Sex Offender Law and the Geography of Victimization

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Sex offender laws that target recidivism (e.g., community notification and residency restriction regimes) are premised—at least in part—on the idea that sex offender proximity and victimization risk are positively correlated. We examine this relationship by combining past and current address information of registered sex offenders (RSOs) with crime data from Baltimore County, Maryland, to study how crime rates vary across neighborhoods with different concentrations of resident RSOs. Contrary to the assumptions of policymakers and the public, we find that, all else equal, reported sex offense victimization risk is generally (although not uniformly) *lower* in neighborhoods where more RSOs live. To further probe the relationship between where RSOs live and where sex crime occurs, we consider whether public knowledge of the identity and proximity of RSOs may make offending in those areas more difficult for (or less attractive to) *all* potential sex offenders. We exploit the fact that Maryland's registry became searchable via the Internet during our sample period to investigate how laws that publicly identify RSOs may change the relationship between the residential concentration of RSOs and neighborhood victimization risk. Surprisingly, for some categories of sex crime, notification appears to *increase* the relative risk of victimization in neighborhoods with greater concentrations of RSOs.

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I. Introduction

Understanding the relationship between where criminal offenders live and where crime occurs is important to understanding criminal behavior and, increasingly, to the design of effective criminal justice policies. Policymakers have recently become enamored with using offender location information not only to track or control the movement of individuals at risk of committing crime, but also to inform the public of the location of potential threats so that people may be more likely to remain vigilant and even proactively protect themselves. Sex offender notification laws in particular are designed to inform at-risk individuals of where previously convicted, registered sex offenders (RSOs) reside. These laws assume that (1) the risk of being victimized is positively correlated with geographic proximity to an RSO, (2) potential victims can use RSO identifying information effectively to reduce their risk of victimization, and (3) notification reduces overall victimization risk near RSOs.

Sex offender registration and notification laws rely critically on the idea that the police and public can employ information about where RSOs *reside*—supposedly, the danger zones—to prevent future crimes. For decades, policymakers have contended that providing RSO residence information to the police, or publicizing the information via the Internet, will reduce overall levels of RSO recidivism. The literature on the consequences of these laws, however, suggests fairly consistently that making RSO information public fails to reduce recidivism—and may in fact increase it (e.g., Agan 2011; Prescott & Rockoff 2011). One possible explanation for this pattern is that while publicly identifying sex offenders seems likely to exacerbate recidivism risk factors (e.g., unemployment, poor housing, and a paucity of stable relationships), the residence information provided to the public is not very useful to potential victims because, for a variety of reasons, sex offenders rarely offend near their own homes.

In this article, we assemble comprehensive geographic data on crimes reported in Baltimore County, Maryland, and a history of registered addresses of all RSOs in Maryland, to explore the empirical interplay between sex offender residency, notification laws, and reported victimization risk. Our data are unique in that they contain both the current and past home addresses of all RSOs, allowing us to create movement histories and make an accurate count of RSOs living in a particular neighborhood at a particular time. Our neighborhood-by-month panel runs from February 2001 through December 2008. Our analysis also exploits a significant and plausibly exogenous shock to Maryland's legal regime for regulating its convicted sex offenders. In April 2002, Maryland's Department of Public Safety and Correctional Services made a functional, searchable Internet registry available for public use. RSOs, already known to the police as a result of registration requirements, suddenly became known to the public at large during our study period.

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These crime and registration data and the legal regime change allow us to study several important policy-relevant questions. First, we examine whether areas where more RSOs reside experience more sex crime relative to areas with fewer resident RSOs. Although the picture is complicated, we find evidence of a generally negative relationship: locations with more RSOs experience relatively *fewer* sex offense incidents, an empirical pattern starkly at odds with public perceptions and the key assumption underlying sex offender notification laws. Second, we explore whether broad public knowledge of the identity and location of RSOs alters the relationship between the number of sex offenders living in an area and the risk of victimization in that neighborhood, exploiting the fact that the Maryland Sex Offender Registry became searchable online during our sample period. Unexpectedly, we find evidence that public awareness of resident RSOs may *increase* the relative number of certain reported sex crimes in those areas with relatively more resident RSOs.

The idea that victimization risk and offender proximity are correlated emerges from a large theoretical literature (with supporting empirical work) that examines how criminals "journey to crime" (Rossmo 2000). According to the routine activities approach (Cohen & Felson 1979), for instance, crime occurs when a motivated offender bumps into a suitable victim in a situation in which a capable guardian happens to be absent. In a similar vein, the rational choice approach envisions an offender who selects (perhaps subconsciously) the locations for his offenses by comparing relevant costs and benefits (Cornish & Clarke 1986; Elffers 2004). These and related environmental theories take as given that an offender's residence is *relevant* to where he will commit crime, and while patterns vary across types of offenses and the demographic characteristics of offenders (Bernasco & Block 2009; Canter & Gregory 1994), distance decay remains the received wisdom: offenders of all varieties (and for intuitive reasons) *generally* commit fewer crimes farther from home.

But the empirical conclusion drawn by policymakers and the public that sex offenses will occur disproportionally where RSOs reside does *not* follow from the fact that nearby victims, all else equal, are easier and quicker to locate. Neither does the logic of distance decay imply that the public announcement of where RSOs live will reduce the public's relative risk of victimization. These intuitive inferences are flawed for two reasons. First, while the costs and constraints of travel and unfamiliar places *generally* reduce crime, certain classes of offenders may employ buffer zones between their residence and their criminal activity (Brantingham & Brantingham 1984). Crime displacement of this sort may be particularly relevant in the sex offender context (Duwe et al. 2008; Barr & Pease 1990). Second, sex offender laws only operate (directly) on RSOs (i.e., those who have been convicted in the past and are therefore known to law enforcement), yet unregistered, first-time sex offenders (NonRSOs) as a group commit a

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large majority of reported sex offenses (e.g., Sandler et al. 2008; Moore 2006). Therefore, it stands to reason that if sex offender laws *also* influence where NonRSOs choose to live and offend, victimization rates could well be counterintuitively lower near where RSOs reside.

The article proceeds as follows. In Section II, we outline the institutional and legal environment for our empirical work. We then explain our conceptual approach to thinking about an offender's decision to offend near where he lives or elsewhere, situating the discussion in the existing literature. In Section III, we introduce our data. Section IV describes our primary empirical strategies. Section V reports and discusses our results. In Section VI, we present extensions to and robustness checks of our analysis. We conclude in Section VII.

II. Legal Background, Conceptual Framework, and Literature

People dread living near criminal offenders, particularly registered sex offenders (e.g., Aleksander 2010). The mere fact that policymakers have demanded the dissemination of residential information of RSOs since the 1990s evinces a social belief that proximity to an RSO increases one's overall risk of victimization (Bandy 2011).[1] Designed to make it costly and difficult for these potential offenders to attack victims who live nearby, particularly children, notification laws are founded on the idea that an RSO's address actually *reveals* where that potential offender is most likely to commit an offense in the future (Duwe et al. 2008) and that government may be able to do something about such risk by making that information available to those who are most vulnerable.

II.A. Registration and Notification Law

Sex offender registration and notification laws spread across the country in the 1990s, a movement sparked in part by public outrage over crimes committed by previously convicted sex offenders and, more directly, by federal laws providing strong incentives for states to adopt them. The Jacob Wetterling Act (1994) pressed states to build confidential registries of sex offender information, in theory giving local law enforcement the information needed to better

[1] A plausible, but ultimately unconvincing, alternative interpretation exists: perhaps it is not that the public and policymakers believe that proximity to RSOs results in higher victimization risk, but rather that sex offender identifying information can allow victims, through precaution taking, to reduce the risk below what it would otherwise be. This possibility is belied by the facts that homes near RSOs sell for less than comparable homes elsewhere and that adjacent home values decline sharply when an RSO moves into a neighborhood (e.g., Linden & Rockoff 2006; Pope 2008). These market indications suggest that perceived risk levels may be higher where convicted sex offenders reside. Stronger evidence against this alternative possibility is that in addition to notification laws, many states have implemented residency restrictions stipulating that RSOs cannot live within a certain distance (e.g., 1,000 feet) of schools, community centers, and bus stops, or other places where children congregate. Here, the clear assumption is that proximity equals risk.

