; Pawson, 2002, 2006; Socia & Stamatel, 2010; Tewksbury & Levenson, 2007).

Residence restrictions may also deter sex crimes committed by individuals without prior sex crime convictions (and therefore who are not directly subject to these policies). Specifically, the *threat* of being subject to a residence restriction policy, and the unintended consequences that they can cause for RSOs (see Bagley, 2008; Levenson, 2008, 2009; Levenson & Hern, 2007; Levenson & Tewksbury, 2009; Tewksbury, 2007; J. T. Walker, 2007), may be enough to deter individuals from committing their first sex

crime.[12] If this is the case, then the passage of a residence restriction should decrease the rate of non-recidivistic sex crimes, regardless of the policy's effect on recidivistic sex crimes. However, since there is no research supporting this hypothesis, it again is more prudent to hypothesize that these policies *do not* have a deterrence effect on non-recidivistic sex crimes, and thus test a null hypothesis of no effect.

In any event, the distinction in the types of sex crimes and the policy mechanisms that may increase or decrease these sex crimes are important considerations when researching these issues. This study acknowledges this distinction in its exploration of the relationships between residence restriction policies and recidivistic and non-recidivistic sex crime rates, and between the spatial distribution of RSO residences and recidivistic sex crime rates.

Research Questions

To explore these relationships, this study considers four main research questions: 1) What are the county characteristics that are associated with passing county-level residence restriction policies? 2) Have these policies affected rates of county recidivistic and non-recidivistic sex crimes committed against either child or adult victims? 3) Are residence restriction policies associated with the spatial distribution (i.e., clustering or dispersion) of RSOs in certain neighborhoods (i.e., census block groups)? 4) Is the spatial distribution of RSO residences in turn associated with countywide recidivistic sex crime rates? These relationships are shown in FIGURE 1.

Note that in this study, 'recidivistic sex crimes' are defined as the number of sex crimes committed by individuals with prior convictions for sexual offenses (i.e., RSOs),

[12] While this seems most plausible for sex crimes committed against child victims, there may also be general deterrence effects for sex crimes committed against adult victims, especially since these crimes may also subject convicted individuals to residence restriction policies (see Meloy, et al., 2008).

while 'recidivistic sex crime rates' are simply the rate of such crimes per a given number of residents (e.g., crimes per 100,000 residents). This is conceptually different from recidivism counts or rates, which involve the number or rate of individuals with prior convictions who commit additional sex crimes, rather than the number or rate of the occurrence of such crimes. While an ideal measure might be the recidivism rate of the actual offenders living within a given area, these historical data are not available, and thus the rate of recidivistic sex crimes for a given population of residents is expected to provide a reasonable proxy measure of sex crimes.

To answer these research questions, this study examines the passage, efficacy, and consequences of sex offender residence restrictions at the county level in the state of New York. New York provides a particularly useful setting for the study since it does not yet have an 'official' statewide residence restriction policy that could influence county-level results (unlike most other states in the U.S.), it contains counties with a wide variation of demographic, social, geographic, and crime characteristics, and it has a mixture of counties (and local-level jurisdictions) with and without residence restriction policies currently in place.[13]

Focusing at the county level is important for a number of reasons, including the ability of a county to be influenced by the actions of either lower-level (i.e., local) or higher-level (i.e., state or federal) governments, because counties are run by politicians and political entities that can be influenced by local political pressures (or at least influenced perhaps more than state and federal politicians), and because counties can

[13] However, under state law, sex offenders who are still on probation or parole may be subject to a restriction that specifies such offenders cannot enter *public* places within 1,000 feet from a school, though it is unclear the extent to which this restriction is actually enforced or that this constitutes an actual residence restriction policy in terms of *private* residences.

contain much diversity in demographic, social, political, and crime measures, even within the same state. Further, jurisdictions with fewer agencies involved in the policy process (e.g., a county government compared to a state government) may be better able to successfully implement responsive policies (Gerston, 1997; Lineberry, 1977), and may be more influenced by the actions of nearby jurisdictions (i.e., other counties), both of which can help explain how hundreds of counties and local-level jurisdictions have been able to pass residence restrictions in only a few years time. These reasons reinforce that policy research at the county level, especially as it relates to sex offender residence restrictions, is both sorely needed and conceptually interesting.

For the first research question, county-level demographic, crime, and political characteristics for all 62 counties in New York are used to identify the characteristics that are associated with the passage of residence restriction legislation. Specifically, a multivariate logistic regression model is used to identify the county characteristics that are associated with the likelihood of a county passing a residence restriction policy between November 2005 and December 2009.[14]

For the second research question, residence restriction legislation indicators are used to predict monthly sex crime rates for each of the 62 counties in New York for the time period between January 1998 and December 2009.[15] Fixed-effects panel models are used to analyze whether the presence of a county-level residence restriction influenced any of the four different types of sex crime rates, controlling for other demographic and social indicators.

[14] November 2005 was the month in which the first residence restriction was passed by a county or local-jurisdiction in New York, and December 2009 was the last month for which crime data were collected.
[15] Specifically, each of the four types of sex crime rates is examined in separate models.

For the third research question, neighborhood-level demographic and crime rate characteristics, RSO addresses, and indicators of county and local-level residence restriction legislation are used to examine the spatial distribution of RSO residences in neighborhoods in the upstate New York area.[16] Specifically, a multivariate OLS regression model is used to determine whether the presence of a county and/or local-level residence restriction is associated with the spatial distribution (i.e., clustering or dispersion) of RSO residences within or between neighborhoods, controlling for other neighborhood characteristics relevant to RSO housing options.

The fourth research question incorporates county-level data from all three previous research questions, including measures of RSO clustering and county-level demographic, and crime characteristics for counties in the upstate New York region, in examining whether the within-county spatial distribution of RSO residences is associated with the rate of recidivistic sex crimes committed against either child or adult victims.[17] Because the dependent variable is not normally distributed, a multivariate Poisson regression model is used to evaluate the relationship between county-level spatial patterns of RSO residences and two types of mean monthly recidivistic sex crime rates in 2009.

Implications for Research and Policy

As noted earlier, the results of this study can help link the fields of sex offender reentry and recidivism research with the larger fields of criminal justice policy passage

[16] The upstate New York area was specifically selected so that the results of the analyses are more applicable to the 'average' neighborhood found throughout most of the state, rather than being overly influenced by the vast number of extremely dense neighborhoods contained within the counties in and around New York City (i.e., the New York Metropolitan Statistical Area). Since the first two research questions focus at the county level, these questions include all counties in New York, since each county has an equal weight in terms of its influence on the final results.

[17] Specifically, each of the two types of recidivistic sex crimes (i.e., those committed against child victims and those committed against adult victims) is analyzed separately.

and diffusion, incapacitation and/or deterrence research, and spatial criminology. Further, the results of this study can help to influence future evidence-based policy decisions regarding the passage of sex offender residence restrictions. For example, the results of the first research question can help researchers determine if the jurisdictional characteristics that influence the passage of residence restriction policies are similar to those that influence the passage of other criminal justice policies, including proximity to other jurisdictions passing their own policies.

Additionally, knowing the characteristics associated with the implementation of residence restrictions can help to more efficiently target research and policy interventions in those counties most likely to otherwise adopt residence restrictions as a way to address perceived 'problems' with RSO residences. Using these results, these interventions could be conducted *before* a county feels pressure to implement a residence restriction policy, which could potentially be subject to legal challenges and result in high legal costs for the county.[18] This would be particularly relevant given the anecdotal evidence that these laws lead to a 'domino effect' of successive legislation in nearby jurisdictions (e.g., Levenson, 2009; Yung, 2007; Zgoba, et al., 2009).

This study also incorporates a body of literature that has largely excluded county-level analyses. Specifically, this study incorporates the (mostly) state-level findings of the diffusion of policy innovation literature to determine its applicability to county-level crime policies. This literature has been underutilized in the study of sex offender policies generally, and residence restriction policies specifically.

[18] Just a few of these court cases include Doe v. Miller (2005), G.H. v. Township of Galloway (2008), Mann v. Georgia DOC (2007), People v. Blair (2009), People v. Oberlander (2009), and Wright v. Iowa DOC (2008).

Perhaps the most important implication of this study concerns the examination of the efficacy of existing residence restriction policies to reduce sex crime rates. Providing policymakers with evidence that these policies either do or do not work as intended can help promote evidence-based policy decisions in the future, potentially reduce the difficulties that RSOs experience when attempting to reenter a community after their incarceration, and help promote policies that successfully protect residents. For example, if it is found that residence restrictions do not reduce any type of sex crime, it may encourage counties to stop implementing such policies or replace existing ones with other policies proven to either be more effective or have fewer unintended consequences for successful reentry. Alternatively, if this study finds that residence restrictions *do* reduce certain types of sex crimes, it would provide policymakers with the research evidence needed to continue the implementation of these policies to successfully protect the public. In either event, these research findings could also be used to help courts determine whether residence restrictions hold intrinsic merit for crime reduction and community safety, or whether they are largely symbolic policies that subject RSOs to unintended consequences without the benefit of reduced sex crime rates.

Finally, important policy implications can come from the research on the associations between residence restrictions and the spatial distribution of RSOs, and between the spatial distribution of RSOs and recidivistic sex crime rates. As noted earlier, while there is some existing literature exploring how changes in the spatial distribution of RSOs may affect the hardships they face (e.g., Barnes, et al., 2009; Mustaine, Tewksbury, & Stengel, 2006b; Socia, in press; Zandbergen & Hart, 2006), there is scant research linking these spatial distributions to recidivistic sex crime rates.

Additionally, if the spatial distribution of RSOs is indeed associated with recidivistic sex crime rates, it can help determine how future interventions should attempt to influence this spatial distribution so as to effectively *reduce* recidivistic sex crimes and better protect the public.

CHAPTER 2: REVIEW OF LITERATURE

Since at least the early 1990s, a number of public policies have applied specifically to RSOs either returning to or already living in the community. These policies have included mandatory public registration, community notification, temporary or lifetime electronic monitoring, chemical castration, civil commitment, and residence restrictions (see Bonnar-Kidd, 2010; Button, et al., 2009; Cohen & Jeglic, 2007; CSOM, 2007, 2008; Farkas & Stichman, 2002; Finn, 1997; Fitzgerald, 1990; Flack, 2005; IACP, 2008; Levenson, 2009; Levenson & D'Amora, 2007; Matson & Lieb, 1997; Socia & Stamatel, 2010; Towers, 2007; Wetterling & Wright, 2009; Winick, 1998). These policies attempt to reduce sex crime rates through either incapacitative measures (e.g., electronic monitoring, chemical castration, civil commitment, residence restrictions), or specific deterrence measures (e.g., public registration, community notification).

One of the most controversial policies, sex offender residence restrictions, has only recently become popular at the state, county and local government levels. Residence restriction policies prohibit certain RSOs from living within a given distance of specific places where children might gather, such as schools, daycares, parks, and playgrounds.[19] In doing so, the expectation is that RSOs will have a harder time finding and approaching young children, whom they can sexually assault, thus driving down sexual recidivism rates and the number of recidivistic sex crimes.[20]

[19] While these residence restrictions typically apply to RSOs convicted of sex crimes involving child victims, some residence restrictions include juvenile sex offenders, offenders with adult victims, offenders with low levels of risk for recidivism, and/or offenders already living in the community when the law is implemented.

[20] Many of these laws are named 'Jessica's Law,' after nine-year-old Jessica Lunsford, who in 2005 was abducted, raped and murdered by a convicted sex offender in Florida. Other names of these policies include residency restrictions, sex offender buffer laws or exclusion zones, or child safety zones.

This chapter reviews the history, current structure, and existing research on residence restrictions in the United States. Additionally, it reviews related research regarding RSO housing, including the spatial distribution of RSO residences and its influence on recidivistic sex crime rates. These literatures form the basis for the theoretical framework used to analyze this study's research questions. This study also draws on the literatures relating to the passage, diffusion, and consequences of criminal justice policies, incapacitation and deterrence research, and on the reentry considerations of ex-offenders generally and RSOs specifically. The chapter concludes by summarizing the existing literature and describing the theoretical model and research questions used in the present study.

History and Current State of Residence Restrictions

Compared to most other criminal justice policies, the history of residence restrictions is relatively short, having only begun at the state level in 1995 and at the county and local level in 2005. Presently at least 30 states and hundreds of counties and local jurisdictions have enacted residence restriction laws (Meloy, et al., 2008). This section reviews the history of state, county and local-level residence restrictions, and then focuses on county-level residence restrictions in the state of New York, which is the subject of the present study.

State-Level Residence Restrictions

Residence restrictions first began when Delaware and Florida passed statewide residence restriction laws in 1995; by 2004, fifteen states had followed suit (Levenson, 2009; Meloy, et al., 2008; Wetterling & Wright, 2009). In 2005, the intense media coverage of the abduction, rape and murder of Jessica Lunsford in Florida influenced

many state, county, and local governments to consider implementing their own residence restrictions, and by 2008 the number of states with such restrictions had doubled from 15 to 30 (Meloy, et al., 2008).[21]

Current residence restrictions vary both in size (i.e., the minimum distance RSO residences must be from certain child congregation locations) and scope (i.e., the child congregation locations subject to the 'buffer zones' of restricted housing). The size of state residence restrictions range from 500 to 2,500 feet, while the scope can be as conservative as to only include schools (e.g., Delaware), or as comprehensive as to include churches, schools, child care facilities, and a blanket statement that includes 'other areas where minors congregate' (e.g., Georgia) (Meloy, et al., 2008). These laws also vary in their inclusivity (i.e., the types of RSOs subject to these laws), with some only including RSOs who are seeking on-campus housing in a university setting (e.g. South Carolina), and others including virtually any RSO in the state, regardless of whether the offender was convicted as a juvenile, or whether their victim was a child or an adult (e.g., Arkansas, California) (see Meloy, et al., 2008).

County and Local-Level Residence Restrictions

In June 2005, shortly after Jessica Lunsford's murder, Miami Beach, Florida passed the first sub-state-level residence restriction law in the United States (Wetterling & Wright, 2009; Zandbergen, et al., 2010). Since then, hundreds of counties and local municipalities have implemented some form of these laws. County and local-level laws exist in states throughout the U.S., regardless of whether a state does or does not have a

[21] Interestingly, the Kansas State Legislature specifically banned county and local residence restriction policies, largely in response to the recommendations of the Kansas Sex Offender Policy Board (2007; see also Wetterling & Wright, 2009). This move, however, is in contrast to the legislative actions of most other states.

state-level residence restriction. Similar to state-level restrictions, these laws vary in their size, scope, and inclusivity. However, county and local-level restrictions have more variability than their statewide counterparts.[22] For example, while state-level residence restrictions range up to 2,500 feet in size (Meloy, et al., 2008; Wetterling & Wright, 2009), some local-level restrictions are as large as a mile (e.g., Malta, NY) (see New York State Office of Sex Offender Management, (NYS OSOM), 2010a). While a comprehensive review of existing state, county and local residence restriction laws in the United States is outside the scope of the present study, the next section provides a more detailed overview of the history and current state of residence restrictions in New York, which is the focus of the present study.[23]

Residence Restrictions in New York

While New York does not have an existing state residence restriction policy, it does contain many different county and local-level policies throughout the state (see NYS OSOM, 2010a).[24] On November 3, 2005, the village of Candor in Tioga County passed New York's first local-level residence restriction, and a few weeks later on November 30, Cayuga County passed the state's first county-level policy (NYS OSOM, 2010a). Presently, county-level residence restrictions in New York range from 500 to 2,000 feet in size and include a wide variety of scopes. The most conservative scope includes only

[22] This is likely due in part because the successful proposal and passage of these laws at the county and local level require fewer policymakers than at the state level, thus leading to greater variation (and more customization) between these policies.

[23] For a more detailed review of state residence restriction laws, see Meloy, et al. (2008) and Council of State Governments (2007).

[24] Technically New York does have a state residence restriction that applies only to RSOs on probation or parole that are either designated high risk (i.e., level 3) or whose victims were under 18 at the time of offense (see "New york state executive law," §259-c(14); "'New york state penal law," § 65.10(4-a)). However, this restriction only prohibits offenders from knowingly *entering* school grounds, and while the definition of school grounds includes public areas (parks, parking lots, stores, etc.) and/or vehicles located within 1,000 feet of the boundary line of a school, it does not appear to include private residential housing located within this 'buffer zone.' As such, the law does not appear to be an actual *residence* restriction, but rather a movement and/or loitering restriction.

parks and victims' residences and workplaces (e.g., Nassau County), while the most comprehensive scope includes virtually any location where children might potentially congregate (e.g., Orange County) (NYS OSOM, 2010a).[25] However, most residence restrictions appear to include some combination of schools, daycares, parks, and/or playgrounds in their scope. A list of both county and local-level laws in New York as of December 3, 2009 is provided in APPENDIX A.[26]

Existing Research on Residence Restrictions and Sex Offender Housing

This section reviews the existing research that has been conducted on residence restrictions and RSO housing. Specifically, the following five subsections review research regarding 1) the passage of residence restrictions, 2) their efficacy at reducing sex crimes, 3) their effect on RSO housing options, 4) their effect on the spatial distribution of RSOs, and 5) other neighborhood characteristics associated with RSO housing. These subsections each describe a part of the present study's theoretical model, and help formulate the first three research questions.

Passage of Residence Restrictions

To date, there has been little research examining the passage of residence restrictions. In fact, only a single study has examined the jurisdictional characteristics associated with their passage at the state level (e.g., Meloy, et al., 2008). In that study, it was found that states with residence restriction policies appeared geographically clustered

[25] While NYS OSOM (2010a) lists Dutchess County as having a residence restriction law, the provisions only require that RSOs complete and sign a Sex Offender Verification Form under oath, but does not specify locations where RSOs are prohibited from residing. As such, this study does not consider Dutchess County to have an actual residence restriction law in place. Additionally, some parole and/or probation offices may place residence restriction provisions on RSOs who may not otherwise be subject to a county or local residence restriction policy. Due to the wide variation in the application of such provisions, those restrictions placed by parole or probation offices on individual offenders are not considered in this study.
[26] NYS OSOM (2010a) declares that the compilation of local residence restriction laws is for information purposes only, and the accuracy cannot be guaranteed. Nevertheless, it represents the most complete and thorough compilation of county and local laws available for the state.

and were more likely to have a Republican/Conservative political philosophy.[27] While exploratory in nature, this early research indicates that geographic proximity and political philosophy may influence the passage of residence restrictions, at least at the state level.

One potential explanation for the findings of geographic proximity stems from research on 'regional diffusion models' in the literature on the diffusion of policy innovations (see Berry & Berry, 1999). Specifically, the spatial clustering of states with residence restrictions may be due to the influence that proximate adopters of innovations (which include public policies) have on other nearby states that are potential adopters (Berry & Berry, 1999; Gray, 1973; Rogers, 2003; J. L. Walker, 1969; Wejnert, 2002). This could result from either interstate competition or simple policy emulation, the latter of which would account for the many similarities between different state-level residence restrictions (see Meloy, et al., 2008).[28] In fact, the findings of Meloy and colleagues (2008) regarding geographic proximity are consistent with the argument that the passage of a residence restriction policy can create a domino effect of competing (and emulating) legislation, where other nearby jurisdictions pass their own residence restriction policy to keep from becoming a haven (or dumping ground) for exiled RSOs (e.g., Levenson, 2009; Wetterling & Wright, 2009; Yung, 2007; Zgoba, et al., 2009). If this is indeed the case, then it seems reasonable to expect that a county would also be more likely to pass a residence restriction policy if a geographically proximate county had already implemented one.

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[27] Specifically, states with residence restrictions were more than twice as likely to have voted for the 2004 Republican presidential candidate as compared to the Democratic candidate (21 vs. 9, respectively). Further, when considering all 50 states, those voting for the Republican candidate were more likely to have residence restrictions (21 of 31) as compared to those voting for the Democratic candidate (9 of 19).

[28] For a similar example regarding anti-immigration policy, see the research of Boushey and Leudtke (2006, pp. 209-210), and for more research on inter-state competition in the passage of policy, see Berry and Berry (1999).

However, passing and enforcing a residence restriction policy can also come with many legal and logistical challenges. If policymakers see neighboring counties facing these challenges, they may be less likely to pass a residence restriction policy of their own. As such, the passage of a nearby county residence restriction may either increase or decrease the likelihood that a county will pass their own restriction. Additionally, when a town or city passes a residence restriction, it seems reasonable that higher levels of government (such as the county) may feel pressure to implement their own residence restriction to keep its local jurisdictional members (i.e., towns and cities within the county) on equal legal footing. This study considers both of these potential domino effects when examining the passage of county-level residence restrictions.

While no further research has directly examined this issue as it relates to sex offender residence restrictions, the literature on the spatial diffusion and adoption of policies (particularly those that are criminal justice related) may provide helpful information about other influencers of the passage of residence restrictions. Unfortunately, much of this existing literature examines state-level policies and/or innovations (e.g., Canon & Baum, 1981; Doerner, 1979; Grattet, Jenness, & Curry, 1998; Soule & Earl, 2001; Williams, 2003), rather than county or local-level policies and/or innovations (but see Hoyman & Weinberg, 2006). As such, it is unclear whether the findings of the relatively modest state-level literature on the diffusion of criminal justice policies are entirely applicable either to county-level policies, sex offender policies, or both. Still, this literature provides a starting point for determining the county characteristics that may influence the passage of residence restriction policies.

For example, research on the diffusion of state hate crime laws in the United States shows that the early adopters of these laws were states that were "larger and richer and had more a liberal and progressive tradition" (Rogers, 2003, p. 276; Grattet, et al., 1998; Soule & Earl, 2001). Since residence restriction policies are inherently spatial in their application, and result in more restricted housing in very dense urban areas (Berenson & Appelbaum, in press; Socia, in press), it seems plausible that the physical size and/or population density of a county may also be associated with its likelihood of passing such a policy.[29] Further, wealthier counties may be more willing (and better able) to try new and innovative criminal justice policies to protect community members, and thus may be more likely to pass a residence restriction despite the lack of research supporting its effectiveness.[30]

However, while a more liberal/progressive political philosophy seems to be a plausible indicator of the likelihood of passing rights-based criminal justice policies (e.g., hate crime legislation) (see Gray, 1973), residence restriction policies are based on the idea of using incapacitation to control crime. As such, it seems more plausible that a *conservative* political ideology would be positively related to the passage of such a law (see Williams, 2003). In fact, the findings of Meloy and colleagues (2008) support this assumption, as do the findings regarding the passage of state-level criminal justice

[29] For instance, large counties with vast expanses of rural areas may find they have more physical space (and possibly more unrestricted housing) available for displaced sex offenders to live in, compared to smaller counties, especially those comprised mainly of dense urban areas.

[30] In fact, funding is one of the key barriers to successful implementation of many policies (Gerston, 1997). While this may be less important for policies that require little start-up funding (such as residence restrictions), there still may be a connection between the economic status of a jurisdiction and policymakers' abilities to successfully propose, support, and implement criminal justice policies.

policies such as the death penalty (see Fisher & Pratt, 2006; Jacobs & Helms, 2001; Jacobs & Jason, 2004).[31]

In addition to geographic clustering, political philosophy, physical size, population density, and economic status, the political competition and rate of sex crimes in a county may also influence the passage of residence restriction policies. For instance, it seems plausible that in counties where the majority party faces less competition from rival parties, politicians from the majority party will have less need to use public policymaking to garner votes for reelection. Conversely, when the majority party *does not* have a large lead in political support, politicians may be more desperate for individual voters' support in reelection campaigns, and thus may be more likely to propose new policies (such as sex offender residence restrictions) in order to entice voters to support their campaign (Williams, 2003). This view is consistent with research that finds more competition among state political parties and/or politicians will generally increase the rate of policy adoption and/or policy spending (e.g., Bibby & Holbrook, 2004).[32] As such, it seems plausible that counties with more political competition (i.e., a *lower* ratio of the number of registered voters in the majority party compared to the number of registered voters in all other parties) will be more likely to have implemented a residence restriction policy. However, it may also be that jurisdictions with weak majority parties will have a harder time implementing their own policy agenda, and thus political competition may be *negatively* related to the passage of residence restriction policies.

[31] However, other research by Williams (2003) on passage of state-level legislation relating to truth-in-sentencing laws, three-strikes legislation, boot camps, and juvenile court transfer provisions indicated that voter ideology had a mixed influence. Specifically, it was only significant in models analyzing boot camp and juvenile court transfer legislation, but not the other two policies.

[32] For a review of various methods of measuring party competition, see David (1972) and Bibby and Holbrook (2004).

Regarding the crime rate, politicians in counties that have high sex crime rates may be more likely to pass a residence restriction policy in an attempt to reduce these crime rates. Addressing high crime rates with public policies could also be used to gain (or retain) the support of concerned residents for political purposes (see Williams, 2003).[33] In any event, it is expected that counties with high sex crime rates will be more likely to implement residence restriction policies compared to counties with lower sex crime rates.

Building upon the results of Meloy and colleagues (2008), and on the literatures regarding the passage and diffusion of criminal justice policies and political competition, the first research question involves determining the county demographic, social, political, and crime characteristics that are associated with the likelihood of passing county residence restriction laws in New York. While admittedly exploratory in nature, the results of this study will provide the basis for further research on the passage of county-level sex offender policies, and help to situate these findings into more general (and established) literatures.