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monitor and, if necessary, more easily apprehend convicted sex offenders. Megan's Law (1996) pushed states to make their private registries public, so that anyone would be able to use sex offender registry information to identify threats and thus better protect themselves and others. With respect to both federal laws, states had (and used) considerable discretion in the timing and features of the registration and notification laws they were expected to pass.

Maryland, the source of our data, began to transform its sex offender policies in 1995 with Senate Bill 79, which proposed requiring convicted sex offenders who had victimized children to register for 10 years with a local law enforcement agency. The bill was enacted and implemented (Gillette 1996), ultimately codified as Md. Ann. Code Art. 27.692B. Before the end of the 1990s, all convicted sex offenders (with the exception of a class of offenders convicted of certain offenses committed prior to July 1, 1997) were required to register with police, and the state had created procedures by which a citizen, in writing, could request information about a registrant and by which a law enforcement agency, in its discretion, could provide information in response to such requests.

In the middle of 1999, Maryland promulgated Md. Code Ann., Crim. Proc. § 11-717, a law that provides that "[t]he Department [of Public Safety and Correctional Services] *may* post on the Internet a current listing of each registrant's name, crime, and other identifying information" (emphasis added).[2] Although the law became effective at the beginning of October 1999, the Internet registry was not available for use until April 2002 (Md. Code Ann., Crim. Proc. § 11-717; The Daily Record 2002). On the day the website became active in mid-April 2002, identifying information for all RSOs (more than 2,000) became instantly available using a free Internet search (The Daily Record 2002).[3]

After that date, only small changes in the scope, duration, and procedures related to registration and notification followed until June 22, 2006, when Maryland implemented sex offender residency and employment restrictions. Maryland's residency restrictions, however, were narrow and discretionary, nothing like what many other states have since imposed (see Md. Code. Ann., Crim. Proc. §§ 11-722–11-724). The 2006 law allowed a parole supervision officer, at the discretion of the Parole Commission, to subject a registered offender to certain restrictions

[2] The 1999 legislation also required that child sexual and sexually violent offenders who had committed certain offenses as well as "sexually violent predators" register for life rather than just 10 years.

[3] Our research suggests that because the 1999 statute used permissive language ("may," not "shall"), the Maryland Department of Public Safety and Correctional Services chose to conduct a feasibility study and receive public comments before deciding whether to make registry information available electronically. Once the Department determined to proceed with an Internet registry, the political branches had to allocate funding for the upfront and yearly maintenance costs, the website and related procedures had to be developed, and so forth (Seergae 2014; The Daily Record 2002).

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on where he or she could live. Anecdotal evidence suggests that this option was rarely used (Cunningham 2009). Given that the law was enacted in the last few years of our sample, and because it appears to have affected so few registered offenders, it is unlikely to confound our analyses or conclusions.

II.B. Conceptual Framework

With just a few exceptions, the theoretical and empirical research on the geography of sex offending behavior, which we describe briefly in Section II.C, has focused on the distance offenders are likely to travel when they seek to commit sex crime and what factors contribute to the selection of a particular location. The central difference between these questions and the questions we examine in this article—whether neighborhoods with more RSOs experience more sex crime and whether this relationship is different if RSOs are known only to the police or also to the public at large—is the recognition that registration and notification laws may cause not only RSOs *but also NonRSOs* to engage in very different spatial patterns of criminal activity and, as groups, even to live apart from each other. Below, to frame our empirical work, we develop this idea in more detail, beginning by asking the foundational question of what drives an offender to choose a particular location to commit a crime.

We assume that both RSOs and NonRSOs choose whether and where (or against whom) to commit a sex offense by consciously or subconsciously weighing the costs and benefits of their options. For simplicity, we also assume that RSOs and NonRSOs benefit equally on average from committing crime and that the punishment for any offense, if the offender is caught and convicted, does not depend on the offense's location. As a result, conditional on deciding to commit a crime, an offender will select a location on the basis of (1) the relative probability that the offender will be caught (and convicted) at the location and (2) the difficulty of traveling to and carrying out the crime at the location. As we show below, *both* considerations may be heavily influenced by whether registration or notification laws are in place in the jurisdiction and, if so, whether the incipient offender in question is subject to them.

In the abstract, travel costs and the difficulty level of locating an appropriate victim and of committing a sex crime are increasing in the distance from the offender's home. Traveling is costly and takes time, and unfamiliar locations make successfully planning and carrying out crime more difficult. But, consistent with the notion of buffer zones, we also posit that the probability that a sex offender is identified and apprehended is *decreasing* in the crime's distance from the offender's home, making the overall attractiveness of offending near one's home unclear. Evidence from existing literature (see Section II.C), at least, suggests that buffer zones in the sex offense context are significant in size.

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Against this backdrop, sex offender laws seem likely to dramatically increase the attractiveness to RSOs of offending away from home. Registration laws require RSOs (but only RSOs) to provide local authorities with their addresses and other identifying information, increasing the relative probability that an RSO will be apprehended if he offends in his neighborhood. Notification laws magnify this effect by making this registration information available to the public, increasing the likelihood that an RSO will be identified by a victim or a witness and, as a result, apprehended if he attempts to commit a crime in his neighborhood. Furthermore, notification information allows potential victims to better protect themselves from attack by RSOs who live nearby, making it more difficult for an RSO to carry out a crime near home.

These consequences seem to imply that victimization risk will be relatively low for the neighbors of RSOs under these laws. Such a pattern would appear consistent with registration and notification laws displacing RSO sex crime or even reducing RSO recidivism. However, this analysis ignores the threat posed by NonRSOs, and therefore conflates *overall victimization risk* with the risk of being victimized by a nearby RSO.[4] Most reported sex crime is committed by NonRSOs (Sandler et al. 2008), so overall victimization risk near RSO homes will depend critically on the behavior of NonRSOs and, in particular, on whether these individuals become more likely or less likely (or neither) to offend near RSO homes when registration and notification laws are in place.

Although registration and notification laws apply only to RSOs, the information provided by these laws (as well as the behavioral responses of the police and victims to this information) may influence the locational offending choices of *all* potential sex offenders. These influences may interact in offsetting or compounding ways that may ultimately render the vicinity immediately adjacent to RSO homes either safer or more dangerous than elsewhere. For instance, NonRSOs may find areas where RSOs live to be *less* attractive as places to commit crime (perhaps because potential victims living there are more cautious, despite Bandy's (2011) evidence to the contrary) or, alternatively, NonRSOs may find them to be *more* attractive (perhaps because law enforcement attention is directed to the "usual suspects"—i.e., RSOs—in the area). Furthermore, if NonRSOs avoid targeting areas where RSOs are known to reside, the costs of travel, unfamiliarity, and time may cause NonRSOs to consciously or otherwise live apart from RSOs so they may more easily reach places with fewer "capable guardians" and more "suitable targets"

[4] The risk of being victimized by a nearby RSO might be lower for someone who lives a building away from the RSO in question relative to a potential victim who lives a mile away. But, *if* we assume RSOs are evenly distributed across neighborhoods and hold all else equal, the overall risk of being victimized by an RSO should not vary geographically. Thus, whether buffer zones and sex offender laws reduce *overall* victimization risk near where RSOs live may turn on the spatial distribution of these RSO homes.

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(Cohen & Felson 1979). Walker et al. (2001) find evidence that sex offenders tend to live closer to concentrated populations of potential victims, suggesting that the spatial distribution of sex offender homes (in this case, all sex offender homes) may matter a great deal to whether victimization risk is higher or lower for neighbors of RSOs.

With the addition of unknown offenders to the analysis, the possible consequences of sex offender laws for victimization risk levels near RSO homes become more complicated. In fact, notification's effects on the relative level of victimization risk in neighborhoods with more RSOs may not only differ in magnitude but may also point in a different direction than a law that simply requires convicted offenders to register with the police.

Registration policy, through heightened police monitoring, increases the probability that an RSO is caught for crimes perpetrated near his home, potentially displacing or deterring offenses he might otherwise have committed. Registration, however, seems unlikely to affect NonRSO spatial patterns of offending.[5] In theory, if RSOs are more likely to travel, every neighborhood RSO could be replaced by one traveling from another neighborhood, leaving the relative victimization risk everywhere unchanged. By contrast, if RSOs tend to cluster, longer journeys to crime caused by registration would result in below-average victimization risk near the homes of RSOs as more RSOs leave the neighborhood to offend than arrive.

Notification further complicates these dynamics. If anyone can search online for the whereabouts of RSOs, the behavior of potential victims, RSOs, and NonRSOs may all change, leading to ambiguous consequences for RSO neighborhoods. On one hand, RSOs, once publicly known, seem even more likely to offend outside of their neighborhoods. Individuals living in neighborhoods with RSOs may undertake more precautions and may monitor their neighborhoods more effectively (or at least be more likely to report crimes). On the other hand, notification could make offending more attractive to RSOs across the board (either by exacerbating recidivism risk factors like unemployment or by reducing the effective severity of punishment). If victim precaution taking is relatively ineffectual and travel is difficult, RSOs may become more likely to attack near home. For their part, NonRSOs may now use notification information (or signs of victim precaution taking) to seek out particular neighborhoods—either with more or fewer RSOs—to reduce their own probability of detection. If RSO neighborhoods become more attractive targets to NonRSOs because the police are likely to focus any

[5] The threat of future registration might deter some NonRSOs from offending, although there will be little effect on the geography of their crimes because these NonRSOs are by assumption unaware of where RSOs live. Prescott and Rockoff (2011) find no evidence of deterrence. Similarly, under a registration regime, victims remain unaware of whether they live near an RSO, and so victim behavior will also not affect NonRSO offending patterns.

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investigation on RSOs or because victim precautions against RSOs make them more vulnerable to NonRSO offenses, relative victimization risk could increase near RSO homes.

The relative importance of these effects and thus the consequences of registration and notification laws are likely to vary (potentially nonlinearly) with the *total number* of RSOs in a neighborhood. For NonRSOs, the probability of being caught may be decreasing in the total number of RSOs in a neighborhood because there may be safety (from apprehension) in crowds: policing may be more costly or difficult, and therefore less effective at the margin, wherever the proportion of local RSOs is high. Alternatively, *if* information about the identities (and number) of RSOs in a neighborhood is made public, the difficulty of finding a suitable victim (because of additional precaution taking) may increase with the total number of RSOs, reducing the relative attractiveness of high-RSO locations to NonRSOs as places to commit crime.

Importantly, the relative costs and benefits of offending in an RSO neighborhood, and the effects of registration and notification laws on the decision where to offend generally, may vary for different types of sex offenses. Some sex offenses are unlikely to be affected by victim precaution taking or reporting, but are *very* likely to be responsive to police monitoring (e.g., prostitution and pornography). Child sex offenses occur more often between adults and children who know each other; thus, geographic closeness is an especially salient aspect of such crimes. Certain categories of sex crimes may be more impulsive on average, while others may involve more planning behavior. As a consequence of these differences and the theoretically ambiguous predictions of our conceptual framework, it seems plausible that relative victimization risk near RSOs may be higher for some crimes and lower for others, and that the implementation of notification may increase this risk for some crimes and reduce it for others.

On the whole, and assuming some residential clustering of sex offenders, our conceptual framework suggests that relative sex offense victimization risk ought to be lower (on average) near RSO addresses under a registration regime. The effects of Internet notification on relative victimization risk near RSO homes, however, are more difficult to predict. Moreover, the nature of any relationship may differ depending on the number of nearby RSOs and the type of sex offense under consideration. In Section III and beyond, we explore these possibilities with our data.

II.C. Existing Literature

Little work has directly addressed whether neighborhoods with more RSOs actually experience more sex crime or whether neighbors of RSOs are more at risk of victimization after controlling for potentially confounding neighborhood characteristics. The effect of sex offender

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notification laws on this relationship has also received scant attention. Given the social costs of sex offender laws, a systematic assessment of whether RSO residency is correlated with victimization risk seems essential. The dearth of research on these basic relationships is even more surprising in light of the fact that in deciding where (and where not) to reside, many thousands of people per day appear to use whether there are nearby RSOs as a proxy for the relative sex offense victimization risk of a neighborhood.

A few papers have made strides on these questions, however. Agan (2011) attempts a version of this analysis with Washington, DC, data. Unfortunately, she only has a snapshot of the current registry (post-notification) and thus has to make strong assumptions, including that RSOs did not move over time, something we know from the Maryland data is unlikely to be true. She finds no statistically significant relationship between the number of RSOs in a Census block group (CBG) and crime, both before (using her stable residency assumption) and after DC implemented online community notification. Another paper, Tewksbury et al. (2008), uses cross-sectional data from Jefferson County, Kentucky, and finds no statistically significant relationship between the number of RSOs in large Census tracts and the frequency of sex crime during a single year.

We improve on and extend this small literature by studying much richer panel data containing current and past addresses and precise crime locations over time, by employing diverse methods and approaches, and by more rigorously exploring the effect of notification on the relationship between the number of RSOs in a neighborhood and the neighborhood's relative risk level for different categories of sex crime.

Even in the absence of sex offender laws (or in a world in which these laws have no behavioral consequences), research suggests that relative victimization rates may still be lower than average near where RSOs live if buffer zones more than compensate for distance decay in the areas near RSOs that we consider at risk. The evidence that emerges from the journey-to-crime research makes clear that this is an open empirical question.

Warren et al. (1998:55), for instance, identify an "area around the important anchor point of the offender's home that contains a lower probability of rape behavior." In their data, offenders traveled 3.14 miles to commit a crime on average; offenders with more extensive criminal histories (i.e., convicted offenders), however, tended to travel farther, perhaps because they were more wary of detection. The buffer zone, the "space over which offending becomes *more* probable as the distance from home increases," was also over three miles, a range outside what most people appear to consider the typical at-risk zone, at least according to hedonic evidence from housing markets (see, e.g., Linden & Rockoff 2006:1121). Even more telling is the fact that

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four out of the five searchable distances available on Maryland's current Internet registry would fall *within* (one-quarter, one-half, one, three, and five miles) this buffer zone.

Davies and Dale (1996) and Canter (1996), among others, similarly find evidence of offenders employing buffer zones.[6] Duwe et al., citing some of this same evidence and presenting their own, contend that "for violent offenders (including sex offenders), [the distance decay] pattern typically does not hold. *Confrontational offenders*—who actually encounter their victims personally—seek offending locations where they are unlikely to be recognized (and therefore apprehended)" (2008:487). In the context of their recidivist sex offender data from Minnesota, they report that:

> Even when offenders established direct contact with victims, they were unlikely to do so close to where they lived. . . . largely because of the fact that offenders are more likely to be recognized within their own neighborhoods [citing Levenson & Cotter 2005]. [W]hen direct-contact offenders look for a victim, they may be more likely to go to an area relatively close to home (i.e., less than 20 miles) but still far enough away (i.e., greater than 1 mile) to decrease the chances of being recognized. These findings fit with previous research showing that repeat sex offenders typically offend outside their immediate neighborhoods (2008:500).[7]

For the same reason distance decay does not imply that sex offense victimization risk should be higher near registered offenders, buffer zones, even large ones, do not imply that this risk will be lower. Sex offense risk does not arise solely from the possibility of recidivism (indeed, recidivism makes up a small portion of total victimization risk), and if the residency decisions of RSOs provide information about the residency or offending patterns of NonRSOs, or sex offender laws change the behavior of these groups in complicated ways, many potential outcomes are possible.

III. Data Sources

We combine data on the history of registered offenders' addresses from Maryland, crime locations from Baltimore County, and the history of Maryland sex offender laws (discussed in

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[6] Davies and Dale find that 75 percent of attacks by stranger rapists took place within five miles of the offender's home, but they also conclude that as many rapes occurred outside of five miles as occurred within 0.5 miles, suggesting highly dispersed recidivism patterns. Moreover, they report evidence that hints at displacement, noting that one offender, "having realized that the police suspected him of a rape, . . . significantly increased the length of the journey to areas where he prowled, escaping arrest for some considerable time" (1996:153–54). Canter studies serial rapists in London. He interprets his data as favoring a marauder model of behavior (in which the attacker travels from a single base, usually the residence, typically moving out from that base in a circle), but also finds that "[t]he average minimum distance of crime to home for these offenders was 1.53 miles. . . . There is . . . strong evidence for a minimum distance that the sexual offender is willing to travel from home, in accordance with the hypothesized desire to be at a safe distance away from home. The criminal's 'safe area' for activity . . . is at least 0.61 miles from home" (1996:224–25).