Efficacy to Reduce Sex Crimes

While only a single study has examined the passage of residence restrictions, perhaps more concerning is that only two studies have examined whether residence restrictions are effective at reducing sex crimes.[34] The earliest study analyzed the

[33] Additionally, media attention given to high profile sex crime cases (e.g., Megan Kanka, Jessica Lunsford) may influence the passage of residence restrictions at the county level. However, this media attention would likely be similar in all counties due to the widespread, frequently national coverage of such cases. Thus, such cases will not be considered in the present analysis, as they do not constitute a characteristic specific to an individual county.

[34] While not examining residence restrictions specifically, three additional studies indicated that proximity to schools and/or daycares was unrelated to sexual recidivism (e.g., Colorado Department of Public Safety (CDPS), 2004; MNDOC, 2003; Zandbergen, et al., 2010), which is counter to the underlying assumptions of such laws. In addition to finding that proximity to schools and daycares did not influence recidivism,

retroactive case histories of 224 recidivist RSOs in Minnesota from 1990 to 2006 (Duwe, et al., 2008; MNDOC, 2007). Although Minnesota did not have a residence restriction policy at the time of the study, researchers concluded that not one recidivistic sex crime in the sixteen years of data would have been deterred had a residence restriction policy been in place.[35] The most recent study, part of a larger state report on crime, found no evidence that Iowa's statewide residence restriction had reduced sex crimes against minors (Blood, et al., 2008). However, it appears that researchers only compared three years of annual conviction rates, and did not separate crimes committed by individuals with and without a prior sex offense conviction, thus combining any unique specific and general deterrence effects the policy may have had.[36] While this limited prior research suggests that residence restriction policies may not be effective at reducing sex crimes, there is not yet enough empirical evidence to generate firm policy conclusions.

Additionally, a residence restriction policy is unlikely to affect all sex crimes in an equivalent manner. As noted earlier, residence restrictions are meant to reduce sex crimes committed against children by RSOs (i.e. those individuals with a prior conviction for a sex crime) under very specific circumstances. Therefore, when examining the effect these policies have on sex crimes, a distinction must be made between sex crimes committed 1) against children by individuals with a conviction for a prior sex crime, 2) against adults by individuals with a conviction for a prior sex crime, 3) against children

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Zandbergen and colleagues (2010) also examined buffer zones of multiple sizes around schools and daycares and determined that recidivists were no more likely to live within these buffer zones than non-recidivists.

[35] The researchers based their conclusion on the profiles of the recidivistic sex crimes, which were compared to the profile of the types of sex crimes potentially deterred by residence restriction policies.

[36] The data in that study included 12 months of convictions prior to the restriction and 24 months after the restriction, and it appears that these rates were aggregated into three 12 month blocks for the comparison. Also analyzed were six month blocks of conviction data, both before and after the passage of the statewide residence restriction. It was unclear what statistical tests were used to confirm the lack of significant change or what methods were used to control for other external influences on sex crime convictions.

by individuals without a prior conviction for a sex crime, and 4) against adults by individuals without a prior conviction for a sex crime.

As noted in the previous chapter, even if these policies work as intended, they may only have a modest effect on recidivistic sex crimes committed against children, due to the rarity of the types of sex crimes they target (i.e., a current RSO selecting a stranger child victim at or around a child congregation location that is also located near the RSO's residence). As such, any direct reduction that these policies have on recidivistic sex crime rates would likely occur from the incapacitation they impose on RSOs via their relocation to other neighborhoods that may have fewer opportunities to encounter all types of suitable targets (not just children who are near child congregation locations).

However, if the relocation of these RSOs limits their ability to access stable housing, suitable employment, supportive family members, and/or adequate treatment facilities (see Barnes, et al., 2009; Casady, 2009; CSOM, 2007, 2008; Levenson, 2008; Levenson & Cotter, 2005; MNDOC, 2003; Zandbergen & Hart, 2006), then the passage of these policies could result in an increased rate of recidivistic sex offenses.

Additionally, the passage of a residence restriction policy may result in decreased non-recidivistic sex crimes, if the *threat* of these policies deters individuals who are *not yet* RSOs from committing sex crimes (i.e., general deterrence). As noted earlier, while this seems plausible given the many unintended consequences these polices can cause for RSOs (see Bagley, 2008; Levenson, 2008, 2009; Levenson & Hern, 2007; Levenson & Tewksbury, 2009; Tewksbury, 2007; J. T. Walker, 2007), there is no research evidence supporting this claim.

In considering these different types of sex crimes, and acknowledging the different mechanisms that could reduce or increase sex crime rates, the second research question involves determining whether existing county residence restrictions are affected any of the four different types of county sex crime rates in the state of New York via either incapacitation or increased hardships (for recidivistic sex crime rates) or general deterrence (for non-recidivistic sex crime rates).

Affecting Sex Offender Housing Options

In contrast to the lack of research on the passage and efficacy of residence restrictions, numerous prior studies have examined how proposed or actual residence restriction policies affect RSO housing options (e.g., Barnes, et al., 2009; Berenson & Appelbaum, in press; Chajewski & Mercado, 2009; Grubesic, et al., 2007; Grubesic & Murray, 2008, in press; Grubesic, et al., 2008; L. A. Hughes & Burchfield, 2008; Mercado, Alvarez, & Levenson, 2008; Red-Bird, 2009; Socia, in press; Zandbergen & Hart, 2006, 2009a, 2009b; Zgoba, et al., 2009). These studies, typically conducted at the county level, find that residence restrictions limit available and affordable RSO housing options, particularly in dense, urban neighborhoods (see Barnes, et al., 2009; Berenson & Appelbaum, in press; Chajewski & Mercado, 2009; Levenson, 2008; Levenson & Cotter, 2005; Red-Bird, 2009; Socia, in press; Zandbergen & Hart, 2006, 2009a, 2009b; Zgoba, et al., 2009; but see Grubesic, et al., 2007).

Thus, a residence restriction might mean that RSOs are more likely to be found in rural neighborhoods, particularly those that still offer unrestricted housing that is affordable (i.e., low rent) and available (i.e., vacant rental units). As a result of these limited housing options, residence restrictions may decrease the chances of successful

reentry for RSOs. This is particularly important and plausible, since finding housing is one of the key factors in the successful reentry (Andrews & Bonta, 2007; CDPS, 2004; CSOM, 2007; Roman & Travis, 2004; Solomon, Visher, La Vigne, & Osborne, 2006; Visher, La Vigne, & Travis, 2004).

Affecting the Spatial Distribution of Sex Offenders

By affecting RSO housing options, residence restrictions may also influence the spatial distribution of RSOs. Despite the wealth of research on how residence restrictions affect RSO housing options, only a few studies have measured how residence restrictions affect the actual or the potential spatial distribution of RSO residences at the neighborhood level (e.g., Grubesic, 2010; Grubesic & Murray, 2008, in press; Grubesic, et al., 2008; Morgan, 2008; Youstin & Nobles, 2009). In these studies, RSO clustering has been measured in a variety of ways, and each method has its own strengths and limitations. These studies, and their associated methods of measuring clustering, are reviewed below.

In one of the earliest studies, Grubesic and colleagues (2008) compared the demographic and social differences between 'restricted' and 'unrestricted' census blocks in Hamilton County, Ohio, based on the majority of parcels in each block being restricted or unrestricted, respectively, by a residence restriction policy. Results indicated that there was little difference in the affordability of housing between restricted and unrestricted locations. Although the spatial distribution of RSOs was not the focus of the study, RSO 'clustering' was measured as the density of RSO residences per square mile. While this measure obviously accounts for differences in the spatial size of each areal unit, it does not account for differences in the number of residents living within each

areal unit. Still, it does imply that the physical size of an area might influence the spatial distribution of RSOs.

Using data from the same geographic location, Grubesic and Murray (2008) used a chi-square test based on the actual and expected frequency of RSO residences in different areas within the county. The researchers noted that while this measure can be used to confirm that there exists uneven distributions of RSOs (i.e., clusters) in an area, it does not inform on the 'geographic specificity' of this distribution within neighborhoods (Grubesic & Murray, 2008).[37] Thus, this method is most appropriate when considering whether the overall distribution of RSO residences is clustered within neighborhoods across a larger area (such as a county).

About the same time, Morgan (2008) measured the spatial distribution of RSO residences in Bay County, Florida after local municipal residence restriction policies were passed by cities within the county. This distribution was measured using nearest neighbor analysis (NNA), a method that calculates the average distance between all paired combinations of relevant points (i.e., between all pairs of RSO residences) for an entire area. While this measure is useful when comparing distributions over time, it is not particularly useful for comparing between two areas, as it does not account for the distribution of the residential population (and associated housing) or the physical size differences between those areas. As such, the use of this method to compare between two areas should at a minimum include controls for the population and/or housing density and the physical size of each area.

[37] In their study, Grubesic and Murray (2008) also used another measure based on the geographic distribution of RSOs. That method, however, relies on specifying a desired distance between RSO residences in an area, which is outside the scope of the present study and is thus not considered.

Similarly, Youstin and Nobles (2009) examined the spatial distribution of RSO residences in Alachua County, Florida before and after the implementation of a 2,500' local-level residence restriction policy in the city of Gainesville. In that study, the spatial distribution was measured at the census block group level with LISA (Local Indicators of Spatial Association) analysis based on the proportion of RSOs compared to the number of residents in a given block group.[38] While LISA values can identify significant clustering in relation to geographically proximate block groups, they are less useful for determining whether these locations still contain clusters after accounting for the expected distribution of RSOs in neighborhoods throughout an entire area, regardless of geographic proximity at the block group level.[39]

Finally, one of the most recent studies on the subject of RSO clustering was conducted by Grubesic (2010) in the state of Illinois.[40] In this study, RSO residences were aggregated to the ZIP code level and analyzed using a variety of techniques to explore, identify and measure spatial clustering. Perhaps the most valuable technique for identifying clustering in individual areal units was the measurement of excess risk, which compared the actual number of RSO residences in a neighborhood (i.e., ZIP code) to the expected number, which was calculated based on the number of RSOs and residents in

[38] Clustering was also measured using point data by examining the mean interpoint distance between RSO residences, and by tabulating the number of RSOs by their geographic proximity to residence restriction zones.

[39] Grubesic and Murray (in press) note that the popular press indicates that a cluster may be considered a community with a higher concentration of RSOs than surrounding communities. When considering relatively small geographic areas, such as the block group, this distinction seems less useful compared to considering clustering based on the block group having a much higher concentration of RSOs *than would otherwise be expected* given the number of RSOs living in the larger community (i.e., a city or county). Still, this definition seems most similar to LISA measures.

[40] Grubesic (2010) noted that Illinois only has a 500-foot residence restriction, and thus it is unclear to what extent this relatively small restriction had influenced RSO clustering.

each specific neighborhood and across the entire study area.[41] Values of excess risk above 4 indicate neighborhoods that have an elevated risk of exposure to RSOs (Grubesic, 2010), although values above 1.0 still indicate more RSOs than would otherwise be expected.[42]

While excess risk seems appropriate for examining individual neighborhoods, a slightly different measure would be required to analyze the spatial distribution of RSOs at the county level. One example would be the Revised Index of Isolation (R.I.I.). When used to analyze RSO residences, this measure compares the probable interaction of RSOs within a neighborhood given the current distribution of RSOs and residents, to the probable interaction of RSOs within a neighborhood if RSOs were homogenously distributed across all neighborhoods within the county based on the current distribution of residents across all neighborhoods.[43]

Another set of measurements discussed by Grubesic (2010) involved global autocorrelation, including the Moran's *I* statistic and the Oden's (1995) I^*_{pop} statistic, which is essentially a Moran's *I* statistic adjusted for a heterogeneous population distribution across a study area (see also Gregorio, DeChello, Samociuk, & Kulldorff, 2005). Compared to Moran's *I*, the Oden's I^*_{pop} statistic would be more valuable for examining the countywide spatial distribution of RSOs across all neighborhoods, rather than comparing between individual neighborhoods. FIGURE 2 shows a basic graphical

[41] More information on the specific equation used to measure relative (and excess) risk can be found on page 5 of the Grubesic (2010) study, and in chapter 3 and APPENDIX D of the current study.
[42] One limitation of this method is that neighborhoods that have very low resident population values (e.g., 1 or 2 residents) have the potential to mistakenly indicate extreme clustering (i.e., excess risk) when *any* number of RSOs live in the area. However, when neighborhoods are relatively similar in their population sizes (such as with census block groups), this becomes less of a concern.
[43] For more information regarding measures of segregation, including the R.I.I. and its historic uses in racial segregation, see the works of Bell (1953, 1954), Duncan and Duncan (1955), Poston and Micklin (2006, p. 507), Reardon and O'Sullivan (2004), and Shevky and Williams (1972, c.1949).

interpretation of what different values of the Oden's I^*_{pop} statistic represent in terms of the spatial distribution of RSO residences across neighborhoods within a county, controlling for neighborhood population levels.

Of these studies, only two have compared the clustering of RSOs over time (e.g., Morgan, 2008; Youstin & Nobles, 2009), and both were set in Florida counties that were already subject to a statewide 1,000' residence restriction. Despite using different measures of clustering, both studies found that the clustering of RSOs decreased following the implementation of local-level residence restriction policies (albeit marginally in the study by Morgan (2008)). That is, the spatial distribution of RSOs became increasingly dispersed throughout the study area. Further, areas *without* local-level residence restrictions experienced increases in the number of RSO residences, compared to decreases in areas *with* local-level residence restriction policies (Morgan, 2008; Youstin & Nobles, 2009). These results indicate that RSOs had been dispersed into more rural (and less restricted) areas and ultimately became less clustered throughout the entire county as a result. However, it is still unclear if certain neighborhoods contained higher concentrations of RSOs than would otherwise be expected given the underlying distribution of residents (and housing) throughout neighborhoods within the county.

Overall, the limited existing research finds that residence restrictions may affect the spatial distribution of RSOs, although perhaps not as expected. Specifically, RSOs may become *less* clustered (i.e., more dispersed) as the result of a residence restriction (see Morgan, 2008; Youstin & Nobles, 2009), although these results are still tentative. In terms of the measurement of these clusters, various methods have been used, including

spatial density (Grubesic, et al., 2008), chi-square tests (Grubesic & Murray, 2008), LISA values (Youstin & Nobles, 2009), nearest neighbor analysis (Morgan, 2008), excess risk measures (Grubesic, 2010), and global autocorrelation measures such as Oden's I^*_{pop} statistic.

Of these, certain methods appear to hold the most promise for measuring either the within or between neighborhood-level spatial distribution of RSO residences within a county (e.g., nearest neighbor analysis, excess risk, and LISA values), or measuring the overall spatial distribution between all neighborhoods within a county (e.g., nearest neighbor analysis, Oden's I^*_{pop}). As noted earlier, the R.I.I. can also act as a measure similar to excess risk, but at the county level.

Other alternative techniques used to measure clustering, such as the spatial moving average method, suffer distinct limitations that make them not attractive for the present study. For instance, techniques involving spatial smoothing are known to be less valuable for identifying *outlying* clusters of RSOs because they are computed based on a regional average (Anselin, Syabri, & Kho, 2006), similar to LISA measures. Some techniques, such as the spatial scan statistic, have been noted to be very sensitive to changes in the settings used to measure these clusters (e.g., the scaling parameters of the SaTScan software program), and could potentially misidentify large groups of spatially proximate neighborhoods as clusters (see Grubesic, 2010). Thus, the present study does not consider these alternative techniques.

Other Neighborhood Characteristics Related to Sex Offender Housing

While residence restrictions *may* affect the spatial distribution of RSOs, this distribution may also be affected by neighborhood characteristics that are unrelated to the

presence of a residence restriction. In other words, certain neighborhood characteristics can affect where RSOs are most likely to reside and/or cluster *before* a residence restriction is ever enacted. This is an important consideration, as ignoring these characteristics could lead to biased conclusions about whether and how residence restrictions affect the spatial distribution of RSOs. For instance, if results indicate that certain neighborhoods contain an increased clustering of RSO residences in counties that have a residence restriction policy in place, these neighborhoods could actually contain many RSO residences simply as a result of internal neighborhood characteristics unrelated to the residence restriction. This subsection reviews research regarding RSO housing options *prior* to a residence restriction policy, and describes the neighborhood characteristics that are associated with RSO residences.

Much of the existing literature finds that RSO residences are frequently located in neighborhoods that exhibit indicators of social disorganization (e.g., Barnes, et al., 2009; Craun, in press; Grubesic, 2010; Grubesic & Murray, 2008, in press; Grubesic, et al., 2008; Hipp, Turner, & Jannetta, 2010; L. A. Hughes & Burchfield, 2008; L. A. Hughes & Kadleck, 2008; Mustaine & Tewksbury, 2008; Mustaine, Tewksbury, & Stengel, 2006a; Mustaine, et al., 2006b; Red-Bird, 2009; Socia & Stamatel, in press; Tewksbury, 2007; Tewksbury & Mustaine, 2008; Turley & Hutzel, 2001; but see Tewksbury and Mustaine, 2006; Youstin & Nobles, 2009).[44] This is not surprising, as disorganized neighborhoods typically offer some of the most available and affordable housing in a county (see Socia, in press). Both the availability and the affordability of housing are key concerns for ex-offenders returning to communities (Andrews & Bonta, 2007; CDPS, 2004; Roman &

[44] The structural characteristics of social disorganization include measures of concentrated disadvantage, residential instability, and ethnic heterogeneity.

Travis, 2004; Solomon, et al., 2006; Visher, et al., 2004). At least in New York, the most disorganized neighborhoods are also typically the most dense in terms of population (see Socia, in press). Thus, without considering the effects of a residence restriction, it is likely that RSOs are more likely to be found (i.e., clustered) in neighborhoods offering more affordable and available housing, which are also likely the most dense and disorganized neighborhoods within a given county.

Therefore, in order to examine whether and how residence restrictions affect the spatial distribution of RSO residences, neighborhood characteristics that are expected to be associated with RSO housing, such as measures of social disorganization, housing availability, housing affordability, and population density, must also be considered.[45] After controlling for the influence of other neighborhood characteristics, if the presence of a residence restriction is associated with the spatial distribution of RSOs, these policies may also be influencing both RSOs' successful reentry and rehabilitation, and the safety of nearby residents. These considerations will be discussed in the next section. Accordingly, the third research question involves examining whether residence restrictions are associated with the spatial distribution of RSO residences in upstate New York neighborhoods, controlling for other demographic and social indictors related to RSO housing.

Sex Offender Clustering and Recidivistic Sex Crime Rates

[45] For example, in New York, a residence restriction policy will generally restrict a disproportionate amount of housing in the most dense and disorganized neighborhoods of a county (Socia, in press). As a result, counties without residence restriction policies might have RSOs clustered in the most dense and disorganized neighborhoods, possibly as a result of seeking the most available and affordable housing options. However, counties with residence restriction policies might have RSOs clustered in the *least* disorganized neighborhoods, possibly because of the high levels of restricted housing contained in more disorganized neighborhoods.

This section reviews the limited research on, and theoretical connection between, RSO clustering and recidivistic sex crime rates. In doing so, it describes the final part of this study's theoretical model, and sets up the basis for the fourth research question.

Although individual level studies find little connection between living near a child congregation location and sexual recidivism (e.g., CDPS, 2004; Duwe, et al., 2008; MNDOC, 2007; Zandbergen, et al., 2010), the relationship between the spatial distribution of RSOs and recidivistic sex crime rates is still tentative. For instance, clustering RSOs into specific areas, whether the result of residence restrictions or other socio-economic processes, may increase the hardships they face in finding stable housing, employment, and support/treatment, which can ultimately increase recidivism rates (Bonnar-Kidd, 2010; Levenson & Cotter, 2005; Mercado, et al., 2008). Therefore, increased clustering of RSOs within a county may be associated with *increased* recidivistic sex crime rates, resulting from the unintended consequences these RSOs are facing. In this instance, clustering would be indicative of the increased hardships facing RSOs in the county's neighborhoods.

Alternatively, research on RSOs in Colorado has found that clustering RSOs into shared living arrangements can actually *reduce* recidivism and lead to more effective monitoring of probation or parole violations or other criminal actions (CDPS, 2004). Therefore, increased clustering of RSOs may be associated with *reduced* recidivistic sex crime rates at the county level. In this instance, clustering would be indicative of increased social control mechanisms in such neighborhoods, either because of the types of neighborhoods these RSOs are clustering into or the ease of monitoring these groups of RSOs due to their physical proximity to one another.

However, the clustering of RSOs may have no relationship to county-level recidivistic sex crime rates, although no studies have yet made this claim. Unfortunately, there is little research confirming which of these hypothetical relationships is most valid, and this leaves a significant gap in the literature regarding sex offender residence restrictions, RSO housing, and recidivistic sex crime rates.

Regardless of whether a relationship exists between the spatial distribution of RSOs and recidivistic sex crime rates, in order to isolate the association between these two, there must be controls for other county-level characteristics that are associated with sex crimes rates. For example, it seems obvious that counties with more RSOs may have higher rates of recidivistic sex crimes (controlling for the population of residents) simply because they contain more potential recidivists. Further, prior research finds that neighborhoods with certain characteristics experience increased crime rates (and reduced reentry success of ex-offenders), which can include increased rates of recidivistic sex crimes. These characteristics include indicators of social disorganization (e.g., economic deprivation, unemployment, residential instability, ethnic heterogeneity) (Hipp, et al., 2010; Holzer, Raphael, & Stoll, 2003; Kubrin & Stewart, 2006; Mulford, et al., 2009; Peterson, Krivo, & Harris, 2000; Pratt & Cullen, 2005; Sampson, 1985; Smith & Jarjoura, 1988), and population and/or structural density (Sampson, 1985; Smith & Jarjoura, 1988; but see J. T. Walker & Ervin-McLarty, 2000). Additionally, controlling for rates of crimes that are unrelated to the clustering of RSOs (e.g., robbery, burglary) can help control for county-level crime policy differences that could otherwise affect the rate of recidivistic sex crimes.

If residence restrictions are associated with the spatial distribution of RSO residences, and this spatial distribution is in turn associated with recidivistic sex crime rates (after controlling for other influences), then residence restriction policies could be indirectly affecting recidivistic sex crime rates *through* their effect on the spatial distribution of RSO residences.[46] As such, the fourth research question builds on the results of the third research question and involves determining whether the neighborhood spatial distribution of RSOs is associated with county recidivistic sex crime rates in upstate New York, controlling for other potential influencers of the recidivistic sex crime rate.

Summary

First passed at the state level in 1995 and at the county and local level in 2005, residence restrictions typically range between 500 and 2,500 feet in size, and almost always include schools and daycares in their scope (Meloy, et al., 2008), with many also including parks and playgrounds. They also vary as to what types of RSOs are subject to these policies, although most state-level policies do not distinguish between RSOs with child victims and those with adult victims. Presently, residence restriction policies are in place in over half of the states and in hundreds of counties and local jurisdictions. While similar to their state counterparts, county and local-level restrictions typically have more variation as to their size and scope. As noted earlier, New York currently does not appear to have an official statewide residence restriction policy, but there are numerous county and local-level restrictions within the state.

[46] Note that this indirect relationship between residence restriction policies and recidivistic sex crime rates via the spatial distribution of RSO residences (i.e., the combined results of research questions three and four) is different from the direct relationship between residence restrictions and recidivistic sex crime rates via incapacitative effects, which is examined in research question 2.

In terms of existing research, only a single study has examined the passage of statewide restrictions, and only two studies have analyzed whether these policies effectively reduce sex crimes, with all of these studies yielding limited and tentative conclusions. While most of the existing research on residence restrictions has examined how these policies restrict housing for RSOs, very few of these studies measure the spatial distribution of RSO residences, and none has compared these distributions between areas that do and do not have such policies. Further, the link between these distributions and recidivistic sex crime rates is tentative at best, despite its importance for considering whether these policies are worthwhile.

The Current Study

Based on the existing research on sex offender residence restrictions, the current study explores four potential relationships (see FIGURE 1). The first relationship concerns county-level demographic, social, political, and crime characteristics and their influence on the likelihood of a county passing a residence restriction policy. The second relationship concerns county residence restriction policies and their effect on different types of county-level sex crime rates. The third relationship concerns residence restriction policies and their association with the spatial distribution of RSOs within and between neighborhoods. The final relationship concerns the spatial distribution of RSOs across neighborhoods within a county, and its association with county recidivistic sex crime rates.[47]

[47] While it may appear that the second and fourth relationships are competing with one another, it is more accurate to view the second relationship as the direct effects of a residence restriction on sex crime rates by individuals without prior sex crime convictions (via general deterrence), and by individuals with prior sex crime convictions (via the incapacitative effect of the restriction), and view the third and fourth relationships as testing the indirect effect of a residence restriction on sex crime rates by individuals with prior sex crime convictions *through* its effect on the spatial distribution of RSO residences.

Based on these relationships, this study considers four specific research questions. Drawing from research on state-level policy diffusion and sex offender legislation, the first research question involves determining the county demographic, social, political, and crime characteristics (see TABLE 1) that are associated with the likelihood of passing county residence restriction laws in New York. Drawing on the literature regarding incapacitation, deterrence, and reentry hardships, the second research question involves determining whether existing county residence restrictions have affected any of four different types of county sex crime rates in the state of New York (see TABLE 2). Drawing on the literature regarding housing and reentry concerns, the third research question involves determining whether residence restrictions are associated with the spatial distribution of RSO residences in upstate New York neighborhoods, controlling for other demographic and social indictors associated with RSO housing options (see TABLE 3). Building on the third research question, and drawing on the limited literature regarding RSO residences, reentry hardships, and recidivism, the fourth research question involves determining whether the neighborhood spatial distribution of RSO within a county is associated with county recidivistic sex crime rates in upstate New York, controlling for other demographic and social indicators related to recidivistic sex crime rates (TABLE 4). The next chapter explains the specific data and methodology used to analyze these research questions.