[7] Duwe et al. (2008) studied sex offenders who were already required to register. Whether NonRSOs have similar behavioral patterns is uncertain, as our conceptual framework illustrates.

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Section II.A) to study two key questions that emerge from our conceptual framework:[8] (1) whether victimization risk is relatively high in neighborhoods with more RSOs, and (2) whether this relationship changes if information about RSOs previously known only to the police is made available to the public as the result of a notification law. These questions have policy significance because notification proponents contend that publicly identifying RSOs allows potential victims to counter the assumed-to-be elevated levels of risk through avoidance behavior, *reducing* victimization risk near where RSOs live.

III.A. Registered Sex Offender Address Data

Our approach to examining these questions requires the home addresses of RSOs in a particular jurisdiction, and not just where those offenders live presently, but a record of when and where they lived over a significant period of time.[9] From the Maryland Sex Offender Registry Unit, we obtained (via a Public Information Act request) all known current and past addresses for all sex offenders *ever* registered in the State of Maryland,[10] the date on which a new address was registered, the offender's name, the offender's date of birth, and the offender's date of first registration. The address data are left censored at February 2, 2001—the date on which the Unit began using its current electronic system for tracking offenders—and so all our analyses take early 2001 as a starting point.[11] We received the data in October 2010, and the last report date included occurred on July 8, 2010.

[8] To evaluate our conceptual framework directly, we would need home address information for all RSOs and NonRSOs as well as data describing the locations at which these specific individuals committed crime. Data of this kind are unavailable. Even if we were to limit our analysis to criminal activity reported to law enforcement, obtaining reliable information on where a first-time offender lived or worked at the time he committed his crime—even when the individual is arrested, charged, and convicted—is almost always out of reach for researchers.

[9] Maryland also requires that RSOs register any work address; however, we do not include those addresses (or the role they might play) in our analysis. The primary reason for this choice is that, at least in the data we were able to acquire, there are simply too few of them. Of all the addresses we received for the State of Maryland, fewer than 5 percent were "work" addresses—fewer than 2,000 in the entire state over our full sample period—presumably because many RSOs are unemployed. Only a few hundred of these work addresses are likely to have been in Baltimore County. In addition, our data tell us nothing about the nature of any reported employment (i.e., whether the employment was part time, full time, or seasonal) or whether an RSO's duties were actually performed at the address registered. Nevertheless, routine activity theory and common sense highlight the potential importance of where RSOs spend their time during the day in understanding offending patterns, and we are hopeful that future research will examine this possible association.

[10] Maryland was unique among the states that we contacted in that it maintained and allowed dissemination to researchers through Public Information Act requests the current address and all past addresses of the state's RSOs.

[11] More precisely, we have each registered offender's residential address information as of February 2001 and movement information for any RSO from that month until July 2010, but we have no information on where any offenders lived prior to early 2001.

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The historical address data for registered offenders for the entire State of Maryland include 9,146 RSOs and 46,193 total address reports (i.e., either an address or an indication that an offender had been incarcerated, had become homeless, or that prior information had been determined to no longer be current). Over 1,542 offenders report having lived in Baltimore County for at least some time (while not in detention) with a total of 4,123 valid (nondetention) address reports from that jurisdiction.[12] Sex offenders in our Baltimore County data have an average of 2.67 address reports,[13] a number consistent with research indicating that sex offenders change residences frequently and are more likely to be homeless (Duwe et al. 2008; Mustaine et al. 2006a, 2006b).

III.B. Sex Offense and Other Crime Data

From Baltimore County's police department,[14] we procured all known federal Uniform Crime Report Part I crimes and Part II sex offenses reported to law enforcement from the early 1990s through part of 2009.[15] Part I crimes include aggravated assault, forcible rape, murder, robbery, burglary, larceny (theft), motor vehicle theft, and arson. Part II sex offenses include sex offenses against adult victims (first through fourth degree), sex offenses against child victims (first through fourth degree), peeping tom violations, pornography offenses, prostitution crimes, and, finally, child abuse and statutory rape.[16] In addition to the type of offense, we know the date, time, and exact location of the incident report.

The dependent variable in our analyses is the number of crimes in a neighborhood during a month, with the number of sex offenses being the main outcome of interest. In addition to studying the total number of sex offenses, we group sex offenses into five other (overlapping) categories in order to further investigate our baseline findings: (1) rape (Part I) offenses, (2) all nonrape (Part II) sex offenses, (3) sex offenses against adults (excluding rape), (4) sex offenses

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[12] By valid, we mean that after cleaning, we were able to match 4,123 of the registered offender addresses that appear to be in Baltimore County to actual residential locations using GIS software, a match rate well over 80 percent. In Figure 2, we plot all Baltimore County matched addresses, including detention facility reports, which total to 4,988 points. See our Appendix for more details.

[13] Our panel of registered offender addresses is unbalanced. Although a number of offenders were present in the data as of February 2001, many entered over the course of our nine-year sample, so the 2.67 average applies to the shorter-than-nine-year average registration period of RSOs in our sex offender address data.

[14] Baltimore County surrounds Baltimore City but does not include it. Our data therefore do not include crimes reported in Baltimore City.

[15] The crime data we received for part of 2009 were only partially complete, and we determined they were not reliable. As a result, our analysis sample only includes data through the end of 2008.

[16] In our analysis, we do not consider (consensual) statutory rape or child abuse because these crimes are not the focus of sex offender laws. There are very few of these crimes in our data, in any event, and their exclusion does not affect any of our results.

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against children, and (5) peeping, pornography, and prostitution (PPP). We also group other crimes into violent crimes (aggravated assault, murder, and robbery) and nonviolent crimes (burglary, theft, motor vehicle theft, and arson). We use counts of these offenses as alternative outcome variables to see if additional RSO residents are associated with changes in crime in the neighborhood generally (and also as controls in unreported analyses) to ensure that general trends in crime do not drive our findings.

III.C. Additional Control Variables

All our regressions account for neighborhood characteristics that are fixed over time, for seasonal patterns, and for general time trends. To address the possibility of demographic changes within neighborhoods over time and, potentially, for the availability of certain types of victims during the day, we incorporate into our work annual Baltimore County elementary school demographic information.[17] To capture changing levels of neighborhood poverty and other forms of social disorganization, we use the percentage of students enrolled at neighborhood schools who were eligible to receive reduced-price or free lunches. In our Census block group analyses, to measure possibly changing neighborhood demographics (particularly the possibility that RSOs live in neighborhoods with few children or with family structures that are different in potentially confounding ways), we include the ratio of pre-K through fifth-grade students enrolled in neighborhood schools (not necessarily residents) to the total population of the neighborhood (using 2000 Decennial Census data).

III.D. Geocoding the Data

To geocode the RSO address and crime location information,[18] we employed a composite address locator to match each address to a set of latitude and longitude points. (For a complete description of this work, please see our Appendix.) We used these coordinates to calculate distances and also placed each set of coordinates into a neighborhood. For example, in our main set of analyses, we locate each RSO residence and each crime in a Census block group (CBG), a geographic unit defined by the U.S. Census Bureau.[19] There were 498 CBGs in Baltimore

[17] We assembled these data for the years 2000 through 2009 by using the Public Elementary/Secondary School Universe Survey, created by the National Center for Education Statistics (NCES), which is available online at: <http://nces.ed.gov/ccd/pubschuniv.asp>.

[18] We also geocode each of the public schools in Baltimore County to build our school demographics control variables. We have annual data, which we attribute to each month of the academic school year, which runs from August 1 through July 31 of each year.

[19] We also consider other neighborhood definitions—including voting districts, zip codes, and the area within one mile from random addresses—and proceed in the same way. We explain these alternative definitions in Sections IV and VI.

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County in 2000, each one an aggregation of Census blocks and each with a population of between 600 and 3,000 individuals. CBGs are the smallest geographic unit for which Decennial Census information is available from the Census Bureau and constitute our primary definition of a neighborhood. Figure 1 shows the borders of all CBGs in Baltimore County (more dense areas result in smaller CBGs) in 2000. Figure 2 illustrates the approximate geographic distribution of RSO residences during our sample period.

Matching RSO residences and crimes to neighborhoods allows us to create a neighborhood-level panel data set spanning 95 months (February 2001 through December 2008). The data include the number of RSOs who live in the neighborhood during that month, annual school enrollment and free lunch information, and the number of crimes that occur in that month, broken down into (1) all sex offenses, (2) (other) violent crimes, and (3) nonviolent crimes, with all sex offenses further broken down into the following (overlapping) groups of crimes: (a) rape, (b) all nonrape sex offenses, (c) sex offenses against adults, (d) sex offenses against children, and (e) peeping, pornography, and prostitution (PPP) crimes. The matching process was complicated by the fact that RSOs do not necessarily move at month intervals, and so we assigned an RSO to a neighborhood only for the fraction of the month he lived there.[20] Accordingly, our monthly neighborhood RSO counts often include fractions of offenders.

Figures 3 and 4 show the distribution of RSOs (rounded to the nearest offender) across CBG-month cells and across years. In Table 1, we report basic summary statistics of our data by CBG-month. We include the overall mean for all CBG-months, the mean for those CBG-months in which the RSO count was above zero, and the mean for those CBG-months in which the RSO count was zero.[21] In our sample, there is roughly one RSO per Census block group on average, and one sex offense occurs in every CBG approximately every 10 months. Not surprisingly, both violent and nonviolent crimes are more common than sex offenses.

One striking association in Table 1 is that CBGs with more RSOs experience much more sex crime than other neighborhoods. This is a misleading comparison, however, because RSO neighborhoods also suffer disproportionally from other forms of violent and nonviolent crime—almost twice as much violent crime and over 50 percent more nonviolent crime. Neighborhoods with RSOs are also more likely to have higher percentages of enrolled students who are eligible

[20] To clarify with an example: an offender who lives in neighborhood A from June 1, 2002, to June 10, 2002, and then in neighborhood B from June 10, 2002, until the end of the sample would be counted as 10/30 (1/3) in neighborhood A and 20/30 (2/3) in neighborhood B for June 2002.

[21] If we weight the data by population using numbers from the 2000 Decennial Census, the summary statistics of our sample change little. This is not surprising, as CBGs are designed to cover geographic areas that contain roughly the same number of people.

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for free or reduced-price lunches and more pre-K to fifth-grade students enrolled relative to the total population. These differences between CBGs where RSOs live and where they do *not* live illustrate why simple demographic comparisons are insufficient to identify the true relationship between neighborhood victimization risk and the number of resident RSOs and, further, they underline the importance of including neighborhood fixed effects in any analysis.

IV. Empirical Strategy

Using the data described in Section III, we can better understand (1) whether neighborhoods with relatively more RSOs in residence experience more reported crime (i.e., whether victimization risk—from whatever source—is higher near the residences of RSOs than it is elsewhere) and (2) whether there is any evidence that this residency-risk relationship changes in response to the implementation of a searchable Internet notification regime. Any evidence on these questions will lead to many additional questions about underlying mechanisms as well as deeper questions about basic drivers of criminal sexual behavior.[22] Nevertheless, both research questions have great relevance for how best to manage convicted sex offenders after they are released and, at present, surprisingly little evidence exists that addresses either question.

To identify and measure the relationship between where sex offenders live and where sex crime occurs, we implement a conditional fixed effects Poisson regression estimated via quasi-maximum likelihood.[23] For our purposes, the outcome of interest is the number of criminal incidents in a neighborhood in a given month, and the number of RSOs in that neighborhood during the same time period serves as the key independent variable. Specifically, we estimate:

$$E(Y_{ilt}|X_{lt}) = \exp(\beta_0 + \beta_1 RSO_{lt} + \beta_2 RSO_{lt}^2 + \beta_3 X_{lt} + \delta_l + \alpha_y + \alpha_m). \qquad (1)$$

[22] For example, if Internet notification *were* to lead to relatively less risk in neighborhoods with RSOs, the reduction could be the result of more effective victim avoidance behavior, but such evidence would also be consistent with other hypotheses, including the possibility that NonRSOs choose to avoid areas where publicly known RSOs reside.

[23] There is debate in the literature about the appropriate regression technique for count data, but the conditional fixed effects Poisson model with robust (sandwich) standard errors is robust to overdispersion, excessive zeros, and serial correlation (see, e.g., Bertanha & Moser (2014) and the citations therein). Implementing fixed effects in a negative binomial analysis, the obvious alternative approach, is not trivial—see Greene (2005), and Allison and Waterman (2002). Nevertheless, we also ran negative binomial versions of these regressions, adding in dummy equivalents of the fixed effects in an unconditional model. These results are qualitatively and quantitatively similar, and are available upon request.

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Y_{ilt} represents the number of incidents of crime type i (e.g., sex crime, violent crime, nonviolent crime, etc.) occurring in neighborhood l in month t. RSO_{lt} is the number of registered sex offenders living in neighborhood l in month t.

One concern with our framework is that RSOs, for financial or other reasons, may live disproportionately in neighborhoods with unusual levels of offending behavior; such selection could generate a spurious correlation between RSO residency and crime. Accordingly, we apply a neighborhood fixed effects analysis that accounts for time-invariant characteristics of neighborhoods that may influence crime rates. In addition, year and month fixed effects (α_y, α_m) are included to account for any period- or season-specific county-wide changes (like new state laws). Another concern is that RSOs may also move into neighborhoods that are in the process of declining—and this would not be captured by our fixed effects analysis.[24] To address the possibility of RSOs moving to neighborhoods whose characteristics were changing in a way that may affect offense patterns, we include two time-varying controls (X_{lt})—the fraction of students in neighborhood schools eligible for free and reduced-price lunches and the ratio of pre-K to fifth-grade students enrolled in neighborhood schools to the neighborhood's total population (in 2000).

As we describe in Section II.B, the effect of an additional RSO on RSO, NonRSO, and victim behavior (and therefore levels of reported criminal activity) may be nonlinear—that is, the consequences may depend on how many RSOs already reside in the neighborhood. A neighborhood moving from zero to one RSO might evolve in a very different fashion than one that moves from one to two or two to three. Once an individual learns that a single sex offender lives in her neighborhood, for example, it may not matter how many additional RSOs live in the neighborhood if the potential victim is engaging fully in precautionary avoidance behavior. We address the possibility of this nonlinearity in two ways. In our main specifications, we include the squared number of RSOs (RSO_{lt}^2) in a neighborhood as a regressor. Because the use of a squared term assumes that any nonlinearity takes a particular form, we also proceed nonparametrically in Section VI.A by separately considering the effects of having at most one, one to three, or more than three RSOs.[25]

[24] Consider one possibility: RSOs decide to move into a neighborhood and all families with children move away or, alternatively, because a neighborhood is in decline, it becomes a more affordable place for RSOs to live. Although sex offenders do tend to live in neighborhoods that are more socially disorganized and have higher poverty levels, neighborhood decline appears to happen more slowly (especially when we control for fixed changes across all neighborhoods, as might happen in a recession).

[25] As we note in Section III.D, RSOs may move into and out of neighborhoods during a month. As a result, it is possible for a neighborhood to have "less than one" but more than zero RSOs in a given month.