CHAPTER 3: DATA AND METHODOLOGY

This study examines four research questions relating to the passage, efficacy, and consequences of sex offender residence restrictions in the state of New York. This section outlines these research questions, their associated hypotheses, and the data, samples, variables, and analytical methods used to examine them.

Research Question 1

Research Question

The first research question involves determining the county-level characteristics that are associated with passing county residence restriction policies.

Hypothesis

As this research question is exploratory in nature, a detailed hypothesis is beyond the scope of the present study. However, based on prior research, it is expected that certain county characteristics *may* be associated with the likelihood of a county passing a residence restriction policy. These describe the demographic, social, political, spatial, and crime characteristics of a county that, based on the existing and related literatures reviewed earlier, could influence the likelihood of passing a county residence restriction policy. These characteristics are listed in TABLE 1.

Data and Sample

The sample for the first research question consists of county-level data for all 62 counties in the state of New York.[48] These data came from county legislative data provided by NYS OSOM (2010a), the U.S. Census (2002, 2010), the New York State

[48] Including all New York counties provides for more generalizable results than if a subsample of New York counties was used. Further, the county-level focus of the analysis will control for any undue influence that would result from including the many extremely urban counties of the New York Metropolitan Statistical Area (NYMSA) in the analysis, since each county has the same opportunity to influence results.

Board of Elections (NYS BOE) (2010) and the New York State Division of Criminal Justice Services Computerized Criminal History Database (NYS DCJS CCH) (2010).[49]

Variables

Dependent variable. For the first research question, the dependent variable is a dichotomous measure of whether a county did (1), or did not (0) pass a residence restriction between November 2005 and December 2009.[50]

Independent variables. The independent variables measure the county-level demographic, social, political, spatial, and crime characteristics that could predict the passage of a county residence restriction, as indicated in the prior review of the literature. These variables include the following county-level characteristics: geographic proximity to an existing county or local residence restriction policy, political philosophy, physical size, population density, wealth (i.e., mean resident income), political competition, and the rate of registerable sex crimes in 2004 (i.e., the number of sex crime arrests, controlling for the number of residents in a county and regardless of victim age or registration status of the accused). The exact measurements and data sources for each of these variables are presented in TABLE 1.

Missing from the analysis is a variable measuring the rate of RSOs living in a county. While this may affect the likelihood that a county passes a residence restriction policy, as the policy it may be seen as a way to 'deal with' the perceived problem of many RSOs living in the community, these historical data were not available at the

[49] The most recent U.S. Census data available for many of the demographic and social characteristics come from the year 2000. While these data may admittedly be somewhat out of date, the U.S. Census is the most reliable source of these data. A follow up study is expected to use 2010 U.S. Census data once they become available.

[50] As noted earlier, November 2005 was the month in which the first county-level residence restriction was passed in New York, and December 2009 is the last month for which crime data was collected.

county level. Further, including the current rate of RSO residences could generate tautological issues in the analyses, as the presence of a policy may have influenced the rate of RSOs moving into or otherwise remaining in the county. Thus, the analysis cannot indicate whether the population of RSOs in some way influences the likelihood that a county will pass a residence restriction policy.

Analytical Strategy

As the dependent variable is dichotomous, the analysis uses a multivariate logistic regression model to determine the county-level demographic, social, spatial, political, and crime characteristics that are associated with the likelihood of passing a residence restriction policy (see TABLE 1).[51] However, given the relatively few observations included in the analysis (62), logistic regression results may not be appropriate. This is because maximum likelihood models like logistic regression generally require a minimum of about 100 observations, with an additional 10 observations for each independent variable in the model, in order to provide consistent *and* unbiased estimates (Long, 1997). Thus, the analysis also uses a linear probability model with robust standard errors for comparison purposes.[52] The robust linear probability model, while not ideal for a dichotomous dependent variable, can provide results that may be less biased

[51] A multivariable logistic regression model predicts the likelihood that the dichotomous dependent variable will be one (1) based on levels of the independent variables. While the direct interpretation of coefficients in the model are in terms of the log-odds change in the likelihood of the dichotomous variable being 1 resulting from a one-unit increase in the independent variables, a more intuitive interpretation is possible. Specifically, by exponentiating the original coefficient to provide the odds-ratio, then subtracting one and multiplying the result by 100, the log odds are transformed into the percentage change in the likelihood of the dependent variable being 1. In this study, this can be interpreted as the percentage change in the likelihood of a county implementing a residence restriction policy resulting from a one-unit increase in the independent variable under consideration. Results for this analysis are presented as odds-ratios. For all of the analyses in this study, a relationship between the independent variable and the dependent variable is considered significant based on an alpha level of .05.

[52] A linear probability model is an ordinary least squares model that incorporates a dichotomous dependent variable. The model provides coefficients representing the change in the likelihood that the dependent variable is 1 given a one-unit increase in the independent variable.

when using relatively low numbers of observations, as it is based on OLS regression (see Long, 1997).[53]

Research Question 2

Research Question

The second research question is whether the presence of residence restriction policies reduced any of the four different types of county-level sex crime rates.

Hypotheses

Since residence restriction policies apply only to certain individuals with a prior conviction for a sex offense, any decrease in the overall rate of sex crimes committed by RSOs (i.e., recidivistic sex crimes) against either child or adult victims would likely be the result of incapacitation via spatial relocation. However, it could also be that relocating RSOs results in increased hardships, leading to *increased* rates of recidivistic sex crimes. As such, this research question tests the null hypotheses that the presence of a residence restriction policy has not affected the rate of recidivistic sex crimes committed against either child or adult victims.[54] If either of these null hypotheses is rejected, then this could indicate that an incapacitative effect has reduced recidivistic sex

[53] The author would like to thank Dr. Robert Apel for his helpful comments and advice regarding this and many other issues with the analyses.

[54] There are two legal constraints that could potentially influence findings. First, these policies may contain grandfather clauses that exempt sex offenders already living in the community at the time of passage from complying with the restrictions until they move from their current residence. Additionally, these laws vary in the types of registered sex offenders subject to the provisions, with some applying to only high risk offenders, only offenders with child victims, or only offenders still on post-release supervision. Still, within the state of New York, the majority of county and local-level laws do not include specific grandfather provisions, and most identify only 'level 2 and 3 sex offenders' as those subject to the restrictions, which are those offenders whose information is released on the NY Sex Offender Registry website. The variations in these policies may have some influence on final results. However, due to the methodological complexity in accounting for these specific provisions that may both vary between and overlap geographical boundaries, these remain as acknowledged limitations of the current study.

crime rates, or that an increase in the hardships that influence recidivism has increased recidivistic sex crime rates.[55]

Additionally, since much of the existing literature finds that residence restrictions impose severe unintended consequences on those individuals subject to such restrictions, it is possible that a residence restriction could have a general deterrence effect on sex crimes committed against either child or adult victims by individuals *without* prior sexual convictions (i.e., non-recidivistic sex crimes). However, there is no research indicating this is either likely or unlikely to be the case. It could also be that the media influence that surrounds the passage of these policies may increase the *reporting* of sex offenses to the police. If this is the case, the passage of a residence restriction should be associated with an increase in the rate of both recidivistic and non-recidivistic sex crimes. As such, this study also tests the null hypotheses that the implementation of a county-level residence restriction has had no effect on the rate of non-recidivistic sex crimes committed against either child or adult victims. Rejecting either of these null hypotheses would likely indicate that residence restrictions either have a deterrence effect that has reduced non-recidivistic sex crimes, or have increased non-recidivistic sex crimes through an increase in the reporting of such crimes to police.

Data and Sample

Similar to research question 1, the sample for the second research question consists of county-level data from all 62 counties in the state of New York. However, for

[55] This is not to say that a finding of no effect means that residence restrictions do not stop *any* sex offenses committed by individuals with prior sexual convictions, but rather that the types of sex offenses they target may simply be too rare, as found by existing research (e.g., Duwe, et al., 2008; MNDOC, 2007), to yield a substantial decrease in the rate of recidivistic sex crimes. However, even this may not be the case, as existing research also questions the underlying theoretical connection between proximity to schools and daycares and sex offender recidivism (e.g., CDPS, 2004; MNDOC, 2003, 2007; Zandbergen, et al., 2010).

this research question the data were collected on a monthly basis for each county between January 1998 and December 2009, resulting in 144 months of data for each county. As such, the unit of analysis is the county-month, which represents the characteristics of a county in a given month and yields 8,928 cases (i.e., 144 months of data for each of the 62 counties in New York). These data came from NYS OSOM (2010a) and the NYS DCJS CCH (2010).

Variables

Dependent variables. For the second research question, the four dependent variables are monthly measures of each of four different types of sex crime rates. These four series measure the rates of sex crimes 1) committed against a child by an offender with a prior sex offense conviction (i.e., recidivistic sex crimes committed against children), 2) committed against an adult by an offender with a prior sex offense conviction (i.e., recidivistic sex crimes committed against adults), 3) committed against a child by an offender without a prior sex offense conviction (i.e., non-recidivistic sex crimes committed against children), and 4) committed against an adult by an offender without a prior sex offense conviction (i.e., non-recidivistic sex crimes committed against adults).[56] The first two rates are recidivistic sex crimes potentially influenced by the spatial consequences (incapacitative and otherwise) of a residence restriction policy, while the latter two rates are non-recidivistic sex crimes potentially influenced by the general deterrence effects of a residence restriction policy.

Independent variables. The main independent variable is a monthly dichotomous indicator of whether a county (1) did or (0) did not have a residence restriction policy in

[56] These variables are measured first as crime arrest counts, and are then converted into rates using an exposure term in the model based on annual population estimates (in 10,000 residents), as of July 1st of each year, that come from the U.S. Census (2010) Population Estimates Program.

place. If residence restrictions are not associated with any of the four types of sex crime rates, as hypothesized, then the relationship between the presence of a residence restriction and the sex crime rate should be non-significant.[57] Also included in the model are two dummy variables, the first of which represents the county (and is incorporated into the setup of the fixed-effects models) and the second of which is the month-year of the observation to account for temporal influences. A third control variable is a county-specific trend variable.[58] [59] Additionally, measures of two series of monthly crime rates which are theoretically unrelated to sex crimes (i.e., burglary and robbery), are included to control for any general crime rate or violent crime rate influences that could have taken place within a county (e.g., a crackdown on all crimes, a change in policing strategy). Finally, the rate of sex crimes in the preceding month is included in the model to control for any short-term autocorrelation within the series of the dependent variable. The exact measurements and data sources for each of these variables are presented in TABLE 2.

Analytical Strategy

The analyses utilize fixed-effects panel models to predict the county sex crime rate based on the presence of a residence restriction policy.[60] As there are four different measures of sex crimes, four individual models are estimated, each predicting a different type of sex crime rate. The use of a fixed-effects panel model controls for the relatively static between-county differences by comparing each county to itself over time, while

[57] If the relationship *is* significant, then depending on which types of sex crime rates are affected, this could indicate an incapacitative effect, a general deterrence effect, an increase in hardships on sex offenders, or an increase in the reporting rates of such crimes.
[58] These control variables are largely consistent with the advice given by Marvell and Moody (2008) regarding the use of cross-sectional time series data and methods to account for autocorrelation among the observations.
[59] The author would like to thank Dr. David McDowall for his helpful comments regarding this issue.
[60] Use of a model based on the rate of sex crimes is preferred over using a model based on the count of sex crimes as it accounts for year-to-year differences in the population of each county.

measuring the effect of a residence restriction policy on each of the four different types of sex crime rates.[61]

Unfortunately, data on the number of RSOs living in each county in a given month or year are not available. Thus, the model cannot control for changes in the recidivistic sex crime rate due primarily to increases in the number of RSOs in the county. However, it is expected that the proportion of RSOs in the population is fairly static *between* counties, and thus should be controlled for using the fixed-effects panel model.

Research Question 3

Research Question

The third research question is whether residence restriction policies are associated with the within or between-neighborhood spatial distribution (i.e., clustering or dispersion) of RSOs, controlling for other neighborhood demographic and social indicators related to RSO housing options.

Hypothesis

There are three competing mechanisms for how a residence restriction policy could affect the spatial distribution of RSOs. For example, since prior research has found that residence restrictions can restrict RSO housing options and *potentially* increase RSO clustering (e.g., Barnes, et al., 2009; Chajewski & Mercado, 2009; Grubesic, et al., 2007; L. A. Hughes & Burchfield, 2008; Red-Bird, 2009; Socia, in press; Zandbergen & Hart, 2006; Zgoba, et al., 2009; but see Berenson & Appelbaum, in press; Morgan, 2008; Youstin & Nobles, 2009), it seems plausible that the presence of a residence restriction

[61] More information regarding the fixed-effects panel model used in this study is provided in APPENDIX B.

may be positively associated with RSO clustering either within individual neighborhoods (i.e., RSOs living closer together), or between neighborhoods (i.e., spatially proximate neighborhoods containing similar rates of RSOs).

Alternatively, RSOs may become more dispersed (i.e., less clustered) throughout a county, especially if residence restrictions force these offenders to move from a small number of more urban neighborhoods into many surrounding suburban or rural neighborhoods less affected by such restrictions (see Berenson & Appelbaum, in press; Morgan, 2008; Socia, in press; Youstin & Nobles, 2009). Similarly, residence restrictions may result in RSOs relocating to neighborhoods or counties that do not have such laws. Either of these alternatives would likely show higher within and between-neighborhood clustering of RSOs in those neighborhoods and counties *without* residence restrictions in place, and lower within and between-neighborhood clustering in those neighborhoods and counties *with* these policies. In either case, this alternative would be indicated by a negative association between residence restrictions and both within and between-neighborhood RSO clustering.

As there are competing mechanisms for how a residence restriction might affect the spatial distribution of RSO residences, this research question tests the null hypothesis that the spatial distribution of RSOs at the neighborhood level is *unrelated* to the presence of a residence restriction policy. If the null hypothesis is rejected, then depending on the results, it could indicate that residence restriction policies are associated with either increased or decreased within or between-neighborhood clustering of RSOs.

Data and Sample

The sample for the third research question consists of data from neighborhoods (i.e., individual census block groups) in upstate New York counties.[62] [63] Neighborhood data came from the U.S. Census (2002), NYS OSOM (2010a, 2010b), the Office of Housing and Urban Development (HUD), and from prior analysis with Geoda (Anselin, et al., 2006). Data on RSO residences was first Geocoded using a three step process, and then aggregated to the neighborhood and county levels for research questions three and four, respectively. For more information regarding the Geocoding of RSO addresses in this study, see APPENDIX C.

Variables

Dependent variables. For the third research question, the three dependent variables are measures of the spatial distribution of RSO residences (i.e., RSO clustering) in each neighborhood as of September 2010. The first measure involves the average nearest neighbor distance (controlling for the physical size and population density of the neighborhood). As such, this is a measure of the *within-neighborhood* clustering of RSO residences. The second measure involves excess risk, which accounts for both population differences between neighborhoods and the number of RSOs and residents within the county. The third measure involves the Local Indicators of Spatial Association (LISA)

[62] Since the third and fourth research questions include data from the neighborhood level either directly or as an aggregation, the inclusion of the large number of extremely urban neighborhoods from counties in the NYMSA may overshadow conclusions regarding neighborhoods and counties in the rest of the state (i.e., upstate New York). This is because neighborhoods in the NYMSA region are much more dense and numerous (per county) than in the rest of the state and therefore are much more likely to both be affected by a residence restriction policy and to influence the results on a county-by-county basis (see Socia, in press). As such, the sample is restricted to the upstate New York area for these latter two research questions.

[63] The census block group is used as a proxy for a neighborhood, as it represents the smallest aggregate area which includes the relevant Census data required for the analysis, while allowing for more diversity between neighborhoods than if census tracts were used instead (see Goodman, 1977; Socia, in press). Using census aggregations (i.e., blocks, block groups, or tracts) as a proxy for neighborhoods also has an established precedence in the existing literature on sex offender residence restrictions (Barnes, et al., 2009; Grubesic, et al., 2007; Red-Bird, 2009; Schiavone & Jeglic, 2009; Socia, in press; Zgoba, et al., 2009).

value (see Anselin, 1995). The latter two measures are both indicators of the *between-neighborhood* clustering of RSO residences. Using three separate measures helps to ensure that results are consistent regardless of the way the spatial distribution of RSOs is measured, and can inform on mechanisms that could influence within and between-neighborhood clustering differently. These three measures are examined and compared using separate models. Each measure is outlined in more detail below

The first model examines the neighborhood mean of the mean distance, in meters, of the five nearest RSO neighbors for all RSOs within that neighborhood (i.e., a neighborhood nearest neighbor analysis or neighborhood NNA). The model also includes a control for the population density of the neighborhood, as that likely influences how close together RSO residences are located. By limiting the nearest neighbor analysis to the five nearest neighbors, this helps to ensure that the value for each neighborhood is influenced by a limited number of nearby RSOs, rather than the entire population of RSOs within a county. This also helps limit the extent that variations in the overall physical county size will affect this measure. Because this measure is not available in neighborhoods that do not have any RSO residences, the model only includes the 1,085 neighborhoods containing at least one RSO residence.[64]

The second model examines the neighborhood excess risk of RSO residences (see Grubesic, 2010). Specifically, this measure is the ratio of the rate of RSOs per capita in a neighborhood compared to the expected rate of RSOs per capita. This expected rate is calculated using the average rate of RSOs per capita in the entire county, multiplied by

[64] One of the downsides of this measure is that it does not provide any results for neighborhoods that do not contain any sex offender residences. However, since within-neighborhood RSO clustering would not be a concern for neighborhoods without any sex offenders, the results should be more relevant for those neighborhoods that are actually affected by such clustering.

the resident population of the specific neighborhood in question. Thus, the denominator assumes a homogenous distribution of RSOs across all neighborhoods in a county based on the population distribution among those neighborhoods. see APPENDIX D for more information regarding the calculation of excess risk.

The third model examines the neighborhood LISA value of the rate of RSOs, controlling for the distribution of residents (see Anselin, 1995). In this study, the LISA value is essentially a measure of 'hot spots' for groups of neighborhoods that have similar rates of RSO residences, after controlling for the neighborhood population. LISA values are given as standard deviations from their overall mean, and are based on comparing the Moran's I value of a given area to the hypothesized Moran's I value using a Monte Carlo randomization procedure (see Anselin, 1995). Higher values indicate positive spatial clustering of neighborhood RSO distributions (i.e., nearby neighborhoods with very similar RSO rates), lower values indicate negative spatial clustering (i.e., nearby neighborhoods with dissimilar RSO rates), and near-zero values indicate no spatial association.

Independent variables. The main independent variable is a dichotomous indicator of whether the neighborhood was (1) or was not (0) subject to a county or local-level residence restriction policy as of December 2009. To account for potential differences in the exposure time of a residence restriction policy on RSO housing options, an alternative continuous measurement that consists of the number of months that a neighborhood was subject to a county or local-level residence restriction policy prior to December 2009 is

substituted in a comparative model.[65] Finally, a third model substitutes two dichotomous indicators of the size of the largest residence restriction policy the neighborhood was subject to (1000' or less, greater than 1000'), and a third dichotomous indicator of whether the neighborhood was subject to a residence restriction that included locations in its scope other than the 'typical' schools, daycares, parks, and playgrounds. Also measured in each of these models is whether a spatially proximate neighborhood had an existing county or local-level residence restriction policy as of December 2009, which could have forced RSOs to relocate from nearby neighborhoods in search of housing or, alternatively, could have kept RSOs from moving into the general vicinity.

The model includes controls for neighborhood demographic and social indicators that may be associated with RSO residences. As noted earlier, these characteristics include measures of social disorganization (i.e., concentrated disadvantage, residential instability, and ethnic heterogeneity) (see Craun, in press; Hipp, et al., 2010; L. A. Hughes & Burchfield, 2008; Mustaine, et al., 2006a, 2006b; Socia & Stamatel, in press; Tewksbury & Mustaine, 2008; but see Tewksbury & Mustaine, 2006; Tewksbury, Mustaine, & Stengel, 2007), available and affordable housing (see more generally, Roman & Travis, 2004), and population density (see Socia & Stamatel, in press; Zgoba, et al., 2009).[66] The exact measurements and data sources for these variables are presented in TABLE 3.

[65] Since RSO residences were measured as of September 2010 while residence restriction legislation was measured as of December 2009, the minimum amount of time any residence restriction policy was in place in a given neighborhood or county in this model was 9 months.

[66] In order to calculate the structural social disorganization measures of concentrated disadvantage and residential instability, factor analysis is used to combine multiple individual demographic and social indicators (see TABLE 3).

Of course, whether a residence restriction affects RSO clustering can depend on whether RSOs are compliant with the restrictions. Unfortunately, these data would have to be calculated on an offender-by-offender basis due to various grandfathering provisions in residence restriction policies, specific exemptions to compliance, and other factors that cannot easily be determined for a large population of RSOs. As such, obtaining these data was not feasible, and so it is assumed for the purposes of this study that the compliance rate with residence restrictions is fairly similar between counties.

Analytic Strategy

A multivariate Ordinary Least Squares (OLS) regression model is used to determine whether the presence of a residence restriction policy is significantly associated with the spatial distribution of RSO residences, controlling for other demographic and social indicators related to RSO housing options. In order to use OLS regression, the dependent and independent variables should exhibit a fairly normal distribution with skewness near 0 and kurtosis near 3. This required modification of some of the dependent and independent variables. The exact normalizing processes are outlined below.

Normalizing the dependent variables. In order to provide a more normal distribution of the excess risk measure, and thus a better fit for OLS regression, all excess risk values were increased by 22 to provide a minimum value above zero, then logged, and then limited to values within three standard deviations of the mean. This resulted in the deletion of 84 extreme outlier neighborhoods from the model, leaving 5,923 (98.6%) neighborhoods in the excess risk models. The modified distribution of excess risk values

is dramatically less skewed compared the original distribution of excess risk values (.4 vs. 25.3, respectively), and exhibits much less kurtosis (3.1 vs. 773.1).

Similarly, in order to provide a more normal distribution all LISA values were increased by 2.5 to provide a minimum value above zero, then logged, then multiplied by 100, and then limited to values within three standard deviations of the mean. This resulted in the deletion of 43 extreme outlier neighborhoods, leaving 5,964 (99.3%) neighborhoods in the LISA models. While the modified distribution of LISA values is dramatically less skewed compared the original distribution of LISA values (4.6 vs. 50.5, respectively), and exhibits much less kurtosis (58.0 vs. 2,747.4), the modified distribution still exhibits substantial skewness and kurtosis. As this could potentially lead to inaccurate estimates of coefficients, conclusions based on the LISA models are tentative at best.

Normalizing the independent variables. In order to provide a more normal distribution, certain independent variables were transformed using offsetting, logging, and/or taking the square root. These variables included ethnic heterogeneity, housing availability, housing affordability, and population density, and the exact transformations are outlined in more detail below.

Specifically, the unmodified distribution for ethnic heterogeneity exhibited skewness of 2.92 and kurtosis of 17.07. After increasing these values by 3.0, in order to provide all positive values, and logging, the modified distribution of ethnic heterogeneity exhibits skewness of 1.83 and kurtosis of 7.49. The unmodified distribution for housing availability exhibited skewness of 2.45 and kurtosis of 10.93. After taking the square root of these values, the modified distribution exhibits skewness of .62 and kurtosis of

4.02. The unmodified distribution of housing affordability exhibited skewness of 1.55 and kurtosis of 15.31. After increasing these values by 1.0, in order to provide all positive values, and logging, the modified distribution of housing affordability exhibits skewness of -.69 and kurtosis of 9.81. The unmodified distribution of population density exhibited skewness of 14.69 and kurtosis of 559.08. After logging, the modified distribution exhibits skewness of -.44 and kurtosis of 2.03.

While the distributions of the modified variables still exhibit some skewness and kurtosis (given an ideal distribution exhibiting skewness of 0 and kurtosis of 3), the distributions are much more normal compared to the unmodified distributions of these variables. As such, the modified variables should provide for more accurate estimates of the coefficients in the OLS model.

Research Question 4

Research Question

The fourth research question is whether the spatial distribution of RSOs (i.e., spatial clustering) is associated with county-level recidivistic sex crime rates, controlling for other demographic, social, and crime indicators.

Hypothesis

Similar to the third research question, there are competing mechanisms for how the spatial distribution of RSOs could be associated with county-level recidivistic sex crime rates. For example, there is tentative evidence that clustering RSOs into specific areas, whether the result of residence restrictions or other socio-economic processes, is either the result of or a contributor to increased hardships for RSOs, and that these hardships can ultimately increase recidivism rates (and thus the rate of recidivistic sex

crimes, controlling for the number of residents in an area). However, other research finds that clustering RSOs into similar areas, especially shared living arrangements, can lead to reduced recidivism rates via the increased monitoring capabilities that result from this clustering (e.g., CDPS, 2004).

As such, this research question tests the null hypothesis that the neighborhood distribution of RSOs within a county is unrelated to the rate of recidivistic sex crimes committed against either children or adult victims at the county level, controlling for the number of RSOs residing in the county, and for demographic, social, and crime indicators potentially related to recidivistic sex crime rates. If the null hypothesis is rejected, then it could indicate that RSO clustering is associated with either increased or decreased recidivistic sex crime rates as a result of either increased hardships or increased monitoring capabilities, respectively.