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To ease interpretation, we report all Poisson results in our tables as exponentiated coefficients, also known as incidence rate ratios (IRR).[26] An IRR has a multiplicative interpretation: a one unit increase in the independent variable (e.g., the number of RSOs) is associated with $\exp(\beta)$ *times* as much of the dependent variable (e.g., the number of offenses).[27] Our tables also present cluster-robust sandwich standard errors, which are not sensitive to overdispersion, excessive zeros, or serial correlation (see, e.g., Cameron & Trivedi 2009). Spatial correlation in the error term across neighborhoods can also pose a threat to correctly estimating standard errors, however. Unobservable shocks that affect crime rates (such as gang activity, police concentration, economic vitality, and local geographic attributes) are likely to be correlated across space in ways not fully captured by neighborhood boundaries. Fortunately, spatial correlation does not appear to be a problem in our work, and explicitly accounting for it leaves our findings unchanged.[28]

Because the implementation of notification may alter the behavior of NonRSOs, RSOs, and potential victims, it may also transform the geographic relationship between where RSOs live and where sex crime occurs. If notification laws are *relevant* to the geographic distribution of sex offense victimization risk—that is, if revealing identifying information about convicted sex offenders changes the criminal activity landscape in a way that either harms or benefits local victims—our empirical strategy has the potential to provide evidence in support of one of these possibilities. Assuming the arrival of the new information environment was exogenous to the decisions of sex offenders to commit crime (a case we make in Section II.A), a causal story can be told about the *change* in any relationship we find using Equation (1).[29]

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[26] The raw Poisson coefficients of interest (β_1 and β_2) are complicated to interpret—in no small part because of interaction terms, including our squared independent variable (RSO_{it}^2).

[27] To see this mathematically, consider the basic Poisson regression $E(Y|X) = \exp(\beta_0 + \beta_1 X)$. If we add one additional unit of X then $E(Y|X = X + 1) = \exp(\beta_0 + \beta_1(X + 1)) = \exp(\beta_0 + \beta_1 X + \beta_1) = \exp(\beta_0 + \beta_1 X)\exp(\beta_1) = E(Y|X)\exp(\beta_1)$. Thus, an IRR of 1 is analogous to finding no association ($\beta = 0$ in an OLS setting), an IRR greater than 1 implies a positive association, and an IRR less than 1 implies a negative association. To be more precise, an IRR of 1.50 indicates that a one unit increase in the value of the independent variable will result in 50 percent more incidents. In the context of this article, an IRR of 0.90 for β_1 indicates that a neighborhood with one additional RSO would have only 90 percent of the crime incidents in question when compared to a neighborhood with one fewer RSO. Our Appendix includes a more thorough explanation of the interpretation of Poisson IRRs, including how to interpret the squared term (and the interaction terms we introduce below).

[28] Bertanha and Moser (2014) show that, so long as any spatial correlation is time invariant, the conditional fixed effects Poisson model will give consistent estimates with robust (sandwich) standard errors. Using their suggested test statistic, we examine whether any spatial correlation in our error terms may vary over time. We are unable to reject the null of time invariance. Bertanha and Moser (2014) also provide code for estimating standard errors in the presence of time-variant spatial correlation. For our estimates of the test statistic as well as standard errors that take into account any time-varying spatial correlation, see our Appendix.

[29] We see no trends (in our relatively short preperiod) that indicate atypical residential movement on the part of RSOs prior to the publication of these data on the Internet, nor do we see a spike in offenses around that time.

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To identify the relationship between notification and victimization risk in RSO neighborhoods, we introduce an indicator for whether the Internet sex offender registry (which came online in April 2002) was available and then interact this variable with our RSO measures. The interaction terms capture how the relationship between the number of RSOs and crime in a neighborhood changes in response to the public being made aware of the whereabouts of RSOs. We estimate:

$$
\begin{aligned}
E(Y_{ilt}|X_{lt}) = \exp\big(&\beta_0 + \beta_1 RSO_{lt} + \beta_2 RSO_{lt}^2 + \beta_3 Internet_t \\
&+ \beta_4 RSO_{lt} * Internet_t + \beta_5 RSO_{lt}^2 * Internet_t + \beta_6 X_{lt} \\
&+ \delta_l + \alpha_y + \alpha_m\big),
\end{aligned} \tag{2}
$$

where, for example, the coefficient β_4 will capture how the relationship between the number of sex offenders and the frequency of crime incidents in a neighborhood changes after registry contents are disclosed via the Internet.

Interpreting interaction coefficients in a nonlinear modeling environment like Poisson is nontrivial. Ai and Norton (2003) highlight the fact that the magnitude of any interaction term does not easily map to its marginal effect and that the coefficient and the marginal effect can even be of opposite sign. With an IRR transformation of the coefficients, however, the interpretation of an interaction term is relatively straightforward (Buis 2010; Doidge et al. 2013). The IRR on an interaction term is also interpreted multiplicatively, but in a way that requires some explanation. Specifically, the coefficient on the interaction term is interpreted as an $\exp(\beta)$ times larger effect as compared to the main effect. To make this more concrete, an IRR ($\exp(\beta_1)$) of 0.90 on β_1 and an IRR ($\exp(\beta_4)$) of 1.05 on β_4 in the model above would indicate that neighborhoods with one additional RSO *after* Internet notification would experience 1.05 times as much crime as those neighborhoods with one additional sex offender *before* Internet notification. In this example, the total effect of one additional RSO post-notification is thus $\exp(\beta_1) \times \exp(\beta_4) = 0.945$.[30]

One important methodological question concerns how we define a neighborhood or, more generally, what we consider to be close proximity to an RSO for purposes of our analysis. In our conceptual framework, the features of a particular location play an important role in understanding criminal behavior and identifying where potential victims may be at the greatest risk. Capturing the location idea best in the sex offender policy context, neighborhoods represent

[30] In other words, an IRR above 1 on the interaction does not indicate that an additional RSO results in higher than average risk post-notification; rather, in the case of an IRR of 0.90 on β_1, the risk associated with one additional RSO would remain below average so long as the IRR on β_1 is less than approximately 1.11 (or $(1/\exp(\beta_4))$). See our Appendix for more details.

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the at-risk or danger zone in which potential offenders and potential victims both live. Sex offender notification regimes seek to operate at the true neighborhood level by distributing identifying information about RSOs to those who live within close proximity and with whom they may interact.

Although any definition will be somewhat arbitrary, to safeguard against the possibility that any particular approach to defining a neighborhood may explain our findings, we experiment with a range of alternatives. In our main analysis, we employ two neighborhood definitions: (1) Baltimore County's Census block groups (of which there were 498 in 2000) and (2) circles with one-mile radii drawn around 500 random addresses.[31]

First, we characterize a crime as proximate to an RSO by whether it occurs within the same Census block group (CBG) as the offender lives. Using a Census definition of a neighborhood translates to our asking whether victimization risk is higher or lower in CBGs where more sex offenders reside than we might otherwise expect. A CBG, however, is at least partly an arbitrary definition of a neighborhood. Although it is a contiguous geographic unit defined by the U.S. Census Bureau, it is designed primarily to contain a certain number of residents, and it seems unlikely that individuals living in Baltimore County would consider their CBG a fully accurate representation of their neighborhoods (or, alternatively, their at-risk zones). In particular, a CBG definition may not count a sex offense just a block away from an RSO as a proximate crime (e.g., if the RSO lives near a border) but may count one that is four blocks away in another direction.

Second, to ensure that CBG arbitrariness does not account for our findings, we adopt an alternative approach to defining a potential victim's neighborhood: for 500 random residential addresses in Baltimore County (the approximate number of CBGs in our analysis), we simply draw a circle (keeping the address at the center) and define the area of that circle as the neighborhood of that address. We then count the number of registered offenders and crime incidents within that radius for each month and repeat the CBG-based analysis by estimating Equations (1) and (2) using these circles. Of necessity, this technique assumes a fixed neighborhood shape (circle) and draws lines by employing a distance (radius) at which near sex offenders (supposedly posing a realistic threat) are distinguished from those offenders who are considered far away (and therefore thought to pose little threat). Circle neighborhoods may thus be arbitrary, but they are also convenient and maximally compact. More importantly, circle

[31] Figure 1 shows Baltimore County's CBG neighborhoods. Figure 5 shows Baltimore County's one-mile circular neighborhoods. We consider other neighborhood definitions below.

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neighborhoods differ significantly from CBG-defined neighborhoods, allowing us to test the robustness of our CBG approach.

Justifying any particular radius for a circular neighborhood as appropriate, however, is more challenging. Fortunately, with respect to both shape (circle) and distance (radius), we are able to defer to the judgment of Maryland's Department of Public Safety and Correctional Services, which manages Maryland's online sex offender registry. Maryland's registry software identifies and maps registered offenders who live within a certain radius of the address entered, but limits the distance options it makes available to users, allowing them to search for offenders who live within one-quarter mile, one-half mile, one mile, three miles, or five miles of a given address.[32] Because Maryland takes these radii as usefully defining neighborhoods—at least for purposes of reducing sex crime through its sex offender notification policy—we employ these same distances. We report results for one-mile radius neighborhoods, but results produced using the other radii Maryland's website suggests are available from the authors upon request.