Data and Sample

The sample for the fourth research question consists of county-level data for the upstate New York area.[67] These data came from the U.S. Census (2002, 2010), NYS OSOM (2010b), the NYS DCJS CCH (2010), and from previous aggregated county-level analysis (not shown) using Geoda (Anselin, et al., 2006) and ClusterSeer (TerraSeer, 2010) software packages.

Variables

Dependent variables. The dependent variables for the fourth research question are two measures of annual county-level recidivistic sex crimes in 2009. These two measures are for sex crimes committed in 2009 against 1) child victims and 2) adult

[67] The analysis is limited to the upstate New York area as the independent variables contain measures of sex offender residences in this same area.

victims by offenders with a prior sexual conviction. These dependent variables are examined in separate models.

Independent variables. The main independent variable is a county-level measure of the average neighborhood spatial distribution of RSOs as of September 2010.[68] This is measured using three separate indicators of the spatial distribution (i.e., clustering) of RSO residences within and between-neighborhoods in the county.

Similar to the third research question, the first measure of clustering is the county mean of the average nearest neighbor distance of the five nearest RSO neighbors (i.e., mean county NNA). By limiting the NNA to the five nearest RSO neighbors, this again helps to ensure that RSO clustering is measured in relation to RSOs in nearby proximity, which limits the extent that variations in the overall physical county size will have on this measure. This is essentially a measure of the within-neighborhood clustering of RSOs in a given county. In order to ease in the interpretation, this variable has been standardized to have a mean of 0 and a standard deviation of 1.

While the measure of excess risk is used at the neighborhood level to measure RSO clustering in the prior research question, a different measure must be used to measure the aggregated neighborhood clustering across an entire county. As such, this research question uses the revised index of isolation, which in this case measures how isolated (or conversely, how clustered) RSOs are in neighborhoods, aggregated for an entire county. This measure compares the probable interaction of RSOs within a neighborhood given the current distribution of RSO residences and residents to the

[68] While this measure would be more valid if it was from 2009, it represents the best available data due to the lack of historical data on RSO residences. It is not expected that the difference in temporal ordering of RSO residences and sex crime rates will result in incorrect conclusions regarding the relationship between these measures.

probable interaction of RSOs within a neighborhood if RSOs were homogenously distributed across neighborhoods within the county based on the underlying distribution of residents. This measure has also been standardized to have a mean of 0 and a standard deviation of 1, and is a measure of the between-neighborhood clustering of RSOs in a given county. For more information on calculating the revised index of isolation measure, see APPENDIX E.

Finally, the Oden's (1995) I^*_{pop} statistic is used as the third measure of RSO clustering. This measure is similar to the Moran's I statistic (or alternatively to an aggregated LISA value), but can account for differences in the underlying resident population across all neighborhoods in a county, which could ultimately affect the number of RSO residences contained in those neighborhoods.[69] This measure has also been standardized to have a mean of 0 and a standard deviation of 1, and is another measure of the between-neighborhood clustering of RSOs in a given county.

To account for higher recidivistic sex crime rates in a county stemming from a higher proportion of RSOs per person, the model includes a measure of the rate of RSOs in the entire county as of September 2010.[70] Also included in the model are other demographic and social indicators that could be associated with the recidivistic sex crime rate, including the three previously mentioned indicators of social disorganization (i.e., concentrated disadvantage, residential instability, and ethnic heterogeneity) and a measure of the county population density. To account for differences in RSO clustering related to the physical size of the county, a measure of the size of the county in square

[69] For more information on how the Oden's I^*_{pop} statistic is calculated, see Oden (1995).
[70] While it is possible that the rate of recidivistic sex crimes in 2009 could have influenced the rate of RSO residences, due to RSOs being taken out of the community following an arrest and/or conviction for another sex crime, it is unlikely that this effect would be large enough to bias conclusions, due to the relatively low rate of recidivistic sex crimes.

miles is also included in the model. Finally, to control for between-county differences in the overall crime rate unrelated to RSO clustering, the model includes the average monthly robbery and burglary crime rates in 2009. The exact measurements and data sources for each of these variables are presented in TABLE 4.[71]

Analytical Strategy

Because the dependent variables all have Poisson distributions, the analysis uses multivariate Poisson regression models to evaluate the relationship between each of the three different measures of county-level RSO clustering and the average monthly county recidivistic sex crime rates in 2009, controlling for other demographic, social, and crime indicators potentially related to the recidivistic sex crime rates (see TABLE 4).

[71] Note that the county-level crime rate measures are measured and described initially as crime counts, and are then converted into crime rates through the use of an exposure term accounting for the population in each county.

CHAPTER 4: RESULTS

This chapter presents the results for each of the analyses. The chapter is divided into four parts, each pertaining to one of the four research questions.

Research Question 1

The analysis addressing the first research question examined the county characteristics that are associated with passing county-level residence restriction policies. As the dependent variable is a dichotomous measure of whether a county had passed a residence restriction as of December 2009, a logistic regression model was used with robust standard errors to estimate the coefficients of each of the county characteristics of interest. As such, coefficients are presented in odds-ratios, and when subtracted from one and multiplied by 100, can be interpreted as the percentage change in the likelihood that a county passed a residence restriction policy by December 2009 given a one-unit increase in the independent variable.

The independent variables were separated into four different categories of county characteristics. The first category describes the geographic proximity of a county either to other nearby county-level residence restriction policies, or to local-level residence restrictions within that county. The second category describes characteristics found to be relevant to policy diffusion, namely whether the county was large (in physical size), dense (in terms of population), or wealthy (in terms of mean resident income). The third category describes the political climate of the county, and measures the percentage of registered Republican or Conservative voters, and the political competition in the county. Political competition is measured as the percentage lead that the majority party (Republican/Conservative vs. Democrat/Liberal vs. Independent/Other) had over all other

parties, as of November 2005, negatively coded so that a larger lead results in a lower value.[72] The fourth category describes the sex crime rate in the county as of 2004.[73] Descriptive statistics for these variables are given in TABLE 5, and a correlation matrix is provided in TABLE 6.

The results of the analyses are given in TABLE 7. For the analyses, coefficients were first estimated using logistic regression with robust standard errors. Given the relatively few observations in the analysis, a linear probability model with robust standard errors was also used for comparison purposes. However, since the linear probability model's results and the logistic model's results were fairly similar, the low number of observations does not appear to be a fatal flaw. Specifically, only one variable (having a prior residence restriction in a nearby county) was significant in the logistic model and was non-significant in the linear probability model. This discrepancy is explained in more detail below.

Geographic Proximity

Results indicated that counties that were geographically proximate to a nearby county-level residence restriction policy were less likely to pass their own residence restriction policy compared to other counties. In the logistic model, counties with neighbors who passed a residence restriction policy were between 76 and 79 percent less likely to pass their own residence restriction. In the linear probability model, these counties were 24 percent less likely to pass a residence restriction policy (i.e., had a

[72] November 2005 is an ideal time period for voter registration data to be collected, as it was just as county and local residence restrictions were starting to be passed in New York.
[73] Using the rate of sex crimes committed only against children, rather than against both children and adults, did not influence any conclusions.

likelihood that was .24 lower) than counties without a nearby county residence restriction policy.

While this result was significant in both of the logistic models (p = .04 for both models), it was non-significant in either of the linear probability models (p = .09 and p = .10) when using an alpha level of .05. However, given the size of the effect in the logistic model, and the similarity between the logistic and linear probability model, it seems likely that the non-significant finding in the logistic model is the result of Type II error. Thus, being geographically proximate to another county-level residence restriction policy appears to *reduce* a county's likelihood of passing their own residence restriction. This goes against the 'domino effect' that other scholars have predicted (e.g., Council of State Governments, 2008; Levenson, 2009; Yung, 2007; Zgoba, et al., 2009), at least in terms of an effect on geographically proximate counties. However, this finding does indicate a type of *negative* domino effect, in that nearby jurisdictions may be *dissuaded* from passing a residence restriction policy.

One explanation may be that when a county is subjected to the various negative and unintended consequences that come with the passage of a residence restriction, nearby counties become aware of these unintended consequences and are thus dissuaded from passing their own policies. This may also apply at the local jurisdiction level as well. For example, the city council of Lebanon, Ohio recently postponed voting on a residence restriction policy after two civil lawsuits were filed contesting a similar policy in the nearby city of Greenville (McClelland, 2011). Given the potential costs associated with defending against such lawsuits and the increased burden on police tasked with enforcing the policy, both of which could lead to increased taxes for residents, the

existence of a 'negative domino effect' seems a reasonable outcome. Additionally, as this finding is an aggregate effect for county legislation, it does not mean that there isn't a domino effect occurring for *some* local jurisdictions.

Having local jurisdictions with their own residence restriction policies did not significantly affect a county's likelihood of passing a residence restriction policy. This finding again runs counter to contentions of a domino effect of such legislation. This may be due to reluctance on the part of county policymakers to entangle themselves in the legal and logistic issues involved with implementing a county residence restriction policy after noticing the troubles that local-level jurisdictions must deal with concerning their own policies. However, this finding may also reflect that local jurisdictions pass their own residence restrictions to augment an existing county policy seen as being too lenient, as appears to be the case for the town of Huntington, NY (see Morris, 2007), the city of Long Beach, NY (see Bain, 2006), and jurisdictions in and around Warren County, Ohio (see McClelland, 2011). If this were indeed the case, then a future study on local-level residence restrictions should indicate that an existing county policy increases the likelihood of a local jurisdiction passing their own policy.

Diffusion of Innovations

Results indicated that the physical size of the county did not have a significant effect on the passage of a residence restriction policy in either the logistic or the linear probability models. Similarly, mean resident income did not have a significant effect on the passage of a residence restriction policy in either model. Since population density had a high negative correlation with the percentage of Republican or Conservative registered voters ($r^2 = -.83$), including both variables in the same model could have led to

collinearity issues and problems with estimating coefficients. Therefore, both the logistic and linear probability models were first estimated with the population density variable (model 1), and then estimated again with the percentage of Republican or Conservative registered voters substituted (model 2) to see if this substitution changed any conclusions.

Results indicated that population density (measured as the log of the number of residents per square mile of land area) was not significantly related to the passage of a county residence restriction policy under either the logistic or linear probability models. Further, conclusions about all other variables were unchanged from those based on models incorporating the percentage of Republican or Conservative registered voters. Thus, residence restrictions do not appear to be adopted in a similar manner as other policy innovations, perhaps because of their highly political nature or the legal and logistic implications they have for adopters of such policies. It may also be because much of the diffusion of policy innovations literature has focused on states rather than counties, and the passage of county legislation has its own unique dynamics. Further county-level studies on other criminal justice policies would help to confirm this hypothesis.

Political Climate

Although results were in the expected (positive) direction, counties with more Republican or Conservative registered voters were not significantly more likely to have passed a residence restriction policy, which goes against the hypothesized relationship. This may have been due to the inclusion of political competition in the model, as noted below, or because residence restrictions are an issue that both parties are willing to exploit to further their own political ambitions, or at least are unwilling to argue against

once they are proposed. In fact, this explanation is supported by the findings on political competition, as noted below.

Perhaps the most interesting finding is that increased political competition significantly increased the likelihood that a county had passed a residence restriction policy. That is, counties where the majority party had a lower proportion of registered voters compared to all other parties (i.e., higher competition) were *more* likely to have passed a residence restriction policy. Specifically, for every percentage increase in political competition (i.e., a percentage decrease in the size of the majority party's registered voter base compared to other parties), a county was about eight percent more likely to have implemented a residence restriction policy according to the logistic model.[74] In the linear probability model, every percentage increase in political competition was associated with a one percent increase in the likelihood of passing a residence restriction policy.

This finding indicates that in counties where the majority party had less political power and stability (in terms of a small base of registered voters in the party), politicians may have felt more pressure to pass residence restriction policies to gain voter support. As such, politicians may be using the passage of a residence restriction policy as a bargaining tool to appease voters and subsequently increase their political power. In fact, the executive director of the advocacy organization Parents for Megan's Law acknowledged that residence restrictions in some New York jurisdictions were likely

[74] It is important to note that the percentage of a majority party's lead does not necessarily have to be a positive number, and cannot be if the majority party contains less than 50 percent of registered voters in a county. For instance, if the Republican/Conservative party contained 45 percent of the registered voters in a county, and that was the largest single group of voters (as compared to Democrat/Liberal or Independent/Other parties), then the majority party's lead would be -10 percent (i.e., 45 percent in the Republican/Conservative party compared to 55 percent in all other parties combined).

enacted out of political concerns (Nahas, 2005), a view supported by cases all over the country.

For example, a member of the California State Senate noted that sex offender penalties and residence restrictions were dominant political issues in a recent campaign (Gardner, 2010), and the California Sex Offender Management Board (2010; see also Shih, 2010) acknowledged that many existing sex offender policies were passed for political reasons rather than to increase community safety. Regarding California's Proposition 83, which implemented a 2,000-foot residence restriction policy statewide, Berkeley law professor Franklin Zimring noted that it was "almost completely symbolic" and was proposed "for political gain" (Shih, 2010, p. A31A(L)).

In Iowa, the executive director of the state chapter of the American Civil Liberties Union noted that while "herding former [sex] offenders into penal colonies may help get politicians re-elected," the state's recently enacted residence restriction law was "poorly conceived and illogical" (Stone, 2003, p. 1). Perhaps stated most directly, in response to two state bills related to sex offenders in Illinois, a corrections reform advocate lamented that "you just can't expect reasoned sex offender policies from legislators in an election year" (Yeagle, 2010, p. 1).

Given these findings and examples from across the country, it is not surprising that politicians continue to propose and pass residence restrictions despite the lack of evidence that they are effective at reducing recidivistic sex crimes. This is similar to the findings of the broader literature on the passage of legislation in response to political pressures, as well as the politicization of crime and crime policy (see Scheingold, 1984). Specifically, the 'problem' of sex offenders living in the community represents a conflict

with other residents. As noted by Schattschneider, "All forms of political organizations have a bias in favor of the exploitation of some kinds of conflict and the suppression of others because organization is the mobilization of bias" (1960, p. 71). Thus, politicians appear to be exploiting the conflict between sex offender housing and residents' fears through the use of residence restriction policies.

Sex Crime Rate

After including all other characteristics in the model, results indicated that counties with higher sex crime rates were not significantly more (or less) likely to have passed a residence restriction policy. This is an interesting finding on its own, as it supports the idea that residence restriction policies are implemented for reasons other than crime control, such as political maneuvering in tight elections. Further, if the counties that pass residence restrictions are not suffering from the 'problem' of high rates of sex crimes, the ability of these policies to make a significant and sizable difference once they are in place already comes into question. However, it is important to remember that the *perceived* threat of sex crimes in a county was not measured, and this may not necessarily be related to the *actual* threat of sex crimes. Further, these policies may be passed in response to the perceived threat of sex offenders clustering in a given section of town or when a single sex offender moves across the street from a school or daycare (see Bain & German, 2006; Shih, 2010). Case studies of individual jurisdictions would be required to further examine the influence of perceived threats on the passage of residence restrictions.

Summary of Results for Research Question 1

The results indicated that the two county characteristics significantly associated with passing a residence restriction policy were geographic proximity to another county-level residence restriction and political competition. Geographic proximity was negatively related to the likelihood of passing a residence restriction policy, while political competition was positively related. Further, the rate of sex crimes was not significantly related to the passage of a residence restriction policy, questioning whether these policies are being implemented in the places that may be the most justified in using them.

Overall, these findings suggest that residence restrictions are being used as bargaining chips by politicians in counties with stiff political competition. Further, there is no domino effect for passing these restrictions from county to county. In fact, results indicate that when a county passed a residence restriction, neighboring counties were *less* likely to pass their own policies. This is possibly because policymakers could see their neighbors dealing with the various legal and logistic challenges involved in implementing such a policy, and decided it was simply not worth the trouble.

Research Question 2

The second research question examined was whether county-level residence restriction policies affected county-level sex crime rates. Four different types of sex crimes were analyzed, with each type of sex crime involving a unique combination of either child or adult victims and either offenders with or offenders without prior sexual convictions. These results are presented in two sections based on whether the sex crime under examination was likely to be affected through either an incapacitative or a general deterrence process, depending on whether the individuals under consideration did or did

not have a conviction for a prior sex crime, respectively. A conviction for a prior sex crime was taken as a proxy for inclusion on the state sex offender registry and for being subject to a residence restriction policy, if one was in place in the county of residence. Based on limited prior research, it was hypothesized that the presence of a residence restriction would not have any effect on any of the rates of sex crimes in a county.

For each unique type of sex crime, controls were included in the model for temporal lag, trends in other non-violent (i.e., burglary) and violent (i.e., robbery) crime rates, temporal and county-specific trends, and changes in the county population. Descriptive statistics are presented in TABLE 8, while a correlation matrix is presented in TABLE 9. The correlation matrix indicates that multicollinearity is unlikely to present a problem for evaluating the relationship between residence restrictions and various sex crime rates at the county-month level. Results are presented in TABLES 10 through 13, with coefficients presented as odds-ratios.

Incapacitative or Specific Deterrence Effects

After including controls in the models, results indicated that the implementation of a county-level residence restriction policy did not significantly decrease the rate of sex crimes committed against either child or adult victims by previously convicted RSOs. Specifically, while a county-level residence restriction policy was associated with 6 percent fewer sex crimes committed against child victims and 9 percent fewer sex crimes committed against adult victims (TABLES 10 and 11, respectively), neither of these decreases reached statistical significance at an alpha level of .05. These findings are consistent with the existing literature indicating that these policies are unlikely to have any effect on recidivistic sex crimes (e.g., Blood, et al., 2008; CDPS, 2004; Duwe, et al.,

2008; MNDOC, 2003; 2007; Zandbergen, et al., 2010). These findings are also not surprising, considering that residence restrictions seek to increase the spatial distance between RSOs and pools of potential *stranger* victims, whereas most sex crimes occur between *acquaintances* (Greenfield, 1997; Snyder, 2000). Thus, the *relational* distance between RSOs and potential victims is likely more important than the spatial distance, and these results suggest that residence restrictions may not be affecting RSOs' abilities to form such relationships with potential victims.

However, there are alternative explanations as to why residence restrictions are not effective at incapacitating RSOs. One explanation may be due to the relatively low rate of recidivism for RSOs. Specifically, it may be that the RSOs most likely to reoffend are willing to go to great lengths to find suitable targets, regardless of their place of residence in relation to areas where children congregate.

Another explanation may be due to the relatively low compliance rates with residence restrictions and registration requirements. While little current data exist as to actual compliance rates after accounting for grandfather clauses or overlapping restrictions, research has found compliance rates with residence restrictions to be around 50 percent or less in some areas (e.g., Berenson & Appelbaum, in press; Tewksbury & Mustaine, 2006; see also Youstin & Nobles, 2009).

Finally, there is the possibility that this finding may be an effect of the relatively low rate of occurrences of recidivistic sex crimes at the county-month level. These low base rates could result in large standard errors and the increased potential for Type II errors. However, given that the data used in the analyses represent the best available

data, it is of little use to second guess the significance of these results without the existence of a suitable alternative source of data.

Control variables. Other than the constant term, none of the control variables reached significance at an alpha level of .05

General Deterrence Effects

After including controls in the models, results indicated that the implementation of a county-level residence restriction policy did not significantly decrease the rate of sex crimes committed against child victims by individuals without prior sexual convictions. Specifically, a county-level residence restriction policy was associated with 3 percent fewer sex crimes committed against child victims (TABLE 12), which did not reach statistical significance at an alpha level of .05. This indicates that the potential punishment implied by these policies did not generally deter non-RSO individuals seeking to commit sex crimes against children.

However, after including controls in the model, results indicate that the implementation of a county-level residence restriction policy *did* yield a significant decrease in the rate of sex crimes committed against adult victims by individuals without prior sexual convictions. Specifically, a county-level residence restriction policy was associated with a 10 percent decrease in non-recidivistic sex crimes committed against adult victims, which was significant at an alpha level of .01 (TABLE 13). This suggests that the increased punishment these policies provide for the first conviction for a sex offense may have a general deterrence effect for individuals who are likely to sexually victimize adults but who have not yet been caught and convicted for a sex crime. This finding is also consistent with the limited research that finds the threat of formal

sanctions can deter rape (Bachman, Paternoster, & Ward, 1992), and that post-release sex offender policies can have a general deterrence effect (Letourneau, Levenson, Bandyopadhyay, Armstrong, & Sinha, 2010; but see Sandler, Freeman, & Socia, 2008).

Unfortunately, it remains unclear as to why residence restrictions significantly deter sex crimes committed against adult victims but not those committed against child victims. However, there are some potential explanations that may be confirmed with future research on the decision making process of potential sex offenders.

One such explanation may lie in the differences between pedophiles and adult rapists. An individual is considered to be a pedophile when they are sexually attracted to children (typically 13 years or younger) at least 5 years younger than themselves and are at least 16 years old (American Psychiatric Association, 2000; Fagan, Wise, Schmidt, & Berlin, 2002).[75] As such, in order to satisfy such specialized sexual urges, a pedophile must commit some form of sex crime involving children (e.g., viewing child pornography, voyeurism involving children, actual physical child molestation). Conversely, non-pedophiles may satisfy sexual urges through either legal (e.g., sex with a consenting adult, viewing legal pornography) or illegal means (e.g., patronizing a prostitute, voyeurism, rape). Thus, potential adult rapists may be more easily deterred simply because they have more legal ways in which to satisfy their sexual urges compared to potential child molesters. This explanation, however, does not account for research that finds the majority of rapes are committed not out of sexual frustration, but rather as a means of power and/or control over the victim (see Robertiello & Terry, 2007). In any event, to confirm this explanation, data would be required on the

[75] Note that not all child molesters are pedophiles, just as not all pedophiles are child molesters. However, in terms of likely crimes involving direct contact with victims, pedophiles are more likely to molest children than to rape adults due to their specific age preferences.

individual decision-making processes of *potential* rapists and *potential* child molesters (i.e., those who do not yet have prior sexual convictions), which would be extremely difficult to obtain.

Another explanation may be due to the 'victim grooming' habits of child molesters. Specifically, child molesters are frequently known to have formed relationships with their victims in order to groom them before committing a molestation, which may involve a lengthy process and may result in continued victimized of the same child over time (see Freeman, 2007; Pryor, 1999; Terry & Tallon, 2004). Thus, the passage of a residence restriction policy may come midway through the grooming process for a potential (or uncaught) child molester, and thus may have less of a general deterrent effect than for a potential rapist or child molester who has *not yet* begun seeking out potential victims. Given a longer post-restriction period than is used in the current study, this effect would be supported by a gradual decrease over time in the rate of non-recidivistic sex crimes committed against child victims. This would result from the proportion of non-RSO individuals that were already involved in grooming or child molestation when the policy was passed decreasing due to either arrest or natural desistence, thereby increasing the proportion of 'new' potential child molesters who had not yet starting the grooming process at the time of policy's passage, and thus whom were potentially more easily deterred.

Control variables. The only control variable to reach significance at an alpha level of .05 was the lag measure in the model examining non-recidivistic sex crimes committed against adults ($p < .001$). This finding is not surprising, and indicates only

that the rate of non-recidivistic sex crimes committed against adults has temporal autocorrelation with the rate of such crimes in the prior month.

Summary of Results for Research Question 2

Overall, the results indicated that county-level residence restriction policies did not have any significant effects on the rate of sex crimes committed either against child or against adult victims by individuals with prior sexual convictions. This finding is consistent with the hypothesis that these policies do not affect recidivistic sex crimes, and may be because most recidivistic sex crimes do not occur in a manner consistent with the spatial-proximity assumptions that residence restrictions are largely based on. Similarly, these policies did not have a significant effect on the rate of sex crimes committed against child victims by individuals without prior sexual convictions, which is again consistent with the hypothesis of no effect.

However, these policies were associated with a ten percent decrease in the rate sex crimes committed against adult victims by individuals without prior sexual convictions, which was significant at an alpha level of .05. This finding rejected the null hypothesis, and suggests that the punishment implied by these policies might be generally deterring some individuals, who are not yet RSOs, from committing a sex crime against an adult victim. Although potential explanations were provided for why non-recidivistic sex crimes committed against adult victims were deterred, while those committed against child victims were not, the current dataset does not allow for further exploration and confirmation of why this difference exists.

Research Question 3

The third research question pertained to whether county and/or local residence restriction policies were associated with the neighborhood spatial distribution (i.e., clustering) of RSO residences, controlling for other neighborhood demographic and social indicators related to RSO housing options. Three different models of RSO residential clustering were analyzed as of September 2010: 1) a measure of the neighborhood mean of the mean distance of neighborhood RSOs' nearest five neighbors (neighborhood NNA), 2) a measure of the excess risk of RSO residences in the neighborhood, and 3) a measure of the LISA value of neighborhood RSO clustering. Due to competing mechanisms for how residence restrictions could affect the within and between-neighborhood distribution of RSOs, the null hypothesis was that residence restrictions were unrelated to the spatial distribution of RSOs.

These results are presented in three sections, each examining an individual clustering measure. For each model, the independent variables measured the presence of a residence restriction policy, the presence of a nearby residence restriction policy, concentrated disadvantage, residential instability, ethnic heterogeneity, housing availability, housing affordability, and population density. The first set of models for each of the three measures of RSO clustering included only a measure of whether the neighborhood did or did not have an existing county or local-level residence restriction as of December 2009. Subsequent models added the other independent variables in three successive groups (nearby restrictions, social disorganization, and housing/population measures).

This final dichotomous model was then compared to two alternative models. The first alternative model (model 2) replaced the dichotomous measure of a residence

restriction with a measure of the number of months that the neighborhood had been subject to any type of residence restriction policy. This model accounts for differences in residence restriction exposure time between neighborhoods. The second alternative model (model 3) replaced the single dichotomous measure with two dichotomous measures representing the maximum size of any of the residence restrictions applying to the neighborhood (1000' or less, greater than 1000'), and a third dichotomous measure of whether any of the residence restrictions applying to the neighborhood included locations other than schools, daycares, parks, and playgrounds in their scope. These four locations are typically included in some combination in most residence restriction policies, and thus including other types of locations (e.g., malls, beaches, arcades, etc.) represents a much more comprehensive (and potentially more restrictive) scope.

Descriptive statistics are presented in TABLE 14, while correlation matrixes of the variables included in each of the analyses are presented in TABLES 15 through 17. These correlation matrixes indicate that while some of the control variables have strong correlations (r >.60), none has a correlation at or above .80, the general rule of thumb for potential issues with multicollinearity.[76] Thus it is unlikely that multicollinearity would present a problem for these models. Results for each of the models are presented in TABLES 18 through 23, and are described below.

Nearest Neighbor Distance

After all independent variables were included, the final model explained 38 percent of the variation in the neighborhood NNA for neighborhoods which contained at least one RSO (N = 1,085) (TABLE 18). While an existing residence restriction policy

[76] The r = .80 rule of thumb for potential issues with multicollinearity has been noted in a number of sources, including statistics texts by Allison (1998) and Knoke and colleagues (2002).

was associated with an *increase* in the distance between the RSOs and their five nearest RSO neighbors (i.e., increased dispersion), it was non-significant in the final model given an alpha level of .05.[77] Conversely, while a nearby residence restriction policy was associated with a *decrease* in the distance between the RSOs and their five nearest RSO neighbors (i.e., increased clustering), it was also non-significant. Both of these results are consistent with the hypothesis, and indicate that the spatial distribution of RSOs within an individual neighborhood is unlikely to be influenced by a residence restriction.

In fact, the only characteristics significantly associated with the distance between the RSOs and their five nearest RSO neighbors were the residential instability and population density of a neighborhood. Specifically, neighborhoods exhibiting more residential instability and those which contained more residents per square mile had RSOs that lived significantly closer together (i.e., were more spatially clustered). The latter makes intuitive sense, as more people per square mile means residents are living closer together spatially. When combined with the residential instability measure, it suggests RSOs are clustering in the neighborhoods offering the most available and affordable housing, which are the most dense, urban neighborhoods in a county. However, individual housing unit data would be required to support this theory.

Measuring an existing residence restriction by the number of months it had been in place in the neighborhood as of December 2009 yielded different results from those using the single dichotomous measure (see TABLE 19). Specifically, the longer a neighborhood had been subject to a residence restriction, the smaller the distance between RSOs' five nearest neighbors was expected to be. Thus, it appears that exposure

[77] However, it was significant at an alpha level of .06, and thus it is possible that the strict lack of significance is simply a Type II error.

to a residence restriction leads to increased clustering within a neighborhood over time, perhaps as RSOs move into, out of, and within the neighborhood and slowly increase the aggregate compliance with the policy's restricted housing. Most of the associations and significance of the other variables in this model were similar to the first model, although the proximity to a nearby residence restriction changed from a non-significant negative association in the dichotomous model to a non-significant positive association in this model.

Measuring an existing residence restriction by its size and scope yielded similar results to those using the single dichotomous measure, and was consistent with the hypothesis of no effect (see TABLE 19). Specifically, while both of the dichotomous size indicators were positively associated with the distance between RSOs' five nearest neighbors, both were non-significant at an alpha level of .05. The measure for the inclusion of other locations in the residence restriction's scope was also positively associated but non-significant. Associations and significance for all of the other variables in the analyses were also similar between the two models.

Excess Risk

After all independent variables were included, the final model explained 12 percent of the variation in the logged measure of the neighborhood excess risk of having RSOs living in the neighborhood (N = 5,912) (TABLE 20).[78] While an existing residence restriction policy was associated with an *increase* in the logged excess risk, it

[78] In addition to the 84 neighborhoods removed from the final model for falling outside of 3 standard deviations of the mean, 11 additional neighborhoods were removed because they lacked the HUD housing market data used to calculate housing affordability. These excluded neighborhoods represent a very small proportion of the overall sample, and thus are not expected to have influenced conclusions.

was non-significant in the final model given an alpha level of .05.[79] Conversely, while a nearby residence restriction policy was associated with a *decrease* in the logged excess risk, it *was* significant at an alpha level of .05. Thus, when there was a nearby residence restriction policy, RSOs were significantly less likely to live in that neighborhood than was otherwise expected, even after controlling for whether that neighborhood was also subject to a residence restriction policy. This finding indicates that RSOs may have sought housing in neighborhoods that were farther away from existing residence restrictions, creating 'zones' of neighborhoods which contained lower than expected RSO rates that were located *around* neighborhoods that had residence restrictions in place. As such, the benefits of a residence restriction (i.e., fewer RSOs in the neighborhood) could have diffused to nearby neighborhoods that did not have their own restrictions. This is, of course, assuming that having fewer RSOs than expected is considered a perceived benefit for a neighborhood. Individual-level data on the movement of RSOs between neighborhoods would be required to confirm this theory.

Interestingly, all of the other neighborhood characteristics in the model were significantly associated with the logged excess risk measure. Specifically, neighborhoods that exhibited characteristics associated with social disorganization, housing availability, housing affordability and/or increased population density contained more RSOs than would otherwise have been expected given a homogenous distribution of RSOs among neighborhoods based on the population of neighborhood residents. This indicates that a number of neighborhood characteristics are associated with RSO residences aside from the presence of a residence restriction. Thus, RSOs may be selecting (or forced into)

[79] However, similar to the neighborhood NNA results, it was significant at an alpha level of .06. Thus, it is possible that the lack of significant is simply a Type II error.

neighborhoods with certain characteristics that are unrelated to the amount of restricted housing those neighborhoods contain.

When an existing residence restriction was measured as the number of months it had been in place in the neighborhood as of December 2009, the model yielded similar results compared to those using the single dichotomous measure (TABLE 21). That is, the length of time a neighborhood had been subject to a residence restriction was positively associated with the logged excess risk measure, but was non-significant at an alpha level of .05. This supports the hypothesis that a residence restriction does not affect the between-neighborhood spatial distribution of RSOs. While most associations and the significance of the other variables in this model were similar to the first model, proximity to a nearby residence restriction policy did not reach significance in this model. This could be because RSOs learn, over time, where the unrestricted housing options are in various neighborhoods (regardless of proximity to a nearby residence restriction). Therefore, even though the implementation of a nearby residence restriction could initially cause RSOs to seek housing elsewhere, they are not kept away from these neighborhoods for very long.

Measuring an existing residence restriction by its size and scope yielded similar results to those using the single dichotomous measure, and was consistent with the hypothesis of association (see TABLE 21). Specifically, although a smaller residence restriction (1000' or less) was negatively related with the logged excess risk, and a larger residence restriction (over 1000') was positively related, neither was significant at an alpha level of .05.[80] Interestingly, including other locations in the residence restriction's

[80] Since this lack of significance is consistent with the model using the single dichotomous measure, it helps dismiss concerns that these results are merely a Type II error.

scope was positively associated with logged excess risk and was significant at an alpha level of .001. This indicates that when additional locations are included in a residence restriction policy's scope, there in an increase in RSOs living in the neighborhood, controlling for the size of the restriction.

These findings may seem counter intuitive, since one might expect more comprehensive residence restrictions to generate more restrictive housing options for RSOs, thus driving them out of the neighborhood and into less restrictive neighborhoods. However, it could be that those neighborhoods that contained a disproportionate number of RSOs were also the most likely to have passed a very comprehensive residence restriction. Such a restriction may have been seen by policymakers as a way to 'fix' the perceived problem of having too many RSOs. If this was the case, then even if RSOs were more likely to move out of such neighborhoods, if the rate of RSOs was disproportionately higher *prior* to the restriction, the resulting rate of RSOs in such neighborhoods may still have been disproportionately higher *after* the restriction, even accounting for an overall decrease. Unfortunately, without historical data on the number of RSOs in each neighborhood prior to a restriction for comparison purposes, this theory cannot be confirmed.

The associations and significance of the other variables were similar between the other models, and when not considering significance, all of the relationships are in the expected direction given an expectation that RSOs cluster in very urban (i.e., dense), socially disorganized neighborhoods that offer available and affordable housing.

LISA Value

After all independent variables were included, the final model explained just 3 percent of the variation in the logged LISA value of a neighborhood's rate of RSOs (N = 5,953) (TABLE 22).[81] While an existing residence restriction policy was associated with a slight *decrease* in the logged LISA value, it was non-significant given an alpha level of .05 in the final model. Similarly, while a nearby residence restriction policy was also associated with a *decrease* in the logged LISA value, it was also non-significant at an alpha level of .05. However, neighborhoods that exhibited more ethnic heterogeneity, more housing availability, and more population density all had higher levels of logged LISA values (i.e., more spatial clustering). This is consistent with prior research on RSO housing, and indicates that RSOs are more likely to live in urban neighborhoods that offer available housing but exhibit little social control (e.g., Barnes, et al., 2009; Craun, in press; Grubesic, 2010; Grubesic & Murray, 2008, in press; Grubesic, et al., 2008; L. A. Hughes & Burchfield, 2008; L. A. Hughes & Kadleck, 2008; Mustaine & Tewksbury, 2008; Mustaine, et al., 2006a, 2006b; Red-Bird, 2009; Socia & Stamatel, in press; Tewksbury, 2007; Tewksbury & Mustaine, 2008; Turley & Hutzel, 2001; but see Tewksbury and Mustaine, 2006; Youstin & Nobles, 2009).

Measuring an existing residence restriction by the number of months it had been in place in the neighborhood as of December 2009 yielded different results from those using the single dichotomous measure (see TABLE 23). Specifically, the longer a neighborhood had been subject to a residence restriction, the lower the expected logged LISA value (i.e., reduced clustering). Thus, a residence restriction was associated with

[81] In addition to the 43 neighborhoods removed from the final model for falling outside of 3 standard deviations of the mean, once again 11 additional neighborhoods were removed due to a lack of HUD housing market data. This again represents a very small proportion of the overall sample, and thus is not expected to have influenced conclusions.

decreased between-neighborhood clustering over time, which could have been the result of RSOs 'diffusing' from very restricted urban neighborhoods into less restricted, more rural neighborhoods, or because RSOs were better able to diffuse among different neighborhoods as they learned, over time, where unrestricted housing could be found. This theory would also be consistent both with the findings of the excess risk measure covered previously, and with the findings of Youstin and Nobles (2009).

The associations and significance of most of the other variables in the analyses were similar to the first model, although proximity to a nearby residence restriction changed from a non-significant negative association in the dichotomous model to a non-significant positive association in this model.

Measuring an existing residence restriction by its size and scope yielded slightly different results compared to those using the single dichotomous measure, although the amount of variation explained by each model was similar (see TABLE 23). Specifically, while both sizes of residence restrictions were negatively related to the logged LISA value, only the smaller restriction (1000' or less) was significant at an alpha level of .05. This indicates that small residence restrictions lead to significantly decreased logged LISA values (i.e., reduced clustering of neighborhoods based on RSO rates), compared to either no restrictions or large restrictions. These results are consistent with some prior studies of RSO clustering over time (e.g., Morgan, 2008; Youstin & Nobles, 2009). Thus, a small residence restriction may have resulted in some neighborhoods having very restricted housing, such as those which contain a school or daycare, while other nearby neighborhoods had much less restricted housing, and thus more RSOs living there. If this were the case, then RSOs might be spatially distributing themselves randomly into the

less restrictive neighborhoods that surround restrictive neighborhoods (i.e., diffusing across neighborhoods). This would also account for both the non-significant findings for the effects of a nearby residence restriction, and the decreased between-neighborhood clustering over time. A visual example of this can be seen in example C of FIGURE 2, where more restricted (lighter) neighborhoods with fewer RSOs are surrounded by less restricted (darker) neighborhoods with more RSOs.

Interestingly, residence restrictions that included other locations in their scope were positively associated with the logged LISA value, and this association was significant at an alpha level of .01. This indicates that neighborhoods with residence restrictions that include additional locations in their scope had increased RSO clustering even when controlling for the size of the restriction. This might have been because comprehensive residence restrictions ended up restricting much of the housing in large groups (clusters) of neighborhoods, forcing RSOs out of these neighborhoods in search of other groups of neighborhoods with more unrestricted housing. A visual example of this can be seen in example A of FIGURE 2, which shows large clusters of more restricted (lighter) neighborhoods containing fewer RSOs and large clusters of less restricted (darker) neighborhoods containing more RSOs.

The associations and significance of all of the other variables, and thus their interpretations and implications, were similar between both of the models examining the between-neighborhood clustering of RSOs.

Summary of Results for Research Question 3

Overall, the results indicate that the presence of a residence restriction policy is not automatically associated with the *within-neighborhood* clustering or dispersion of

RSO residences. That is, in neighborhoods with a residence restriction policy, RSOs are not more or less likely to live closer to other RSOs than in neighborhoods without such policies. Additionally, when a neighborhood is next to neighborhoods that are subject to residence restrictions, RSOs within that neighborhood are not more likely to live closer together or farther apart compared to neighborhoods whose neighbors do not have such policies. However, over time a residence restriction appears to result in RSOs living closer together within a neighborhood.

Results were more mixed when examining the *between-neighborhood* clustering of RSO residences using logged excess risk. Specifically, a residence restriction was not significantly associated with having more RSOs in a neighborhood than would otherwise be expected, based on a homogenous distribution of the county's RSOs among its population of residents. However, neighborhoods subject to a restriction that had a comprehensive scope (i.e., which included non-typical locations), were expected to have higher values of logged excess risk and thus contain more RSOs than otherwise expected. This may be because areas with the most RSOs were also the most likely to pass comprehensive residence restrictions. Additionally, being spatially proximate to a neighborhood with a residence restriction policy was associated with lower values of logged excess risk. Thus, when a neighborhood is located near a residence restriction, RSOs may be less likely to live in that neighborhood for fear of being subject to a future residence restriction or the expectation of facing a harder time finding housing in that general area.

Similarly, there were mixed results when examining the neighborhood clustering of RSO rates using logged LISA values. Specifically, a residence restriction overall was

not significantly associated with more or less clustering of neighborhoods based on RSO rates (i.e., having a higher or lower logged LISA value, respectively). However, when the size of the residence restriction was examined, smaller restrictions were associated with significantly less between-neighborhood clustering (i.e., lower logged LISA values), while larger restrictions were not significantly related to clustering. This may be due to RSOs paying more attention to where they can live (and thus finding housing in many different neighborhoods) when a residence restriction is in place compared to when it is not, but also having an easier time finding such housing when the restriction is smaller compared to when it is larger.

Additionally, a comprehensive scope was associated with significantly more between-neighborhood clustering (i.e., higher logged LISA values). Again, this may be because areas with the most RSOs were the most likely to pass comprehensive residence restrictions, although this theory could not be explored in the current study (i.e., in research question 1). Being spatially proximate to a neighborhood with a residence restriction policy was not significantly associated with between-neighborhood clustering as measured by logged LISA values.

In conclusion there is no support that any type of residence restriction policy or spatially proximate residence restriction policy is always associated with RSOs living closer together (i.e., clustering) within neighborhoods. However, over an extended period of time a residence restriction policy may lead to RSOs living closer together within a neighborhood. There is only weak support that the size of a residence restriction policy increases certain types of between-neighborhood clustering; that is, when the size of the restriction is small (i.e., 1000' or less) and clustering is measured based on groups

of nearby neighborhoods having similar RSO rates (i.e., logged LISA values). Thus, residence restrictions increase the clustering of RSOs between neighborhoods only in very specific circumstances. The scope of a residence restriction policy is positively associated with between-neighborhood RSO clustering, both in terms of logged excess risk and logged LISA values. This indicates either that a comprehensive residence restriction may cause RSOs to cluster in specific neighborhoods, or that areas with very high rates of RSOs are more likely to pass such comprehensive restrictions. Finally, being spatially proximate to a nearby residence restriction policy is associated with a neighborhood having fewer RSOs than expected (i.e., a reduced logged excess risk), but not with having fewer (or more) RSOs compared to nearby neighborhoods (i.e., a non-significant logged LISA value).

Research Question 4

The fourth research question pertains to whether county RSO clustering was associated with the 2009 mean monthly rates of recidivistic sex crimes committed against either child or adult victims, controlling for other county demographic, social, and crime characteristics potentially related to these sex crime rates. Due to the competing mechanisms noted earlier, the null hypothesis for this question was that RSO clustering at the county level is unrelated to either type of recidivistic sex crime. The results are presented in two sections, the first examining the rate of sex crimes committed against child victims, and the second examining the rate of sex crimes committed against adult victims.

Each section examines three different models of RSO residential clustering as of September 2010: 1) a standardized measure of the county mean of the mean distance of

the neighborhoods' RSOs' nearest five RSO neighbors (i.e., mean county NNA), 2) a standardized measure of the revised index of isolation of RSOs in the county (R.I.I.), and 3) a measure of the Oden's I^*_{pop} value for the county. For each of the models, control variables were included to account for the rate of RSO residences in the county as of September 2010, and measures of the concentrated disadvantage, residential instability, ethnic heterogeneity, population density, physical size, 2009 mean monthly rate of robbery crimes, and 2009 mean monthly rate of burglary crimes of the county.

Descriptive statistics are presented in TABLE 24, and a correlation matrix is presented in TABLE 25. The correlation matrix indicates that while some of the control variables have strong correlations with each other ($r > .60$), none has a correlation at or above .80 with any of the other variables, and none is collinear with any of the three measures of RSO clustering that represent the independent variables of interest.[82] As such, it is unlikely that multicollinearity would present a problem for these models. The results in TABLES 26 and 27 are presented as odds-ratios using robust standard errors, and are described in more detail below.

Recidivistic Sex Crimes Against Child Victims

Mean county NNA. As shown in model 1 of TABLE 26, after all of the control variables were included the relationship between the rate of recidivistic sex crimes against child victims and the standardized mean county NNA was non-significant. Thus, having RSOs living closer to one another within the neighborhoods of a county, such as

[82] The revised index of isolation was highly collinear with the Oden's I^*_{pop} value ($r > .80$, not shown). However, as these variables were substituted for one another in each of the models, it is unlikely that multicollinearity presents a problem for any of the individual models given the size of the other correlations. Additionally, the fact that these two measures are highly collinear indicates that they are likely measuring the same construct of 'between-county RSO clustering.' It is also not surprising that neither is collinear with the mean county NNA, as this measures an inherently different construct, namely within-neighborhood RSO clustering.

when they live in certain mobile home parks, in a 'strip' of low-cost motels, or in the same or nearby apartment buildings in certain sections of a city, was not related to the rate of recidivistic sex crimes committed against children. Specifically, while a one standard deviation increase in the mean county NNA was associated with a 25.5 percent lower rate of recidivistic sex crimes committed against child victims, this relationship was non-significant given an alpha level of .05. Assuming geographic proximity increased the likelihood of associations between sex offenders, then this finding is consistent with research on sexual recidivists in Canada, which found that associating with other sex offenders was not significantly related to recidivism (Hanson & Harris, 1998). However, individual-level data on sex offender recidivism and their associations with other geographically proximate sex offenders would be required to confirm this theory.

In fact, the only significant association with the rate of recidivistic sex crimes committed against child victims involved the rate of RSO residences in the county. Specifically, an increase of one RSO per 10,000 residents was associated with a 19 percent higher rate of recidivistic sex crimes committed against child victims, and was significant at an alpha level of .05. This makes intuitive sense, as having more RSOs per capita in a county would likely mean more recidivistic sex crimes per capita.

Revised index of isolation. As shown in model 2 of TABLE 26, after all of the control variables were included the relationship between the rate of recidivistic sex crimes committed against child victims and the revised index of isolation (R.I.I.) was positive but non-significant. Thus, counties with uneven distributions of RSOs among neighborhoods, such as when RSOs clustered into neighborhoods containing the

aforementioned mobile home parks, low cost motels, or many affordable rental units, did not have higher rates of recidivistic sex crimes committed against children compared to counties with RSOs that were evenly distributed among neighborhoods. Specifically, while a one standard deviation increase in the R.I.I. was associated with a 19 percent higher rate of recidivistic sex crimes committed against child victims, this relationship was non-significant given an alpha level of .05. Once again, the only significant relationship involved the rate of RSO residences in the county, and this was comparable in both size and significance to that of the mean county NNA model.

Oden's I^*_{pop}. As shown in model 3 of TABLE 26, after all of the control variables were included, the relationship between the rate of recidivistic sex crimes committed against child victims and the Oden's I^*_{pop} value was negative but non-significant. Thus, counties with neighborhoods that were geographically clustered based on the rate of RSOs per capita were not more likely to have higher or lower rates of recidivistic sex crimes committed against children. Specifically, while a one standard deviation increase in the Oden's I^*_{pop} was associated with a nine percent higher rate of recidivistic sex crimes committed against child victims, this relationship was non-significant given an alpha level of .05. Again, the only significant relationship involved the rate of RSO residences in the county, and was similar in size and significance to that of the previous two models.

Summary. In summary, none of the within or between-neighborhood measures of RSO clustering at the county level was significantly associated with the rate of recidivistic sex crimes committed against children. Thus, increased clustering of RSOs within or between certain neighborhoods was not associated with the rate of sex crimes

committed against children by those RSOs. However, the *rate* of RSOs in a county was significantly and positively related to the rate of recidivistic sex crimes committed against children in all three models. These results make intuitive sense given the previously reviewed literature on relational vs. spatial distance between victims and offenders and the lack of a relationship between associations with other RSOs and recidivism. Overall, these results indicate that the rate of recidivistic sex crimes committed against children is unrelated either to the spatial distance between RSOs or to the clustering of RSOs between neighborhoods across a county, but *is* related to the overall number of RSOs living in the county.

Recidivistic Sex Crimes Against Adult Victims

Mean county NNA. As shown in model 1 of TABLE 27, after all of the control variables were included the relationship between the rate of recidivistic sex crimes against adult victims and the mean county NNA was positive but non-significant. Thus, the proximity of RSOs to other RSOs within a county was unrelated to the rate of recidivistic sex crimes committed against adults. Specifically, while a one standard deviation increase in the mean county NNA was associated with a nine percent higher rate of recidivistic sex crimes committed against adult victims, this relationship was non-significant given an alpha level of .05. Similar to results regarding the recidivistic sex crimes committed against children, assuming geographic proximity increases the likelihood of associations between RSOs, these non-significant results are again consistent with the findings of Hanson and Harris (1998).

In fact, the only significant relationship in the model involved the physical size of the county, with every additional square mile in county size being associated with a one

percent lower rate of recidivistic sex crimes committed against adult victims. This indicates that larger counties have slightly lower rates of recidivistic sex crime committed against adults, controlling for other characteristics.

Revised index of isolation. As shown in model 2 of TABLE 27, after all of the control variables were included the relationship between the rate of recidivistic sex crimes committed against adult victims and the R.I.I. was negative and significant given an alpha level of .05. Thus, counties with RSOs clustered into a limited number of neighborhoods had lower rates of recidivistic sex crimes committed against adults compared to counties with more even distributions of RSOs among neighborhoods. Specifically, a one standard deviation increase in the R.I.I. was associated with an 11.2 percent lower rate of recidivistic sex crimes committed against adult victims.

This result indicates that it may be easier for either community members or law enforcement to monitor RSOs whereabouts and exert informal or formal social control (respectively) when RSOs are clustered in specific neighborhoods. However, if this were the case, one might expect that this relationship would be similar regardless of whether the individual was targeting a child or an adult victim, which the results do not support. Another explanation may be because recidivistic sex crimes committed against adult victims are more likely to involve a stranger victim (Greenfield, 1997), and/or involve direct contact (compared to contact through a mutual acquaintance or an existing biological relationship) (MNDOC, 2007), compared to recidivistic sex crimes committed against child victims. As a result, it may be that community members and/or law enforcement are better able to monitor for and/or respond to sex crimes committed against strangers in public locations, which happen to involve adult victims more often.

Thus, data on neighborhood-level social control mechanisms would be required to test this theory, and to examine why there may be a difference between recidivistic sex crimes committed against child victims compared to those committed against adult victims. Further, this relationship was non-significant when clustering was measured as the Oden's I^*_{pop} value, as noted below, which limits the strength of the conclusions that can be based on between-neighborhood RSO clustering. No other relationships in this model were significant.

Oden's I^*_{pop}. As shown in model 3 of TABLE 27, after all of the control variables were included, the relationship between the rate of recidivistic sex crimes committed against adult victims and the Oden's I^*_{pop} value was non-significant. Thus, counties with neighborhoods that were geographically clustered based on the rate of RSOs per capita were not more likely to have higher or lower rates of recidivistic sex crimes committed against adults compared to other counties, similar to results regarding recidivistic sex crimes committed against children.

Specifically, while a one standard deviation increase in the Oden's I^*_{pop} was associated with a 7.3 percent lower rate of recidivistic sex crimes against adult victims, this relationship was non-significant given an alpha level of .05. However, the relationship *was* significant at an alpha level of .06, which limits the strength of the conclusions that can be based on the non-significant findings, particularly given the significance of the R.I.I. measure in the second model. For example, if this non-significance were actually due to Type II error, then findings from both measures of the between-neighborhood clustering would indicate that increased clustering of RSOs among neighborhoods within a county yields lower rates of recidivistic sex crimes

committed against adults. Once again, no other independent variables in the model were significantly associated with the rate of recidivistic sex crimes committed against adults.