V. Results

Table 2, which explores the association between the number of registered sex offenders and the number of crimes within Census block groups, presents results from estimating Equation (1) for different crime types.[33] Overall, our analysis indicates that the existence of more registered sex offenders living in a particular Census block group is associated with *fewer* reported sex offenses. According to Column (1), each additional sex offender is associated with 7.5 percent fewer sex offenses (0.925 times as much crime). To explore this relationship further, in Columns (2) through (6), we subdivide all sex offenses into different (sometimes overlapping) subcategories: (2) forcible rape, (3) all nonrape sex offenses, (4) sex offenses against adults (excluding rape), (5) sex offenses against children, and (6) peeping, pornography, and prostitution (PPP) offenses. Although not all results are statistically significant, they show a clear pattern: an additional RSO resident is associated with fewer reported sex offenses of all types.[34]

[32] To illustrate, Figure 6 shows the results (from a January 2014 Internet registry query) of searching for RSOs within a one-half-mile radius of an arbitrary address in Maryland. The inner circle represents one-quarter mile and the outer circle one-half mile. The flag is the entered address (the "home address" of the individual who is searching); the other pins represent the locations of RSOs.

[33] Because we employ CBG fixed effects in our Poisson regressions, any CBG that does not experience a single incident of the type of crime counted on the left-hand side in any month is effectively dropped, causing the number of observations included in each column in our tables to differ (because each column represents a different dependent count variable). We also ran our regressions on a balanced panel that included the same number of observations for all crime types (i.e., on only those CBGs that had at least one incident of every crime type we examine in Table 2) and found few differences in our results. These results are available upon request.

[34] Note that our regressions include Census block group fixed effects, and so our results are calculated using variation in the number of RSOs *within* a block group over time. The fact that RSOs might congregate in particular

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We note that the estimated IRRs on the squared terms are very close to 1, providing no evidence in favor of a nonlinear relationship between the number of RSOs and sex crime in a neighborhood.[35]

Table 3 presents the results of estimating Equation (2), which explores how the relationship between the number of RSOs and victimization risk differs before and after the implementation of Internet notification in Maryland. The IRR estimate on RSO_{lt} represents the association between an additional RSO and crime before notification (when only a registry was in place) and the exponentiated interaction coefficient on $RSO_{lt} * Internet_t$ indicates how this relationship changes after notification.

Although most of our coefficients are imprecisely estimated, we find that before the implementation of Internet notification, one additional neighborhood RSO was associated with fewer overall reported sex offenses, driven by fewer nonrape sex offenses, especially sex offenses against adults and PPP crimes. These negative associations are slightly larger than those reported in Table 2: each additional neighborhood RSO is associated with approximately 10.3 percent fewer total sex offenses.[36] Importantly, forcible rape and, possibly, sex offenses against children do not follow this pattern. Under registration, our (admittedly imprecise) point estimates suggest that an additional RSO in a neighborhood increases the frequency of forcible rape by 11.5 percent.

How do these relationships change under an Internet notification regime? With respect to all sex offenses, an additional RSO post-notification is associated with an IRR that is 1.026 times the pre-Internet IRR.[37] In other words, an additional neighborhood RSO post-notification is still associated with *fewer* reported sex offenses, but the difference is now smaller than prior to notification (0.897 vs. $1.026 \times 0.897 = 0.920$), implying the law may have resulted in a slight *increase* in the overall relative risk of being victimized in RSO neighborhoods. Nevertheless, our point estimates still indicate that RSO-heavy neighborhoods are safer on the whole post-

types of neighborhoods cannot account for our findings, unless these neighborhoods changed in important ways during our sample period and these changes correlated with the number of RSOs, a possibility we address by including our school enrollment and free and reduced-price lunch controls.

[35] If we were to take the point estimate of the coefficient on RSO_{lt}^2 as true, then Column (1) would imply that a neighborhood with one additional RSO resident will have fewer crimes, *but also* that as the number of resident RSOs rises, this difference will weaken just slightly on average (compare $\exp(\beta_1)$ of 0.925 to $\exp(\beta_1) \times \exp(\beta_2)$ of 0.928).

[36] In Table 3, we also now detect some evidence of a nonlinear relationship between the number of RSOs and reported sex crime (especially with respect to nonrape sex offenses) in the pre-notification period. Excepting forcible rape, our estimates of β_2 suggest that crime does not decline as quickly with the addition of an RSO if RSOs are more common.

[37] These relationships (across crimes) are estimated imprecisely, despite being large in magnitude.

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notification relative to other neighborhoods. Importantly, forcible rape and sex offenses against children show a clearly different pattern. Just as victimization risk for these crimes under registration seemed to increase with the addition of an RSO to a neighborhood, notification appears to reduce victimization risk.

Our findings may be consistent with alternative explanations if, during our sample period, RSOs tended to move into neighborhoods where crime was generally changing relative to other neighborhoods.[38] We evaluate this possibility by re-running our Equations (1) and (2) for other violent and nonviolent crimes. If RSOs happen to congregate in places where crime is declining, for example, we would also expect a correlation between the number of RSOs in a neighborhood and *other* measures of crime. Our results offer no support for this possibility. We find that the number of RSOs in a CBG has very little association with the number of other (nonsexual) violent crimes or nonviolent crimes (see Tables 2 and 3, Columns (7) and (8)). We also estimated regressions identical to those in Columns (1) through (6), but with measures of violent and nonviolent crime included as controls to account for any confounding crime or criminal justice trends. Our results were unchanged, although, of course, we cannot rule out the possibility of some other omitted variable.

In Table 4, we present our results from estimating Equation (2) using circular neighborhoods with radii of one mile centered on 500 random addresses in Baltimore County.[39] In substance, the patterns in Table 4 line up closely with the numbers reported in Table 3 for all offenses except PPP crimes. There are relatively fewer total sex offense reports within one mile of a random residential home that has relatively more RSOs within a mile and, as before, this pattern becomes weaker after public notification commences. Forcible rape and sex offenses against children continue to point in a different direction, however: victimization risk in RSO neighborhoods is slightly higher (although not statistically significantly so), with notification appearing to reduce the risk of these crimes. Indeed, in both Tables 3 and 4, our estimates suggest that notification resulted in a net reduction of risk for rape and sex offenses against children, such that an additional RSO in a neighborhood post-notification is associated with a below-average level of victimization risk, despite public assumptions to the contrary.[40]

[38] Our neighborhood fixed effects control for the fact that RSOs may *generally* live in atypical neighborhoods and move between them. Only RSOs disproportionally living in and moving to neighborhoods that are becoming safer over time has the potential to spuriously produce the results we find.

[39] Our results for the analyses of other radii do not differ in substance and are available upon request.

[40] In fact, this result helps explain the consistency across sex crime types of the negative relationship between an additional neighborhood RSO and victimization risk in Table 2. The negative relationship is driven by the implementation of notification for forcible rape and sex offenses against children, whereas the negative relationship with respect to sex offenses against adults (not including rape) and PPP crimes remains *in spite* of the effects of

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Figure 5 maps these circular neighborhoods using a one-mile radius definition. Unlike with our CBG analysis, these neighborhoods overlap with each other. As a result, our regression error terms may be spatially correlated across neighborhoods—that is, two neighborhoods that are physically overlapping will clearly be subject to some of the same random shocks. In our Appendix, we carry out the method described in Bertanha and Moser (2014) (see footnote 28) for testing for and correcting spatially-biased standard errors. As expected, this analysis reveals significant time-variant spatial correlation, but when we calculate consistent spatial variance estimators, the results are roughly similar. Therefore, Table 4 records our estimates with regular robust (sandwich) standard errors; results with spatially corrected standard errors can be found in the Appendix.[41]

In summary, we find that sex offense victimization risks are *generally* lower (across all offenses) in neighborhoods that have more RSO residents,[42] and that reported crime in RSO neighborhoods appears to increase slightly after the implementation of notification. There is considerable heterogeneity across types of sex offenses, however. With respect to forcible rape and sex offenses against children (which are a minority of sex crimes in numbers, but may be more serious in their aggregate consequences), we discern the opposite pattern. The fact that registration and notification have different effects on different types of sex offenses is not surprising; sex offenses are diverse. Some require planning or involve money; others are impulse crimes or involve intimidation or violence. There is no reason to assume, ex ante, that offender, police, and victim behavior will respond to how the law addresses these crimes in a way that affects their frequency and geographic distribution in the same way.