Summary. In summary, the within-neighborhood measure of RSO clustering at the county level was not significantly associated with the rate of recidivistic sex crimes committed against adults. Thus, increased clustering of RSOs within neighborhoods in a county was not associated with the rate of sex crimes committed against adults by those RSOs, although the overall rate of RSOs in a county *was* related. However, counties with RSOs that were clustered into fewer neighborhoods in some cases had lower levels of recidivistic sex crimes committed against adults. Given the near significant results of the other between-neighborhood clustering measure, this suggests that clustering RSOs into certain neighborhoods or into groups of nearby neighborhoods *may* subject these RSOs to increased social control mechanisms and thus reduced aggregate sexual offending against adult victims but not against child victims. Reasons for this discrepancy were previously discussed, but would require individual level data on RSO geographic proximity to other RSOs, community social control, and reasons for recidivating (or not recidivating) in order to confirm whether and why this was the case.

Summary of Results for Research Question 4

Overall, results indicated that the within and between-neighborhood RSO clustering at the county level was unrelated to the rate of sex crimes committed against child victims, since none of the three measures of within county clustering (the mean county NNA, the R.I.I., or the Oden's I^*_{pop}) reached significance given an alpha level of .05. Thus, when RSOs lived closer together, or lived in a limited number of neighborhoods within a county, there was not any increased or decreased risk of

recidivistic sex crimes against children. However, the rate of RSO residences within the county *was* associated with a significant increase in the rate of sex crimes committed against child victims, which makes sense given that more potential offenders (i.e., RSOs) per capita should yield more offenses (i.e., recidivistic sex crimes committed against children) per capita.

Alternatively, there are mixed results for whether within county RSO clustering is associated with the rate of sex crimes committed against adult victims. While the within-neighborhood clustering measure (i.e., the mean county NNA) was non-significant, one of the between-neighborhood clustering measures (i.e., the R.I.I.) was significant and negatively associated with the rate of sex crimes committed against adult victims. Further, the other between-neighborhood clustering measure (i.e., the Oden's I^*_{pop}) was also negatively associated but just outside of significance, indicating the potential for Type II error. Thus, certain types of between-neighborhood RSO clustering at the county level may be associated with reduced rates of recidivistic sex crimes committed against adult victims, but this conclusion is tentative at best. However, within-neighborhood RSO clustering at the county level was unrelated to the rate of recidivistic sex crimes committed against adult victims. Interestingly, the rate of RSO residences in a county was not significantly associated with the rate of sex crimes committed against adult victims, indicating that RSOs may not have been recidivating against adults at a similar rate between counties.

Overall, these results suggest that the dynamics between the county characteristics associated with recidivistic sex crimes committed against child victims are different from those associated with recidivistic sex crimes committed against adult victims in certain

cases but not others. For instance, when RSOs lived closer to one another within neighborhoods, counties did not have higher or lower rates of recidivistic sex crimes committed against either children or adults. However, counties that had RSOs clustered into a limited number of neighborhoods in a county, or possibly into certain areas of the county, had reduced recidivistic sex crimes committed against adults, but not against children. As already noted, this may be due to the different mechanisms and situations involved in selecting child victims compared to adult victims. Also, these results indicate that how RSO clustering is measured can have an important effect on conclusions, and that the rate of RSOs in a county is related to rates of recidivistic sex crimes committed against children, but not against adults.

CHAPTER 5: DISCUSSION AND CONCLUSION

This chapter begins by reviewing the purpose of the present study, including the four research questions and related hypotheses that were examined in the analyses. Following this, overall conclusions from the analyses are interpreted in light of the existing literatures. The contributions this study makes to these literatures, its limitations, and its implications for future research and policy are then discussed. The chapter ends with a summary conclusion which includes a call for more evidence-based research and a closing message for both researchers and policymakers.

The Present Study

This study explored four separate but related research questions in an attempt to address concerns with the existing literature on residence restrictions, their passage and efficacy, and their effect on RSO housing.

The passage of county residence restrictions. The first research question involved the passage of residence restrictions, with the analysis devoted to identifying the county characteristics associated with passing county-level residence restriction policies in New York between November 2005 and December 2009.

The efficacy of county residence restrictions. The second research question concerned the direct link between the presence of a county-level residence restriction policy and various sex crime rates. Specifically, multiple residence restriction legislation indicators were used to predict four types of monthly sex crime rates (i.e., recidivistic and non-recidivistic sex crimes committed against children and against adults) for each of the 62 counties in New York between 1998 and 2009.

The association between residence restrictions and RSO clustering. The third research question concerned the link between residence restrictions and the spatial distribution of RSOs. Specifically, the analysis for the third research question examined neighborhood-level demographic and crime rate characteristics, RSO addresses, and indicators of county and local-level residence restriction legislation to determine whether the presence of a residence restriction was associated with the spatial distribution of RSO residences in neighborhoods in upstate New York.

The association between RSO clustering and recidivistic sex crime rates. The fourth research question concerned the link between the spatial distribution of RSOs and rates of recidivistic sex crimes in a county. The analysis for the fourth research question incorporated data on RSO clustering, demographic, crime, and political characteristics for upstate New York counties to determine if the spatial distribution of RSO residences was associated with the rate of recidivistic sex crimes committed against either child or adult victims in 2009.

Prior Literature, Findings, and Interpretations

While the previous chapter contained some discussion and interpretation of specific results, this section further discusses the broader findings and conclusions of each research question in light of findings from prior and related bodies of literatures.

The Passage of Residence Restrictions

The prior literature on the passage of residence restrictions was limited to a single study conducted by Meloy and colleagues (2008) on state-level residence restriction policies. Based on the results of that study, it was expected that two characteristics would be positively associated with the likelihood of passing a residence restriction

policy: geographic proximity to a nearby jurisdiction passing a residence restriction policy and a Republican political philosophy. The related literature on the diffusion of innovation policies generally supported these hypotheses, and also suggested that the physical size, population density, and wealth of the county would be positively related with the passage of residence restriction policies. Finally, based on the highly political nature of sex offender policies and their focus on reductions in sex crimes, it was expected that the political competition in the county and its sex crime rate (from 2004) would also be positively related to the passage of such policies.

The analyses largely rejected the state-level findings of Meloy and colleagues (2008), and also largely rejected a diffusion of policy innovations perspective, but with one key exception. Specifically, the findings suggested that political competition, not political philosophy, was significantly associated with the passage of a county residence restriction. That is, the likelihood of passing a residence restriction policy was higher in counties that had high levels of political competition between rival political parties, compared to counties that had less political competition. Geographic proximity to a nearby county residence restriction was associated with a lower likelihood of passing a residence restriction, although this finding was only significant in half of the models. Interestingly, the rate of sex crimes was not significantly related to the likelihood of passing a residence restriction policy.

Overall, these results suggest that residence restrictions are not being used to address high rates of sex crimes, but instead are used as political maneuvers when politicians are facing stiff competition, regardless of the political party the politician belongs to.

Based on the results of this study, it is possible to still integrate findings into both the existing literature on the passage of residence restrictions as well as the literature on the diffusion of policy innovations. Specifically, a slightly revised diffusion of innovations perspective may provide a better theoretical framework to describe the spread of residence restrictions. In this sense, a residence restriction policy is a 'preventative adoption' theoretically based on preventing an unwanted occurrence (i.e., recidivistic sex crimes) in the future (see Rogers, 2003). Adopting a residence restriction policy in a county represents an 'authority innovation-decision' based on a 'decentralized diffusion system,' which depends on a decision by relatively few individuals (i.e., politicians) that spreads 'horizontally' from peer to peer (Rogers, 2003). Therefore, residence restrictions may first begin to spread like any other authority innovation-decision through a decentralized diffusion system among peer counties (see Rogers, 2003), which could have resulted in initial indications of a domino effect in certain cases.

In fact, the anecdotal evidence suggests that residence restrictions may initially spread quickly from county to county (though not necessarily geographically proximate counties), as small groups of politicians rush to adopt untested, largely symbolic legislation targeted at voter's negative sentiment regarding registered sex offenders (see Shih, 2010). However, the statistical evidence suggests that when counties are able to see a neighboring county dealing with the *consequences* of residence restrictions, they become less likely to pass such policies. Therefore, once counties start dealing with the consequences of a residence restriction, the window of opportunity to pass residence restrictions and politically benefit from their enactment without fearing the consequences may pass.

Kingdon (1995) refers to these short-lived opportunities as 'policy windows,' and Gerston (1997) links this idea to the 'triggering mechanism' of the Great Depression, which helped Franklin Roosevelt win the presidency and enact New Deal legislation. The 'triggering mechanism' for residence restrictions could be the initial passage of a residence restriction in the state, a particularly heinous sex crime that leads to widespread media coverage, or public outcry against perceived 'clustering' of RSOs in a community.

As such, once these policies are put into place in the early adopting counties, and those politicians begin to deal with the implementation troubles, legal liability, and other unintended consequences, they may serve as a policy deterrent for geographically proximate counties. That is, over time this increased adoption rate also results in increased 'observability' of the unintended consequences and lack of efficacy that come with residence restrictions. As nearby counties are able to observe these effects, they become less likely to adopt a residence restriction. This would result in the geographic diffusion of a new innovation: feelings *against* residence restriction policies, which would serve to limit the continued spread of residence restrictions.

No studies have yet considered this dual-innovation-diffusion possibility of residence restrictions, and thus future research could focus on examining the spread of both residence restriction policies and anti-residence restriction views among county-level politicians. Case studies of the passage of residence restrictions in specific counties may shed more light on this issue, and may help to better link their passage to the broader literature on the implementation of legislation. Future county-level research on the passage of residence restrictions in other states would be useful in confirming the findings of the present study, which are limited in their generalizability. Overall,

however, it seems that residence restrictions are passed for political reasons unrelated to sex crime rates, which leads one to question whether they are actually effective at either incapacitating or generally deterring sex crimes.

The Effectiveness of Residence Restrictions

As the first analysis indicated that residence restrictions are being used by politicians as tools to encourage voter support, and not to address high rates of sex crimes, and because the existing research provides no support for their efficacy (e.g., Blood, et al., 2008; Duwe, et al., 2008; MNDOC, 2007; Zandbergen, et al., 2010), it was expected that these policies would have no effect on either recidivistic or non-recidivistic sex crimes committed against either children or adults. However, much of the existing literature has only examined their ability to reduce recidivistic sex crimes through incapacitation, typically relying on theoretical instead of actual situations (e.g., Duwe, et al., 2008; MNDOC, 2007; Zandbergen, et al., 2010). Thus, examining the general deterrence effect of residence restrictions on non-recidivistic sex crimes represents a novel contribution to the literature regarding the efficacy of these policies, as does the studying the effect of these policies using longitudinal data at the county level.

Overall, the present study supports prior research that found residence restrictions were not effective at incapacitating recidivistic sex crimes that were committed against either children or adults. Given that many of these crimes occur among acquaintances and not among geographically proximate strangers (Greenfield, 1997), as well as indications that these policies are passed for political reasons, this finding is not surprising.

However, while residence restrictions did not generally deter non-recidivistic sex crimes committed against children, they did appear to generally deter at least some non-recidivistic sex crimes committed against adults. This is the first time a study has found that residence restrictions have had such an effect, and provides a new venue for future research based on the literature on deterrence. Specifically, analyzing the general deterrence effect of these laws on non-recidivistic sex crimes is an unexplored area that would greatly benefit from future longitudinal studies based on other states. Additionally, the 'mixed' findings of the present study are actually similar to those of the larger body of research on deterrence.

For example, prior research on deterrence has been mixed regarding a negative association between the crime rate and imprisonment risk, with some research indicating a strong support of a deterrence effect (e.g., Blumstein, Cohen, & Nagin, 1978; Nagin, 1998), while other research showing little support (e.g., Pratt & Cullen, 2005). However, much of this research has focused on arrest and imprisonment, while less research has been conducted on post-release criminal justice sanctions. Specific to sex offenders, however, research on the deterrence effect of sex offender registration and community notification has also found mixed results (e.g., Letourneau, et al., 2010; Sandler, et al., 2008; Veysey, Zgoba, & Dalessandro, 2008; Washington State Institute for Public Policy, 2009; Welchans, 2005; Zgoba, Veysey, & Dalessandro, 2010). In line with the mixed findings of both the broader literature on deterrence and the research specific to sex crimes, the current study also provides mixed support for a general deterrence effect. Specifically, only non-recidivistic sex crimes committed against adults were deterred, not those committed against children. Therefore, it is not surprising that the prior studies that

have examined sex crime rates *without* separating crimes involving child victims from those involving adult victims have found mixed results, and this indicates that different mechanisms may be influencing different types of sex crimes. Thus, future research on the effects of sex offender specific policies should attempt to analyze crime rates by separating offenders who are RSOs from those who are not, as well as separating crimes involving child victims from those involving adult victims.

The Association Between Residence Restrictions and RSO Clustering

As previously noted, the results of the present study indicate residence restrictions are not affecting recidivistic sex crime rates through the incapacitation of RSOs. However, the prior literature has indicated that these policies can influence the spatial distribution of RSOs through their restrictions on housing options (e.g., Chajewski & Mercado, 2009; Grubesic & Murray, 2008; Morgan, 2008; Youstin & Nobles, 2009; but see Berenson & Appelbaum, in press), although conclusions on whether this influence results in increased clustering or increased dispersion have been mixed. Thus, this study examined whether there was an association between the presence of a residence restriction and the spatial distribution of RSOs within and between neighborhoods.

Overall, the results support the existing literature that finds RSOs become slightly more clustered *within* neighborhoods. However, this effect appeared to be positively related only to the length of time the residence restriction had applied to that neighborhood. Conversely, this study found mixed evidence that neighborhoods were slightly less geographically clustered based on concentrations of RSOs the longer a residence restriction had been in place, although this finding was not present when neighborhood concentrations of RSOs were considered county-wide, regardless of

neighborhood geographic proximity. Thus, these mixed results reflect the similarly mixed findings in the existing literature, and could indicate that both the geographic area under consideration and the way in which clustering is measured can influence findings.

The Association Between RSO Clustering and Recidivistic Sex Crime Rates

Having found that residence restrictions are, in some instances, related to the spatial distribution of RSOs, the next set of analyses examined whether the spatial distribution of RSOs within a county was associated with rate of the recidivistic sex crimes committed against either child or adult victims. Despite the wealth of existing research on the relationship between residence restrictions and the spatial distribution of RSOs, it is still unclear whether this spatial distribution is associated with recidivistic sex crimes rates. This information is vital to exploring the link between the passage of a residence restriction and indirect policy implications for RSOs and community members. That is, if residence restrictions resulted in changes to the spatial distribution of RSOs, which then resulted in changes to the recidivistic sex crimes rates, then these policies could indirectly be affecting recidivistic sex crime rates. Without this type of important research, information regarding any indirect influence that residence restrictions could have on sex crime rates through their effect on RSO housing would remain unknown, and thus unavailable for consideration by those policymakers pondering such policies.

In contrast to the misconception of legislators and residents that sex offender clustering is related to sexual recidivism (e.g., Kilgannon, 2008; US States News, 2008), the present study found that increased clustering of RSOs was unrelated to rates of recidivistic sex crimes committed against children. Further, increased clustering of RSOs was in some instances associated with *fewer* recidivistic sex crimes committed against

adults. Reasons for these lower rates of recidivistic sex crimes committed against adult victims were previously discussed, but this leads to an important conclusion when the results of the third and fourth research question are considered together.

Specifically, even though residence restriction policies in some cases resulted in increased within-neighborhood and decreased between-neighborhood clustering of RSOs, and RSO clustering was related to lower rates of recidivistic sex crimes committed against adults, the *combined* results do not provide for an indirect effect. That is, the types of spatial distributions of RSOs at the neighborhood level that were associated with residence restrictions were *not* the ones that were, at the county level, associated with the rates of recidivistic sex crimes committed against adults. Thus the combined results of research questions three and four indicate that residence restrictions are not indirectly associated with recidivistic sex crime rates *through* their association with the spatial distribution of RSOs.

Overall Conclusions

Overall, this study finds that residence restrictions are passed in response to political competition and not high rates of sex crimes. Therefore, it is not surprising that these policies do not reduce the rate of recidivistic sex crimes as intended, nor is there any indication that they increase the safety of children from sexual assault (regardless of whether the crime is recidivistic or not). However, there is some evidence that these policies act as a general deterrence mechanism for non-recidivistic sex crimes committed against adults, although this certainly was not the original intention of policymakers.

This study also found that residence restrictions are in certain cases associated with increased within-neighborhood RSO clustering over time, and in certain cases

associated with decreased between-neighborhood RSO clustering. However, these measures of increased within-neighborhood and decreased between-neighborhood RSO clustering, when examined at the county level, are not associated with recidivistic sex crime rates. Thus, this study found no indirect association between residence restrictions and recidivistic sex crime rates. As such, the conclusions in this study (and much of the prior literature) find very little support for residence restrictions as an effective or otherwise useful criminal justice policy when it comes to protecting community members from RSOs.

Limitations

This study suffered from a few distinct limitations. One of these involved the use of aggregate data, particularly when examining the passage of residence restrictions. As a result, individual data on the decision making process of policymakers would have been useful to support the findings that residence restrictions are used as political tools, and that seeing the troubles that nearby counties must deal with after passing these policies can dissuade policymakers from passing their own residence restrictions. Still, as this study is the first to focus on the passage of county-level residence restrictions, even analyzing aggregate data can provide a starting point for future, more detailed, research agendas.

A second limitation concerns the generalizability of this study's findings. As this study used data only from counties and neighborhoods in New York, and only from the upstate New York area when considering the spatial distribution of RSOs in research questions three and four, the generalizability of these findings are obviously limited to

states similar to New York and/or to areas similar to upstate New York. As such, future research on other states would be useful to support the conclusions of the current study.

Perhaps the most important limitation involved the lack of historical/longitudinal data when examining the indirect association between residence restrictions and the rate of recidivistic sex crimes *through* an association with the spatial distribution of RSOs. Unfortunately, historical data on RSO residences were unavailable, and thus both the third and fourth research questions were limited to examining an association rather than causation. Still, the findings indicate no indirect association exists between residence restrictions and the rate of recidivistic sex crimes, and so it seems unlikely that a causal relationship would exist.

Future Research

This study has provided a number of avenues for future research regarding the passage, efficacy, and direct and indirect consequences of residence restrictions. One such avenue involves individual-level research involving policymakers. Specifically, research that involves interviewing policymakers about their decision-making process on proposing (or bypassing) residence restriction legislation can help support findings that these policies are being used for political purposes, and can help to better integrate the passage of residence restrictions into the literature regarding the diffusion of policy innovations.

A second avenue for future research involves replicating this study using data from other states and/or longitudinal data on RSO residences. As noted in the limitations section, the findings of this study are limited in their generalizability, as well as limited to examining association and not causation in the third and fourth research questions. Thus,

research findings from other states and/or involving longitudinal data would both help support the findings and generalizability of the current study, as well as help confirm the lack of an indirect causal relationship between residence restrictions and recidivistic sex crime rates.

This study also provided a number of best practices that can be used in future research. One involves the measurement of political competition in addition to the 'typical' measures of political philosophy. As shown, while political philosophy may be an important motivator for passing certain criminal justice policies, ignoring political competition may obscure what is really driving the proposal and passage of such policies. Additionally, this study reviewed and utilized a number of different measures of spatial clustering at both the neighborhood and county level. These various measures provide future researchers with a blueprint for measuring the spatial clustering of individuals in ways that are both methodologically sound and conceptually different from each other.

Policy Implications

While there are many policy implications that stem from this study, perhaps the most important involves the continued passage of residence restrictions. Specifically, the results of this study combined with the limited prior research imply that these policies are not appropriate methods to protect citizens from recidivistic sex crimes. Thus, while these policies are obviously popular with both politicians and residents, they are not *effective* at achieving their intended goals of increasing the protection of children from sex crimes and/or increasing the protection of *any* individuals from sex crimes committed by RSOs in the community.

A related policy implication is that residence restrictions *may* increase the safety of residents from non-recidivistic sex crimes involving adult victims through a general deterrence effect. However, the deterrence of individuals who may potentially commit a sex crime against an adult represents a fairly weak basis for implementing policies that subject *other* individuals (i.e., RSOs) to restrictions on where they can live. This is particularly true given the large body of existing research showing the numerous unintended consequences that these policies can have on the successful reentry and rehabilitation of RSOs. Therefore, policymakers should seek other policies that are not only effective, but are either specifically targeted at generally deterring sex crimes, or are *also* effective at reducing recidivistic sex crimes.

A final policy implication is that while residence restrictions can influence the spatial distribution of RSOs within neighborhoods, this develops slowly over time, and does not appear to have an indirect influence on rates of recidivistic sex crimes. Thus, while these policies may be effective either at removing RSOs from certain neighborhoods and/or at clustering RSOs into specific areas, this does not appear to ultimately result in the increased safety of residents.

Final Thoughts

This study has examined a number of issues related to sex offender residence restrictions and generated a number of interesting and perhaps surprising conclusions. However, even if the results of this study are widely distributed among academics and policymakers, and/or this call for more research is heeded by future researchers, these results will only be valuable to the extent that they are *used* by policymakers to make sound policy decisions. It is not enough to produce yet another research article or

(extremely impressive) dissertation highlighting the significant and non-significant associations between two variables in a model, hypothesizing about intended and unintended consequences on community members, or otherwise damming the effectiveness of a certain policy through the use of statistical evidence. Instead, connections between researchers and policymakers must be forged and strengthened, and policymakers must have the courage to stand before their constituents and propose effective policies that are based on sound research, even if this does not help them win voter support or ensure their reelection. Without these actions, the tangible implications of this study (and most other academic research) will remain unused and largely irrelevant, and the recommendations and warnings for policymakers and community members will remain unheeded.

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TABLE 1

County-Level Variables Analyzing the Passage of Residence Restrictions

Variable Name	Measurement	Data Source
Passed a residence restriction policy	Did the county pass a residence restriction policy as of December 2009?	NYS OSOM (2010a)
Prior residence restriction in a nearby county	Was there a residence restriction policy in an adjoining at least one month prior to the earlier of either a) the date of implementation of the county's residence restriction policy or b) December 2009?	NYS OSOM (2010a)
Prior residence restriction in a local jurisdiction	Was there a residence restriction policy in a local jurisdiction within that county at least one month prior to the earlier of either a) the date of implementation of the county's residence restriction policy or b) December 2009?	NYS OSOM (2010a)
County size	The physical size of the county in 100 square miles of land area.	U.S. Census (2002)

TABLE 1, continued

Variable Name	Measurement	Data Source
Political philosophy	Percentage of residents who were registered in either the Republican or Conservative parties as of November 3005.	NYS BOE (2010)
Population density	Logged rate of residents per square mile.	U.S. Census (2002)
Resident income	Logged mean resident income (in $1,000s).	U.S. Census (2002)
Political competition	Percentage lead that the majority political party had over all other parties (combined) as of 2010, based on the population of registered voters as of November 2005. This Measure was negatively coded.	NYS BOE (2010)
Sex crime rate in 2004	Average rate of all sex offenses committed in 2004 per 100,000 residents. Used as a proxy for an aggregate measure of individuals' exposure to sex crimes.	NYS DCJS CCH (2010); U.S. Census (2010)

TABLE 2

County-Level Variables Analyzing the Efficacy of Residence Restrictions

Variable Name	Measurement	Data Source
Sex offenses with child victims by prior sex offenders	Number of sex offenses committed that month against children (under 18 years old) by offenders with a prior sexual conviction (converted into a rate using a population exposure term).	NYS DCJS CCH (2010); U.S. Census (2010)
Sex offenses with adult victims by prior sex offenders	Number of sex offenses committed that month against adults by offenders with a prior sexual conviction (converted into a rate using a population exposure term).	NYS DCJS CCH (2010); U.S. Census (2010)
Sex offenses with child victims by non-prior sex offenders	Number of sex offenses committed that month against children (under 18 years old) by offenders without a prior sexual conviction (converted into a rate using a population exposure term).	NYS DCJS CCH (2010); U.S. Census (2010)

TABLE 2, continued

Variable Name	Measurement	Data Source
Sex offenses with adult victims by non-prior sex offenders	Number of sex offenses committed that month against adults by offenders without a prior sexual conviction (converted into a rate using a population exposure term).	NYS DCJS CCH (2010); U.S. Census (2010)
Existing residence restriction policy	Did the county have a residence restriction policy in place at any point during the month?	NYS OSOM (2010a)
Lagged sex crime rates	The number of similar sex crimes per 10,000 residents committed in the previous month. This measure is specific to each of the four types of sex crimes.	NYS DCJS CCH (2010); U.S. Census (2010)
Burglary rate	The number of burglaries per 10,000 residents committed that month.	NYS DCJS CCH (2010); U.S. Census (2010)
Robbery rate	The number of robberies per 10,000 residents committed that month.	NYS DCJS CCH (2010); U.S. Census (2010)

TABLE 2, continued

Variable Name	Measurement	Data Source
County	A set of dummy variables indicating the county.	N/A
Month-year	A set of dummy variables indicating the month and year of the observation.	N/A
County trend	A county-specific trend variable.	N/A
Resident population	The number of residents (in 10,000s) living in the county as of July 1st of that year. This variable is used as an exposure term in the model.	U.S. Census (2010)

TABLE 3

Neighborhood-Level Variables Analyzing Sex Offender Clustering as of December, 2009

Variable Name	Measurement	Data Source
Average nearest neighbor distance (neighborhood NNA)	The average distance (in meters) of the nearest five RSO neighbors, averaged for all RSOs within a neighborhood.	NYS OSOM (2010a)
Excess risk	The ratio of the actual number of RSO residences per 10,000 residents in a neighborhood compared to the expected number per 10,000 residents given a homogenous distribution of RSOs within the county based on each neighborhood's population of residents. This value was increased by 22 and then logged.	NYS OSOM (2010a); U.S. Census (2010)
LISA value of the rate of RSOs	The average LISA value of the rate of RSOs per 10,000 residents. This value was increased by 2.5, multiplied by 100, and then logged.	Anselin et al. (2006)

TABLE 3, continued

Variable Name	Measurement	Data Source
Months of a residence restriction policy	The number of months a neighborhood was subject to any residence restriction policy as of December 2009.	NYS OSOM (2010a)
Existing residence restriction policy	Was the neighborhood subject to at least one county or local-level residence restriction policy as of December 2009?	NYS OSOM (2010a)
Existing RR 1000' or less	If a neighborhood was subject to at least one residence restriction policy as of December 2009, was the buffer zone of the largest sized policy 1000' or less?	NYS OSOM (2010a)
Existing RR over 1000'	If a neighborhood was subject to at least one residence restriction policy as of December 2009, was the buffer zone of the largest sized policy greater than 1000'?	NYS OSOM (2010a)

TABLE 3, continued

Variable Name	Measurement	Data Source
RR includes 'other' locations	Did any of the residence restriction policies applying to the neighborhood include locations other than schools, daycares, parks, and playgrounds in the scope?	NYS OSOM (2010a)
Nearby residence restriction policy	Did an adjoining neighborhood have an existing residence restriction policy as of December 2009?	NYS OSOM (2010a)
Concentrated disadvantage	A factor score based on the following: Percent living in poverty, percent unemployed, percent female heads of household with children, and percent non-Hispanic black residents.	U.S. Census (2002)
Residential instability	A factor score based on the following: Percent owner-occupied homes and percent residents five years and older who have lived in the same house for at least five years.	U.S. Census (2002)

TABLE 3, continued

Variable Name	Measurement	Data Source
Ethnic heterogeneity	A factor score based on the following: Percent Hispanic residents and percent foreign born residents. This factor score was increased by 3 and logged.	U.S. Census (2002)
Housing availability	The square root of the percent of vacant rental housing units.	U.S. Census (2002)
Housing affordability	The neighborhood's median gross rent in 2000, divided by the fair market rent in that housing market for a two bedroom apartment in 2001. This ratio was increased by 1 and logged.	HUD (2010); U.S. Census (2002)
Population density	Logged residents per square mile.	U.S. Census (2002)

TABLE 4

County-Level Variables Analyzing Sex Crime Rates as of December, 2009

Variable Name	Measurement	Data Source
2009 mean monthly recidivistic sex offenses committed against child victims	Mean 2009 monthly rate of sex offenses committed against children (under 18 years old) by offenders with a prior sexual conviction.	NYS DCJS CCH (2010); U.S. Census (2010)
2009 mean monthly recidivistic sex offenses committed against adult victims	Mean 2009 monthly rate of sex offenses committed against adults (18 years and older) by offenders with a prior sexual conviction.	NYS DCJS CCH (2010); U.S. Census (2010)
County mean of the mean NNA distance (mean county NNA)	The average distance of the nearest five RSO neighbors, in meters, for all RSOs within a county as of September 2010. This measure was standardized.	NYS OSOM (2010a)

TABLE 4, continued

Variable Name	Measurement	Data Source
Revised index of isolation (R.I.I.)	Compares the probable interaction of RSOs within each neighborhood given the current spatial distribution to the probable interaction based on a homogenous mixture across neighborhoods within the county. See APPENDIX E for more information. This measure was standardized.	NYS OSOM (2010a); U.S. Census (2010)
Oden's I^*_{pop}	Compares the actual spatial clustering of RSOs across neighborhoods to a random distribution within each county, controlling for underlying variations in the neighborhood population. See Oden (1995) for more information. This measure was standardized.	NYS OSOM (2010a); U.S. Census (2010)
Rate of RSO residences	RSO residences per 10,000 adult residents as of September 2010.	NYS OSOM (2010a); U.S. Census (2010)

TABLE 4, continued

Variable Name	Measurement	Data Source
Concentrated disadvantage	A factor score based on the following: Percent living in poverty, percent unemployed, percent female heads of household with children, and percent non-Hispanic black residents.	U.S. Census (2002)
Residential instability	A factor score based on the following: Percent owner-occupied homes and percent residents five years and older who have lived in the same house for at least five years.	U.S. Census (2002)
Ethnic heterogeneity	A factor score based on the following: Percent Hispanic residents and percent foreign born residents.	U.S. Census (2002)
Population density	Residents per square mile.	U.S. Census (2002)
County size	Size of the county in square miles.	U.S. Census (2002)

TABLE 4, continued

Variable Name	Measurement	Data Source
Robbery rate	The 2009 mean monthly rate of robberies per 100,000 residents in a county.	NYS DCJS CCH (2010); U.S. Census (2010)
Burglary rate	The 2009 mean monthly rate of burglaries per 100,000 residents in a county.	NYS DCJS CCH (2010); U.S. Census (2010)

TABLE 5

Descriptive Results for Variables in Research Question 1

Variable	N	Mean	SD	Min	Max
Passed a RR policy	62	.32	.47	0	1
Geographic proximity					
Prior RR in nearby county	62	0.73	0.45	0	1
Prior RR in local jurisdiction	62	0.32	0.47	0	1
Diffusion of innovations					
County size	62	7.62	4.92	0.23	26.86
Population density	62	5.36	1.89	1.14	11.11
Resident income	62	2.97	0.21	2.64	3.76
Political climate					
Political philosophy	62	40.36	11.24	8	65
Political competition	62	6.97	17.13	-49	35
Sex crime rate					
Sex crime rate in 2004	62	50.87	19.47	18.14	115.58

TABLE 6

Correlation Matrix for Variables in Research Question 1

Variable	1	2	3	4	5	6	7	8	9
Passed a RR	1.00								
Geographic proximity									
Prior RR in nearby county	-.20	1.00							
Prior RR in local jurisdiction	-.03	-.12	1.00						
Diffusion of innovations									
County size	-.09	.05	.31	1.00					
Population density	-.05	-.19	.01	-.64	1.00				
Resident income	.22	-.02	.08	-.38	.54	1.00			
Political climate									
Political philosophy	.00	.15	-.01	.45	-.83	-.38	1.00		
Political competition	.27	.04	.30	.15	-.26	.17	.09	1.00	
Sex crime rate									
Sex crime rate in 2004	-.16	.05	-.06	.36	-.53	-.50	.43	-.21	1.00

TABLE 7

Predicting the Passage of a County Residence Restriction Law

	Passed a RR policy							
	Logistic model				Linear probability model			
	1		2		1		2	
Variable	e^b	(se)	e^b	(se)	b	(se)	b	(se)
Geographic proximity								
Prior RR in nearby county	.24*	(.17)	.21*	(.16)	-.23º	(.14)	-.24º	(.14)
Prior RR in local jurisdiction	.46	(.34)	.36	(.27)	-.16	(.14)	-.15	(.14)
Diffusion of innovations								
County size	.97	(.08)	.99	(.08)	<.01	(.01)	<.01	(.01)
Population density	.74	(.28)	-	-	-.01	(.04)	-	-
Resident income	40.5	(99.6)	105º	(266)	.40	(.34)	.42	(.32)
Political climate								
Political philosophy	-	-	1.09	(.07)	-	-	.01	(.01)
Political competition	1.05*	(.02)	1.08*	(.04)	.01*	(<.01)	.01*	(<.01)
Sex crime rate								
Sex crime rate in 2004	1.00	(.02)	1.01	(.02)	.00	(<.01)	.00	(<.01)
Constant	<.01	(<.01)	<.01	(<.01)	-.63	(1.09)	-.86	(1.05)
N	62		62		62		62	
R^2	.16[a]		.19[a]		.17		.18	

Note. The coefficients in the Logistic models are reported as odds-ratios, while the coefficients in the linear probability models (OLS) are reported as the change in likelihood of passing a county residence restriction policy. Models present robust standard errors.

[a] Pseudo R^2

º p < .10; * p < .05; ** p < .01; *** p < .001 (two-tailed)

TABLE 8

Descriptive Statistics for Variables in Research Question 2

Variable		Mean	SD	Min	Max	Observations
Sex crimes: child	overall	.11	.37	0	5	N = 8,928
victims, prior	between		.12	0	.56	n = 62
conviction	within		.35	-.44	4.56	T = 144
Sex crimes: adult	overall	.31	.79	0	0	N = 8,928
victims, prior	between		.52	.01	3.22	n = 62
conviction	within		.59	-2.91	6.09	T = 144
Sex crimes: child	overall	2.44	3.83	0	38	N = 8,928
victims, no prior	between		3.39	.06	16.10	n = 62
conviction	within		1.84	-8.95	24.34	T = 144
Sex crimes: adult	overall	7.04	14.11	0	141	N = 8,928
victims, no prior	between		13.37	.14	67.47	n = 62
conviction	Within		4.82	-33.43	80.57	T = 144
County residence	overall	.07	0.26	0.00	1.00	N = 8,928
restriction policy	between		0.11	0.00	0.34	n = 62
	within		0.24	-0.27	0.98	T = 144
Lagged sex crime rate:	overall	.01	.03	0	.52	N = 8,966
child victims, prior	between		.01	0	0.02	n = 62
conviction	within		.03	-.01	.53	T = 143

TABLE 8, continued

Variable		Mean	SD	Min	Max	Observations
Lagged sex crime rate:	overall	.01	.06	0	1.93	N = 8,966
adult victims, prior	between		.01	<.01	.03	n = 62
conviction	within		.06	-.02	1.92	T = 143
Lagged sex crime rate:	overall	.12	.17	0	3.84	N = 8,966
child victims, no	between		.05	.03	.24	n = 62
prior conviction	within		.16	-.12	3.85	T = 143
Lagged sex crime rate:	overall	.25	.23	0	3.86	N = 8,966
adult victims, no	between		.07	.10	.43	n = 62
prior conviction	within		.23	-.13	3.83	T = 143
Burglary rate	overall	.63	.51	0	9.12	N = 8,966
	between		.17	.23	1.03	N = 62
	within		.48	-.30	8.91	T = 144
Robbery rate	overall	.22	.32	0	2.23	N = 8,966
	between		.28	0	1.31	N = 62
	within		.17	-.42	2.31	T = 144
Resident population (in	overall	30.81	52.30	.49	256.71	N = 8,928
10,000s)	between		52.67	.51	246.84	N = 62
	within		2.17	8.57	40.68	T = 144

TABLE 9

Correlation Matrix for Variables in Research Question 2

Variable	1	2	3	4	5	6	7	8	9	10	11
Sex crime rate: child victims, prior conviction[a]	1.00										
Sex crime rate: adult victims, prior conviction[a]	-	1.00									
Sex crime rate: child victims, no prior conviction[a]	-	-	1.00								
Sex crime rate: adult victims, no prior conviction[a]	-	-	-	1.00							
County residence restriction policy	-.01	-.01	-.06	-.03	1.00						

TABLE 9, continued

Variable	1	2	3	4	5	6	7	8	9	10	11
Lag sex crime rate: child victims, prior conviction	.00	-	-	-	-.01	1.00					
Lag sex crime rate: adult victims, prior conviction	-	.01	-	-	-.01	-	1.00				
Lag sex crime rate: child victims, no prior conviction	-	-	.11	-	-.05	-	-	1.00			
Lag sex crime rate: adult victims, no prior conviction	-	-	-	.11	-.02	-	-	-	1.00		
Burglary rate	.01	.02	.13	.12	-.02	.03	.03	.10	.12	1.00	
Robbery rate	-.05	-.02	-.11	.02	-.02	-.06	-.03	-.11	.03	-.02	1.00

[a] Converted into the rate of crimes per 10,000 residents.

TABLE 10

Predicting the Incapacitative Effect of a Residence Restriction Policy on Rates of Recidivistic Sex Crimes Committed Against Children

Variable	Child victim sex crimes, prior sexual convictions			
	1	2	3	4
County RR policy	.68*	.93	.93	.94
	(.11)	(.22)	(.23)	(.23)
Lag			.31	.31
			(.37)	(.37)
Burglary rate				.99
				(.10)
Robbery rate				1.10
				(.23)
Constant	.16***	.43	.44	.04*
	(.06)	(.41)	(.41)	(.06)
Temporal and county specific trends	N	Y	Y	Y
N	8,496	8,496	8,437	8,437

Note. Coefficients are in odds-ratios. An exposure term was included that represented the population (in 10,000 residents) in each county as of July 1st of each year.
º p < .10; * p < .05; ** p < .01; *** p < .001 (two-tailed)

TABLE 11

Predicting the Incapacitative Effect of a Residence Restriction Policy on Rates of Recidivistic Sex Crimes Committed Against Adults

Variable	Adult victim sex crimes, prior sexual convictions			
	1	2	3	4
County RR policy	1.01	.89	.88	.91
	(.10)	(.14)	(.14)	(.14)
Lag			.48	.47
			(.31)	(.31)
Burglary rate				1.04
				(.07)
Robbery rate				1.26
				(.16)
Constant	.48***	.63	1.21	.87
	(.05)	(.55)	(1.21)	(.72)
Temporal and county specific trends	N	Y	Y	Y
N	8,928	8,928	8,866	8,866

Note. Coefficients are in odds-ratios. An exposure term was included that represented the population (in 10,000 residents) in each county as of July 1st of each year.
º p < .10; * p < .05; ** p < .01; *** p < .001 (two-tailed)

TABLE 12

Predicting the General Deterrence Effect of a Residence Restriction Policy on Rates of Non-Recidivistic Sex Crimes Committed Against Children

Variable	Child victim sex crimes, no prior sexual convictions			
	1	2	3	4
County RR policy	.76***	.98	.98	.97
	(.03)	(.05)	(.05)	(.05)
Lag			.99	.99
			(.08)	(.08)
Burglary rate				1.02
				(.03)
Robbery rate				.94
				(.04)
Constant	.65***	1.42	.78	.78
	(.07)	(.29)	(.25)	(.25)
Temporal and county specific trends	N	Y	Y	Y
N	8,928	8,928	8,866	8,866

Note. Coefficients are in odds-ratios. An exposure term was included that represented the population (in 10,000 residents) in each county as of July 1st of each year.
º p < .10; * p < .05; ** p < .01; *** p < .001 (two-tailed)

TABLE 13

Predicting the General Deterrence Effect of a Residence Restriction Policy on Rates of Non-Recidivistic Sex Crimes Committed Against Adults

Variable	Adult victim sex crimes, no prior sexual convictions			
	1	2	3	4
County RR policy	.92***	.89***	.90**	.90**
	(.02)	(.03)	(.03)	(.03)
Lag			1.23***	1.23***
			(.05)	(.05)
Burglary rate				1.06**
				(.02)
Robbery rate				.95
				(.03)
Constant	.41***	.82*	.70*	.69*
	(.02)	(.08)	(.12)	(.12)
Temporal and county specific trends	N	Y	Y	Y
N	8,928	8,928	8,866	8,866

Note. Coefficients are in odds-ratios. An exposure term was included that represented the population (in 10,000 residents) in each county as of July 1st of each year.
º p < .10; * p < .05; ** p < .01; *** p < .001 (two-tailed)

TABLE 14

Descriptive Statistics for Variables in Research Question 3

Variable	N	Mean	SD	Min	Max
Neighborhood NNA	1,085[a]	3,226.54	2,500.02	0	9,852
Excess risk[c]	5,923[b]	2.95	.57	1.26	4.92
LISA value[c]	5,964[b]	92.70	3.60	51.80	138.37
Existing residence restriction	6,007	.52	.50	0	1
Months of residence restriction	6,007	5.62	13.99	0	49
Existing RR 1000' or less	6,007	.26	.44	0	1
Existing RR over 1000'	6,007	.26	.44	0	1
RR includes 'other' locations	6,007	.28	.45	0	1
Nearby residence restriction	6,007	.72	.45	0	1
Concentrated disadvantage	6,007	0	.85	-.97	5.84
Residential instability	6,007	0	.86	-1.90	3.23
Ethnic heterogeneity[c]	6,007	1.08	.17	.90	2.30
Housing availability[c]	6,007	.29	.16	0	1
Housing affordability[c]	5,996	.66	.16	0	1.64
Population density[c]	6,007	6.86	2.03	-.16	12.41

[a] Only includes neighborhoods with at least one registered sex offender.
[b] Neighborhoods with values that fell outside of +/- 3 standard deviations of the mean were removed from the model.
[c] Variable has been transformed through multiplication, offsetting, logging, and/or the use of a square root to produce a more normal distribution suitable for OLS regression.

TABLE 15

Correlation Matrix for Variables in Research Question 3 for the Nearest Neighbor Distance Model

Variable (N = 1,085)[a]	1	2	3	4	5	6	7	8	9	10	11	12	13
Neighborhood NNA	1.00												
Months of residence restriction	.03	1.00											
Existing residence restriction	-.32	-	1.00										
Existing RR 1,000' or less	.21	-	-	1.00									
Existing RR over 1,000'	-.19	-	-	-.38	1.00								
RR includes 'other' locations	.04	-	-	.63	.02	1.00							
Nearby residence restriction	.06	.52	.20	.31	.27	.32	1.00						
Concentrated disadvantage	-.40	.03	.35	-.20	.24	-.05	-.08	1.00					
Residential instability	-.43	-.00	.29	-.19	.20	-.08	-.08	.63	1.00				
Ethnic heterogeneity[b]	-.32	-.08	.18	-.16	.08	-.12	-.15	.46	.55	1.00			
Housing availability[b]	.14	.03	.03	.01	.02	-.02	-.08	.21	.04	.04	1.00		
Housing affordability[b]	.01	-.13	-.12	-.08	-.06	-.06	-.04	-.18	-.16	-.07	-.19	1.00	
Population density[b]	-.61	.03	.47	-.30	.36	.02	-.05	.58	.61	.48	-.26	.03	1.00

[a] Only includes neighborhoods with at least one registered sex offender.
[b] Variable has been transformed through multiplication, offsetting, logging, and/or the use of a square root to produce a more normal distribution suitable for OLS regression.

TABLE 16

Correlation Matrix for Variables in Research Question 3 for the Excess Risk Model

Variable (N = 5,912)[a]	1	2	3	4	5	6	7	8	9	10	11	12	13
Excess risk[b]	1.00												
Existing residence restriction	.01	1.00											
Months of residence restriction	.06	-	1.00										
Existing RR 1,000' or less	-.04	-	-	1.00									
Existing RR over 1,000'	.05	-	-	-.35	1.00								
RR includes 'other' locations	.02	-	-	.58	.11	1.00							
Nearby residence restriction	-.04	.62	.24	.35	.36	.37	1.00						
Concentrated disadvantage	.30	-.01	.17	-.12	.11	-.06	-.07	1.00					
Residential instability	.25	-.01	.07	-.07	.05	-.09	-.06	.61	1.00				
Ethnic heterogeneity[b]	.15	-.11	-.03	-.09	-.03	-.09	-.18	.32	.43	1.00			
Housing availability[b]	.21	.02	.04	.05	-.03	-.01	.00	.31	.16	-.00	1.00		
Housing affordability[b]	-.09	-.02	-.07	-.02	.00	-.04	.00	-.18	-.14	-.03	-.11	1.00	
Population density[b]	.12	.05	.27	-.20	.26	-.02	-.05	.40	.49	.36	-.29	.02	1.00

[a] Neighborhoods with values that fell outside of +/- 3 standard deviations of the mean were removed from the model.
[b] Variable has been transformed through multiplication, offsetting, logging, and/or the use of a square root to produce a more normal distribution suitable for OLS regression.

TABLE 17

Correlation Matrix for Variables in Research Question 3 for the LISA Value Model

Variable (N = 5,953)[a]	1	2	3	4	5	6	7	8	9	10	11	12	13
LISA value[b]	1.00												
Existing residence restriction	-.00	1.00											
Months of residence restriction	-.01	-	1.00										
Existing RR 1,000' or less	-.04	-	-	1.00									
Existing RR over 1,000'	.03	-	-	-.35	1.00								
RR includes 'other' locations	.01	-	-	.58	.11	1.00							
Nearby residence restriction	-.03	.61	.23	.35	.35	.36	1.00						
Concentrated disadvantage	.13	-.01	.17	-.12	.11	-.06	-.07	1.00					
Residential instability	.14	-.01	.06	-.06	.05	-.09	-.06	.61	1.00				
Ethnic heterogeneity[b]	.12	-.11	-.03	-.09	-.03	-.09	-.18	.32	.43	1.00			
Housing availability[b]	.03	.02	.04	.05	-.03	-.01	.01	.31	.16	.00	1.00		
Housing affordability[b]	-.02	-.02	-.07	-.02	.00	-.04	.00	-.18	-.14	-.03	-.11	1.00	
Population density[b]	.14	.05	.26	-.20	.26	-.02	-.05	.39	.49	.36	-.29	.02	1.00

[a] Neighborhoods with values that fell outside of +/- 3 standard deviations of the mean were removed from the model.
[b] Variable has been transformed through multiplication, offsetting, logging, and/or the use of a square root to produce a more normal distribution suitable for OLS regression.

TABLE 18

Neighborhood Characteristics Associated with the Neighborhood Mean Distance of the 5 Nearest RSO Neighbors

	Neighborhood NNA					
	1		2		3	
Variable	b	(se)	b	(se)	b	(se)
Existing residence restriction	28.4	(177)	191	(166)	295º	(151)
Nearby residence restriction	341	(230)	-72.8	(211)	-60.5	(192)
Concentrated disadvantage			-664***	(95.6)	-141º	(82.0)
Residential instability			-924***	(137)	-276*	(114)
Ethnic heterogeneity[a]			-1,222**	(437)	170	(365)
Housing availability[a]					109	(480)
Housing affordability[a]					285	(547)
Population density[a]					-643***	(38.5)
Constant	2,936***	(177)	4,234***	(512)	6,148***	(601)
N	1,085		1,085		1,085	
R^2	<.01		.22		.38	

Note. Only includes neighborhoods with at least one registered sex offender. Models present robust standard errors.

[a] Variable has been transformed through multiplication, offsetting, logging, and/or the use of a square root to produce a more normal distribution suitable for OLS regression

º p < .10; * p < .05; ** p < .01; *** p < .001 (two-tailed)

TABLE 19

Neighborhood Characteristics Associated with the Neighborhood Mean Distance of the 5 Nearest RSO Neighbors

Variable	Neighborhood NNA					
	1		2		3	
	b	(se)	b	(se)	b	(se)
Existing residence restriction	295º	(151)	-	-	-	-
Months of residence restriction	-	-	-8.86*	(4.04)	-	-
Existing RR 1000' or less	-	-	-	-	136	(221)
Existing RR over 1000'	-	-	-	-	275	(185)
RR includes 'other' locations	-	-	-	-	182	(174)
Nearby residence restriction	-60.5	(192)	213	(170)	-65.9	(194)
Concentrated disadvantage	-141º	(82.0)	-131	(81.0)	-134º	(82.2)
Residential instability	-276*	(114)	-295**	(114)	-266*	(115)
Ethnic heterogeneity[a]	170	(365)	76.5	(369)	226	(376)
Housing availability[a]	109	(480)	324	(486)	65.6	(487)
Housing affordability[a]	285	(547)	39.3	(553)	294	(548)
Population density[a]	-643***	(38.5)	-598***	(43.8)	-659***	(45.0)
Constant	6,148***	(688)	6,068***	(617)	6,189***	(740)
N	1,085		1,085		1,085	
R^2	.38		.38		.38	

Note. Only includes neighborhoods with at least one registered sex offender. Models present robust standard errors.

[a] Variable has been transformed through multiplication, offsetting, logging, and/or the use of a square root to produce a more normal distribution suitable for OLS regression.

º $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed)

TABLE 20

Neighborhood Characteristics Associated with Neighborhood Excess Risk

Variable	Neighborhood excess risk[a]					
	1		2		3	
	b	(se)	b	(se)	b	(se)
Existing residence restriction	.07***	(.02)	.05*	(.05)	.04º	(.02)
Nearby residence restriction	-.09***	(.02)	-.05*	(.02)	-.04*	(.02)
Concentrated disadvantage			.16***	(.01)	.12***	(.01)
Residential instability			.06***	(.01)	.04***	(.01)
Ethnic heterogeneity[a]			.10*	(.05)	.14**	(.05)
Housing availability[a]					.56***	(.06)
Housing affordability[a]					-.12**	(.04)
Population density[a]					.02**	(.01)
Constant	2.98***	(.01)	2.85***	(.06)	2.64***	(.07)
N	5,923		5,923		5,912	
R^2	< .01		.10		.12	

Note. Neighborhoods with values that fell outside of +/- 3 standard deviations of the mean were removed from the model. Models present robust standard errors.

[a] Variable has been transformed through multiplication, offsetting, logging, and/or the use of a square root to produce a more normal distribution suitable for OLS regression.

º $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed)

TABLE 21

Neighborhood Characteristics Associated with Neighborhood Excess Risk

Variable	Neighborhood excess risk[a]					
	1		2		3	
	b	(se)	b	(se)	b	(se)
Existing residence restriction	.04º	(.02)	-	-	-	-
Months of residence restriction	-	-	<.01	(.01)	-	-
Existing RR 1000' or less	-	-	-	-	-.04	(.03)
Existing RR over 1000'	-	-	-	-	.02	(.02)
RR includes 'other' locations	-	-	-	-	.09***	(.02)
Nearby residence restriction	-.04*	(.02)	-.02	(.02)	-.04*	(.02)
Concentrated disadvantage	.12***	(.01)	.12***	(.01)	.12***	(.01)
Residential instability	.04***	(.01)	.04***	(.01)	.05***	(.01)
Ethnic heterogeneity[a]	.14**	(.05)	.13**	(.05)	.14**	(.05)
Housing availability[a]	.56***	(.06)	.56***	(.06)	.56***	(.06)
Housing affordability[a]	-.12**	(.04)	-.13**	(.04)	-.11**	(.04)
Population density[a]	.03**	(.02)	.02**	(.01)	.01*	(.01)
Constant	2.64***	(.07)	2.63***	(.07)	2.65***	(.07)
N	5,912		5,912		5,912	
R^2	.12		.12		.12	

Note. Neighborhoods with values that fell outside of +/- 3 standard deviations of the mean were removed from the model. Models present robust standard errors.

[a] Variable has been transformed through multiplication, offsetting, logging, and/or the use of a square root to produce a more normal distribution suitable for OLS regression.

º p < .10; * p < .05; ** p < .01; *** p < .001 (two-tailed)

TABLE 22

Neighborhood Characteristics Associated with the Neighborhood LISA Value of the Rate of RSO Residences

Variable	Neighborhood LISA value[a]					
	1		2		3	
	b	(se)	b	(se)	b	(se)
Existing residence restriction	.12	(.11)	.07	(.11)	.00	(.11)
Nearby residence restriction	-.28*	(.13)	-.08º	(.12)	-.05	(.12)
Concentrated disadvantage			.29**	(.11)	.17	(.11)
Residential instability			.30**	(.09)	.16º	(.09)
Ethnic heterogeneity[a]			1.37**	(.44)	1.07*	(.45)
Housing availability[a]					.90*	(.39)
Housing affordability[a]					-.08	(.23)
Population density[a]					.18***	(.03)
Constant	92.8***	(.09)	91.2***	(.49)	90.2***	(.50)
N	5,964		5,964		5,953	
R^2	<.01		.03		.03	

Note. Neighborhoods with values that fell outside of +/- 3 standard deviations of the mean were removed from the model. Models present robust standard errors.

[a] Variable has been transformed through multiplication, offsetting, logging, and/or the use of a square root to produce a more normal distribution suitable for OLS regression.

º $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed)

TABLE 23

Neighborhood Characteristics Associated with the Neighborhood LISA Value of the Rate of RSO Residences

Variable	Neighborhood LISA value[a]					
	1		2		3	
	b	(se)	b	(se)	b	(se)
Existing residence restriction	.00	(.11)	-	-	-	-
Months of residence restriction	-	-	-.01**	(<.01)	-	-
Existing RR 1000' or less	-	-	-	-	-.35*	(.16)
Existing RR over 1000'	-	-	-	-	-.12	(.16)
RR includes 'other' locations	-	-	-	-	.45**	(.14)
Nearby residence restriction	-.05	(.12)	.05	(.10)	-.05	(.12)
Concentrated disadvantage	.17	(.11)	.20º	(.11)	.17	(.11)
Residential instability	.16º	(.09)	.13	(.09)	.19*	(.09)
Ethnic heterogeneity[a]	1.07*	(.45)	.97*	(.45)	1.08*	(.45)
Housing availability[a]	.90*	(.39)	1.06**	(.40)	.89*	(.39)
Housing affordability[a]	-.08	(.23)	-.15	(.23)	-.03	(.24)
Population density[a]	.18***	(.03)	.21***	(.03)	.16***	(.03)
Constant	90.2***	(.50)	90.1***	(.50)	90.2***	(.49)
N	5,953		5,953		5,953	
R^2	.03		.03		.03	

Note. Neighborhoods with values that fell outside of +/- 3 standard deviations of the mean were removed from the model. Models present robust standard errors.

[a] Variable has been transformed through multiplication, offsetting, logging, and/or the use of a square root to produce a more normal distribution suitable for OLS regression.

º p < .10; * p < .05; ** p < .01; *** p < .001 (two-tailed)

TABLE 24

Descriptive Statistics for Variables in Research Question 4

Variable	N	Mean	SD	Min	Max
2009 mean rate of monthly recidivistic sex offenses (child victims)[a]	52	.05	.10	0	.45
2009 mean rate of monthly recidivistic sex offenses (adult victims)[a]	52	.18	.19	0	1.17
Mean county NNA[b]	52	0	1	-1.44	3.15
Revised index of isolation[b]	52	0	1	-.61	5.09
Oden's I^*_{pop}[b]	52	.89	1.58	-.05	9.29
Rate of RSO residences	52	12.07	4.10	1.46	21.26
Concentrated disadvantage	52	.02	.81	-2.59	1.57
Residential instability	52	.03	.82	-1.50	3.41
Ethnic heterogeneity	52	.02	.84	-.96	2.78
Population density	52	192.49	225.70	21.10	1,117.30
County size (square miles)	52	834.36	451.21	206.10	2,685.60
2009 mean monthly robbery rate	52	1.49	1.31	0	5.69
2009 mean monthly burglary rate	52	6.78	2.21	2.35	13.72

Note. Only includes neighborhoods with at least one registered sex offender.
[a] Variable has been converted into a rate per 100,000 county residents to account for the use of an exposure term in the model.
[b] Variable has been standardized to have a mean of 0 and a standard deviation of 1.

TABLE 25

Correlation Matrix for Variables in Research Question 4

Variable (N = 52)	1	2	3	4	5	6	7	8	9	10	11	12	13
2009 mean rate of monthly recidivistic sex offenses (child victims)[a]	1.00												
2009 mean rate of monthly recidivistic sex offenses (adult victims)[a]	-	1.00											
Mean county NNA[b]	.08	.13	1.00										
Revised index of isolation[b]	.03	-.05	-	1.00									
Oden's I^*_{pop}[b]	-.05	-.09	-	-	1.00								
Rate of RSO residences	.27	.07	.10	-.01	-.27	1.00							
Concentrated disadvantage	.09	-.06	.02	.24	.32	.22	1.00						
Residential instability	-.11	-.09	-.24	.23	.28	-.09	.49	1.00					

See notes at end of table.

TABLE 25, continued

Variable (N = 52)	1	2	3	4	5	6	7	8	9	10	11	12	13
Ethnic heterogeneity	-.21	-.10	-.40	.07	.25	-.59	.08	.44	1.00				
Population density	-.12	-.12	-.52	.44	.71	-.46	.24	.33	.45	1.00			
County size (square miles)	.06	-.10	.27	-.01	-.04	.26	.27	.01	-.10	-.26	1.00		
2009 mean monthly robbery rate	-.19	-.02	-.43	.49	.69	-.37	.39	.44	.47	.69	-.08	1.00	
2009 mean monthly burglary rate	.09	.14	.02	.24	.16	.13	.43	.11	-.19	.04	-.17	.28	1.00

Note. Only includes neighborhoods with at least one registered sex offender.
[a] Variable has been converted into a rate per 100,000 county residents to account for the use of an exposure term in the model.
[b] Variable has been standardized to have a mean of 0 and a standard deviation of 1.

TABLE 26

County Characteristics Associated with Recidivistic Sex Offenses Committed Against Child Victims

	2009 mean monthly recidivistic sex offenses committed against child victims					
	1		2		3	
Variable	b	(se)	b	(se)	b	(se)
Mean county NNA[a]	.85	(.21)	-	-	-	-
Revised index of isolation[a]	-	-	1.19	(.17)	-	-
Oden's I^*_{pop}[a]	-	-	-	-	1.09	(.18)
Rate of RSO residences	1.19*	(.10)	1.19*	(.10)	1.21*	(.11)
Concentrated disadvantage	1.24	(.48)	1.34	(.53)	1.18	(.43)
Residential instability	.59	(.34)	.55	(.30)	.62	(.32)
Ethnic heterogeneity	.86	(.30)	.97	(.34)	1.00	(.38)
Population density	1.00	(<.01)	1.00	(<.01)	1.00	(<.01)
County size (square miles)	1.00	(<.01)	1.00	(<.01)	1.00	(<.01)
2009 mean monthly robbery rate	1.06	(.13)	1.03	(.14)	1.06	(.13)
2009 mean monthly burglary rate	.87	(.19)	.81	(.23)	.82	(.29)
N	52		52		52	

Note. Only includes neighborhoods with at least one registered sex offender. The models include an exposure term measuring the county population in 2009 and present robust standard errors.

[a] Variable has been standardized to have a mean of 0 and a standard deviation of 1.

º p < .10; * p < .05; ** p < .01; *** p < .001 (two-tailed)

TABLE 27

County Characteristics Associated with Recidivistic Sex Offenses Committed Against Adult Victims

	2009 mean monthly recidivistic sex offenses committed against adult victims					
	1		2		3	
Variable	b	(se)	b	(se)	b	(se)
Mean county NNA[a]	1.09	(.18)	-	-	-	-
Revised index of isolation[a]	-	-	.89*	(.05)	-	-
Oden's I^*_{pop}[a]	-	-	-	-	.93º	(.04)
Rate of RSO residences	1.03	(.05)	1.03	(.05)	1.01	(.04)
Concentrated disadvantage	1.08	(.26)	1.07	(.24)	1.13	(.26)
Residential instability	.94	(.13)	.95	(.13)	.91	(.13)
Ethnic heterogeneity	.96	(.16)	.91	(.14)	.88	(.14)
Population density	1.00	(<.01)	1.00	(<.01)	1.00	(<.01)
County size (square miles)	.99*	(<.01)	1.00	(<.01)	1.00	(<.01)
2009 mean monthly robbery rate	1.00	(.09)	1.02	(.09)	1.00	(.09)
2009 mean monthly burglary rate	1.05	(.18)	1.09	(.18)	1.11	(.20)
N	52		52		52	

Note. Only includes neighborhoods with at least one registered sex offender. The models include an exposure term measuring the county population in 2009 and present robust standard errors.

[a] Variable has been standardized to have a mean of 0 and a standard deviation of 1.

º p < .10; * p < .05; ** p < .01; *** p < .001 (two-tailed)

FIGURE 1

Theoretical Framework

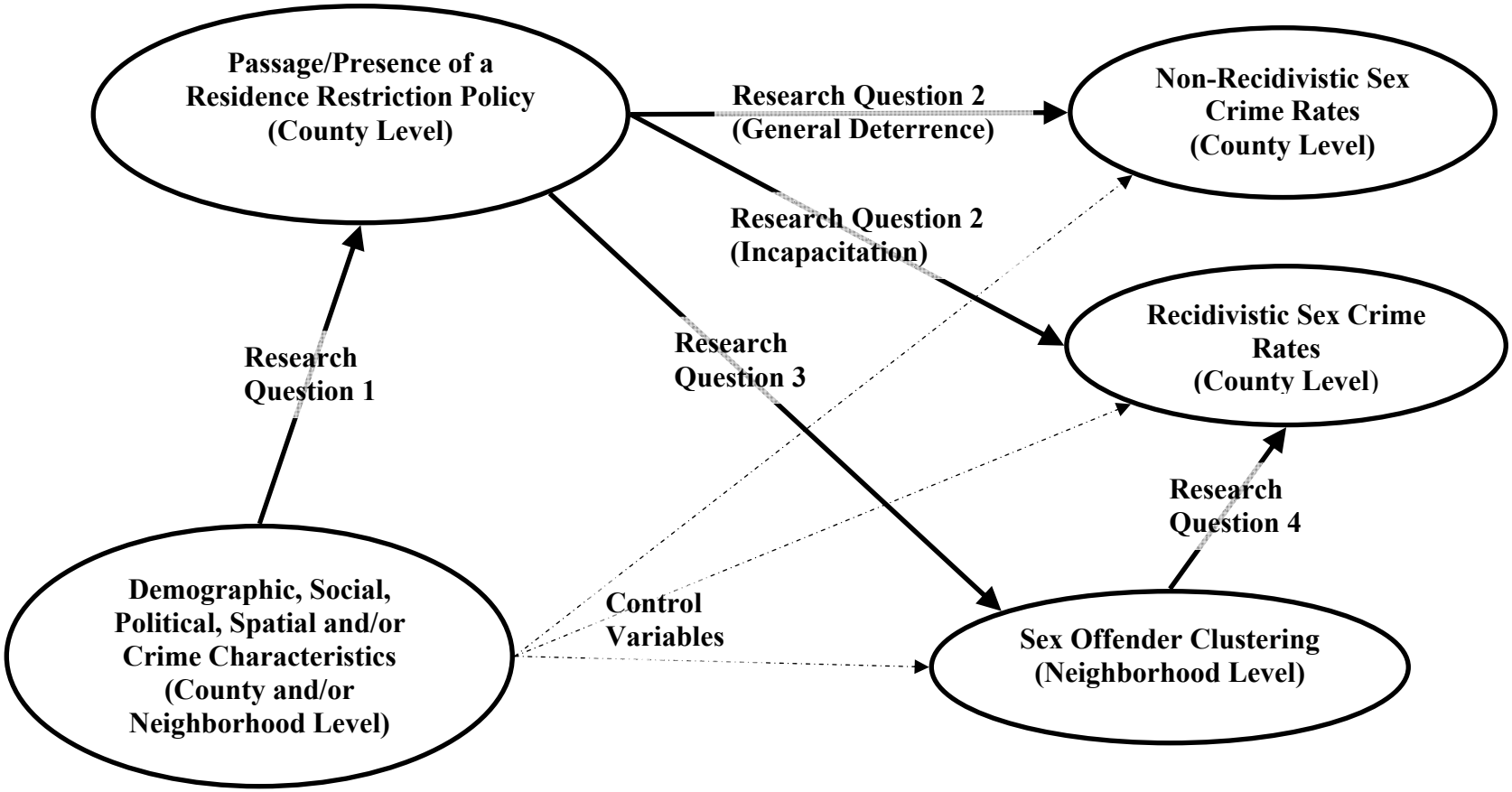
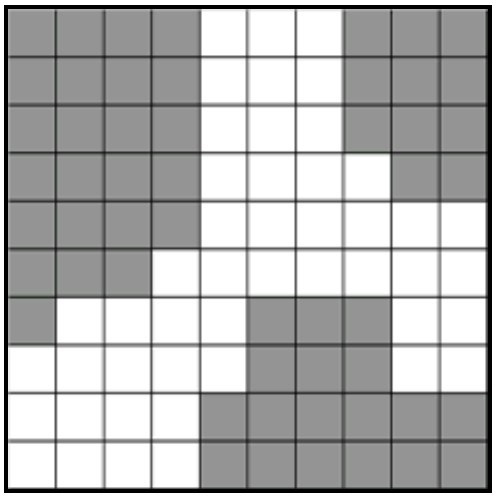


FIGURE 2

Example Oden's I^*_{pop} Values

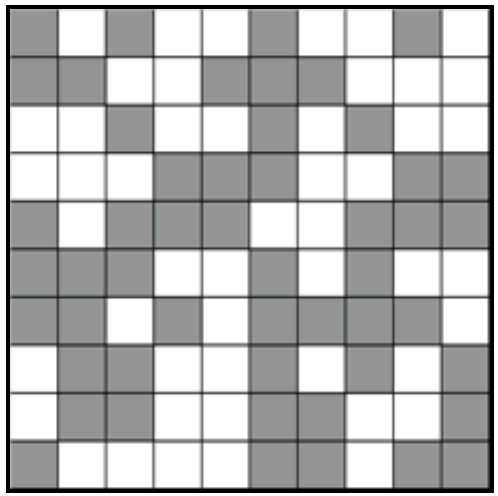
A. Positive I^*_{pop} Value

(Positive Spatial Clustering)



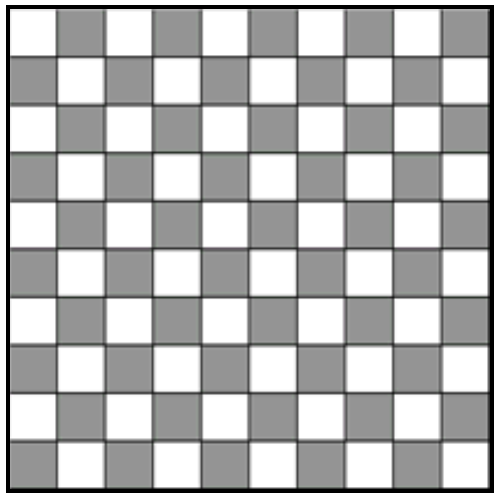
B. Near Zero I^*_{pop} Value

(Spatial Randomness)



C. Negative I^*_{pop} Value

(Negative Spatial Clustering)



KEY:

A neighborhood with a low rate of sex offenders, controlling for the resident population

A neighborhood with a high rate of sex offenders, controlling for the resident population

APPENDIX A

County and Local-Level Residence Restrictions in New York State

County	County law	Local laws	County	County law	Local laws
Albany	X[a]		Niagara	X	X
Allegany			Oneida	X	X
Bronx			Onondaga		X
Broome	X	X	Ontario		X
Cattaraugus		X	Orange	X	X
Cayuga	X	X	Orleans		
Chautauqua		X	Oswego		X
Chemung			Otsego	X	
Chenango		X	Putnam	X	
Clinton		X	Queens		
Columbia			Rensselaer	X[a]	
Cortland			Richmond		
Delaware			Rockland	X[a]	
Dutchess	(b)		Saratoga	X	X
Erie		X	Schenectady	X[a]	
Essex		X	Schoharie		
Franklin	X	X	Schuyler	X[a]	
Fulton			Seneca	X	X
Genesee			St Lawrence		X
Greene			Steuben		
Hamilton			Suffolk	X	X
Herkimer			Sullivan		
Jefferson	X	X	Tioga		X
Kings			Tompkins		
Lewis			Ulster		X
Livingston			Warren	X	
Madison			Washington	X	
Monroe		X	Wayne		X
Montgomery			Westchester		X
Nassau	X	X	Wyoming		X
New York			Yates		

Note. These data were adapted from NY OSOM (2010a), and were current as of December 3, 2009.

[a] No longer valid.

[b] Dutchess County's residence restriction policy requires RSOs to complete and sign a Sex Offender Verification Form, under oath, but does not restrict the locations where RSOs can live. As such, for the purposes of this study, Dutchess County is not considered to have a county-level residence restriction law.

APPENDIX B

Fixed-Effects Panel Model

A fixed-effects panel model is used to analyze whether the presence of a county-level residence restriction influenced sex crime rates. The basic setup of the equation is as follows:

$$Y_{it} = \beta_0 + \beta_1(\text{has RR policy})_{it} + \beta_2(\text{robbery})_{it} + \beta_3(\text{burglary})_{it} + \beta_4(\text{month - year})_t + \beta_5(\text{time period})_{it} + Y_{it-1} + \alpha_i + \varepsilon_{it}$$

Where for the i^{th} county at time t: Y_{it} is the sex crime rate, β_0 is the intercept, $\beta_1(\text{has RR policy})_{it}$ is the coefficient for the presence of a residence restriction policy, $\beta_2(\text{robbery})_{it}$ is the coefficient of the robbery rate, $\beta_3(\text{burglary})_{it}$ is the coefficient of the burglary rate, $\beta_4(\text{month - year})_t$ is a set of indicator variables representing the month-year of the observation, $\beta_5(\text{time period})_{it}$ is a county-specific trend variable, Y_{it-1} is the sex crime rate in the previous month (i.e., at time $t - 1$), α_i is the coefficient of the stable characteristics of the county, and ε_{it} is the random error.

Using a fixed-effects panel model controls for any of the relatively static characteristics of each county (e.g., demographic, social, political, and history) via α_i, while allowing for the estimation of the dynamic characteristics' influence on the sex crime rate. This is important, as the presence of a residence restriction policy is a dynamic characteristic that can vary between counties and between time periods. Including sets of indicator variables for the month-year and a county-specific trend

variable control for autocorrelation and the unknown factors that affect each county similarly and differently, respectively, in each time period (Marvell & Moody, 2008). Finally, including the lagged sex crime rate from the prior month (Y_{it-1}), controls for potential autocorrelation in the sex crime rate from one month to the next and help account for missing variable bias (Marvell & Moody, 2008).

APPENDIX C

Geocoding Sex Offender Addresses[83]

The Geocoding of RSO residential addresses used a three-step process. The first step involved analyzing the entire New York sex offender registry of 18,770 addresses of level 2 and 3 RSOs as of September 21, 2010. Addresses that were either located out of the state, were in a secure facility (e.g., mental institution, jail, prison, etc.), or that lacked a residential address (e.g., homeless or unknown) were removed from the dataset. Shared housing that was not in a secure facility, such as homeless shelters or half-way houses, was not removed from the dataset. This process left 11,760 RSO residential addresses that could then be Geocoded. The 11,760 addresses accounted for about 63 percent of the total number of level 2 and 3 RSOs that had addresses publicly listed on the registry.

In the second step, these 11,760 addresses were then Geocoded using ESRI's ArcView 9.3. The process used an address locator that used a database of individual address points. These address points were based on the centroid location of tax parcels, and covered a large portion of the state. The Geocoding process used a spelling sensitivity of 80 percent, a minimum candidate score of 20, and a minimum matching score of 60, with tied addresses counting as a match. While the address database was not complete statewide, 5,146 RSO addresses were matched to a tax parcel address point, accounting for 43.8 percent of the 11,760 residential addresses.

The third and final step Geocoded the remaining 6,614 unmatched residential addresses using a second address locator based on a statewide compilation of county-level street centerline files, which came from the U.S. Census (2009) Tiger/Line website

[83] The author would like to thank Andrew Wheeler for his helpful comments and suggestions regarding this section.

for New York. This process matched another 6,449 RSO residential addresses to street-based points, leaving 165 addresses unmatched. A quick review of these unmatched addresses indicated that many were actually non-residential addresses, such as in-patient drug treatment facilities, although certainly some of these addresses were actual residential addresses that could not be accurately Geocoded.

It should be noted that the street-based address points are typically less accurate for more rural areas compared to more urban areas. However, given that these data are being aggregated to the neighborhood and county level, these inaccuracies are not expected to affect overall conclusions. Further, while more urban counties appeared to have slightly better Geocoding success rates (analysis not shown), the differences in Geocoding rates across counties were not enough to warrant concern. For excellent discussions of the limitations of Geocoding mechanisms, see Hughes and Kadleck (2008), Zandbergen (2008), Zandbergen and Green (2007), and Zandbergen and Hart (2009c).

Overall, of the 18,770 RSO addresses publicly listed on the New York sex offender registry on September 21, 2010, there were 11,760 apparently valid residential addresses. Of these 11,760 RSO residential addresses, about 98.6 percent (11,595) were able to be matched to either a parcel-based address point (5,146) or a street-based address point (6,449). According to the existing literature, a Geocoding success rate of above 80 percent, and preferably above 90 percent, is considered standard and acceptable (e.g., Berenson & Appelbaum, in press; Clontz & Mericle, 2004; Hipp, Jannetta, Shah, & Turner, 2008; L. A. Hughes & Kadleck, 2008; Red-Bird, 2009; Zandbergen & Hart, 2009c; but see Grubesic, et al., 2007, 2008).

APPENDIX D

Calculating Excess Risk

The excess risk for a neighborhood in this study is calculated as follows:

$$Risk = \frac{RSO_n}{POP_n} - \frac{RSO_c}{POP_c}$$

Where RSO_n is the number of RSOs in a neighborhood, RSO_c is the number of RSOs in the county, POP_n is the resident population of the neighborhood, and POP_c is the resident population of the county. Values above zero indicate a neighborhood has more RSOs than is otherwise expected given a homogenous distribution of RSOs between neighborhoods based on the population distribution of residents among neighborhoods. Values below zero indicate a neighborhood has fewer RSOs than is otherwise expected.[84]

[84] This measure can be thought of as a modification of the P* ("p star") value, but is focused at the *individual* neighborhood level rather than at the *aggregate* neighborhood (i.e., county) level. For more information about the P* value, see the works of Bell (1954), Lieberson & Carter (1982) and Poston & Micklin (2006).

APPENDIX E

Calculating the Revised Index of Isolation

The formula for calculating the revised index of isolation (R.I.I.) for a given county is as follows:

$$R.I.I._c = \frac{\sum_{i=1}^{n}\left(\frac{RSO_i^2}{POP_i}\right) / RSO_c - \left(\frac{RSO_c}{POP_c}\right)}{1 - \frac{RSO_c}{POP_c}}$$

Where RSO_i is the number of RSOs in neighborhood *i*, RSO_c is the number of RSOs in the county *c*, POP_i is the resident population of the neighborhood, and POP_c is the resident population of the county. With this measure, values of 0 indicate no between-neighborhood clustering (e.g., the population of RSOs are homogenously mixed between neighborhoods), while a value of 1.00 indicate complete clustering (where all neighborhoods are composed of either all RSOs or no RSOs). As such, higher values indicate a county with relatively more clustering of RSOs between neighborhoods.[85]

[85] Poston and Micklin (2006) note that this measure can link aggregate-level segregation to individual-level spatial attainment models. Spatial attainment models are essentially measures of the extent that one group has assimilated into an area as predicted by measures of individual-level characteristics such as education or income. For the purposes of this study, the important individual-level characteristic is consistent across all RSOs and is simply a prior conviction for a sexual offense, while the main difference between areas is the presence of a residence restriction policy. For more information on spatial attainment models, see Masey and Denton (1985) and Waren (2008).