Our framework's predictions in Section II.B were ambiguous, but they are clearly consistent with the evidence we see with respect to nonrape adult sex offenses and peeping, pornography, and prostitution crimes. Registration can lead to low levels of crime in areas with more RSOs when RSOs are not evenly geographically distributed, and notification can indeed result in an increase in reported crime in RSO neighborhoods either because NonRSOs become more likely to offend in RSO neighborhoods (perhaps because they are less likely to be detected in the

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notification because of the strong negative association of nearby RSOs and these crimes under registration.

[41] In addition, these regressions do not include the school enrollment numbers because it is not possible to calculate the total populations in our circular neighborhoods. Our regressions do include the percent of students who are eligible for free and reduced-price lunch as an independent variable, and we also include as controls the number of violent and nonviolent crimes in each neighborhood in each month as well.

[42] These results *would* suggest that sex offenders use "buffer zones" in their crime location decisions *if* RSOs were the primary source of reported sex offenses. Unfortunately, as we have noted, they are not—a study using data from New York State showed that over 90 percent of reported sex offense cases were committed by *first-time* offenders, and so any interpretation is more complicated (Sandler et al. 2008).

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presence of RSOs) or because RSOs become more likely to recidivate near their homes as a result of the economic and social costs of being publicly known as a sex offender.[43] Although our data do not allow us to untangle these two possibilities, the size of the effects we discern are nontrivial and should be of concern to policymakers and the public.

Indeed, consistent with the evidence we present here, other scholarship has raised the possibility that notification may be counterproductive, at least with respect to controlling RSO recidivism (Prescott & Rockoff 2011). At the same time, while the possibility exists that NonRSOs may be deterred from committing sex crimes by sex offender laws that will be applied to them down the road should they be caught and convicted, no work of which we are aware has raised the possibility that other dimensions of NonRSO offending behavior—for example, the decision *where* to commit a crime—may be influenced by sex offender laws like notification. At the very least, even if notification has moderated the association somewhat, our findings support the idea that the forces that encourage sex offenders as a group to commit crimes away from where RSOs live outweigh any net benefits—on average—of committing those crimes nearby, where people and policymakers assume the greatest risk exists.

In our CBG-neighborhood analysis, notification's largest effect on victimization risk in terms of magnitude is by far with respect to peeping, pornography, and prostitution (PPP) offenses. Reports of such crimes appear to have increased dramatically post-notification—although neighborhoods with more RSOs nevertheless still have fewer PPP crimes on average post-notification, and the number of PPP offenses is also relatively low, meaning that changes in the numbers of these crimes is less dramatic than it may seem from the IRRs we calculate. Recall that we use reported measures of crime, and so any new report could represent a new incident or an existing incident that is newly reported. In our view, although the PPP effects are run-of-the-mill in the circular neighborhood analysis (raising the possibility that the pattern is spurious), either interpretation of the increase is both plausible and interesting.

To begin with, notification may have increased the propensity of RSOs to engage in vice or peeping tom offenses, especially if the notoriety that followed being publicly identified in their neighborhoods led to more RSO unemployment. Likewise, if such crime became more common in RSO neighborhoods, NonRSOs may have found visiting these neighborhoods more attractive post-notification. Alternatively, notification may have generated more frivolous reports of criminal activity by frightened residents who were newly informed of the identities of their RSO

[43] See Prescott (2011) for an argument (in the child pornography context) that the identifying information made available by Internet registry sites might allow potential sex offenders (both NonRSOs and RSOs) to conspire more easily with RSOs, making them more dangerous.

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neighbors. Reports of peeping tom offenses, in particular, appear to fit this story.[44] Finally, if police felt pressure to patrol RSO neighborhoods more intensely or if the public increased its monitoring effort, previously undetected PPP crimes seem more likely to be newly observed than other categories of undetected sex crimes, which are often more hidden. This seems particularly to be the case with prostitution, and if the police were regularly engaging with RSOs in their neighborhoods post-notification, the discovery of pornography offenses seems possible.

Importantly, our framework can also help us understand the quite different patterns we observe with respect to forcible rape and child sex offenses. While on average victimization risk of these crimes appears to be lower over our entire sample period in RSO-dense neighborhoods, the point estimates of the IRR on RSO_{lt} in Equation (2) (Tables 3 and 4) are positive (although statistically insignificant), pointing to an increase in average victimization risk with an additional RSO resident *before* the implementation of notification. By contrast, *after* notification, an additional RSO appears to *reduce* forcible rape and child sex offense victimization risk. Whether we use CBG or circular neighborhoods, our findings suggest that notification reduces the risk of these crimes to below-average levels. In Table 3, an additional RSO reduces victimization risk for rape by almost 8 percent and for sex offenses against children by more than 6 percent. In Table 4, the reduction is smaller in magnitude, but still important in practical terms.

With one caveat (and keeping in mind that our estimates are not precisely estimated), the patterns in the data for forcible rape and child sex offenses appear to be consistent with public and policymaker perceptions. Absent notification, children and potential rape victims may have been at greater risk in neighborhoods with additional RSOs, and our results are consistent with notification mitigating those risks.

These conjectures accord with research showing that notified parents take more protective actions on behalf of their children than do nonnotified parents (Bandy 2011; Beck et al. 2004), despite the fact that the literature has shown no overall effect of knowledge of RSOs on risk-mitigating behavior (Anderson & Sample 2008; Bandy 2011; Caputo & Brodsky 2004; Phillips 1998). If true, victim precaution taking (i.e., adults protecting their children) near RSO homes may be *particularly* intense. In our conceptual framework, such victim activity might well drive NonRSOs and RSOs alike to search for victims elsewhere. On the other hand, most sex crimes committed against children are perpetrated by people they know, meaning that displacement to other neighborhoods may be less likely to occur in this context. Other possibilities, then, are that (1) the existence of nearby known RSOs serves as a deterrent to those NonRSOs who live near

[44] At odds with this story is the fact that in our data there are very few peeping tom reports; a majority of the PPP category is made up of prostitution and pornography crimes.

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RSOs (as local NonRSOs may pose the greatest threat) and (2) more intense policing in RSO neighborhoods may make offending even against children who an RSO knows more difficult. These possibilities imply deterrence rather than displacement.

It is worth noting that our findings with respect to sex offenses against children seem *inconsistent* with a reporting effect story, a conclusion that may inform our other results, although the discussion above makes plain that drawing inferences about the dynamics of one sex offense from the dynamics of another can only be done with caution. *Before* notification, when RSOs were presumably living anonymously, a relatively high number of sex crimes against children were reported in neighborhoods with more RSOs. Once these RSOs became publicly known, the likelihood of a reported incident decreased with additional RSOs. Such a story seems at odds with the idea that notification may have resulted in more frivolous reports in RSO neighborhoods or that crime that *would* have gone undetected prior to notification in RSO neighborhoods was instead detected via more intense public monitoring.

Similarly, the forcible rape numbers dovetail with the buffer zone literature's claim that serial rapists are particularly concerned about being recognized during the commission of a crime against a stranger (Duwe et al. 2008), even when the potential adult rape victims in an RSO's neighborhood take no effective precautions. Additionally, to the extent that adult rapes are more likely to occur in places more easily monitored, and because notification may have led to increased monitoring by the public and the police, RSOs and NonRSOs alike may have gravitated toward committing rape away from where RSOs live post-notification.

The caveat to the idea that these results align generally with the public's assumptions about the desirability and likely consequences of notification, of course, is that our data also suggest that, under notification, a neighborhood with an additional RSO may actually be *safer* for children and potential rape victims than otherwise comparable neighborhoods with fewer RSOs. As we explained at the beginning of Section II, the idea that you might actually be *safer* living near an RSO of whom you have been made aware is at stark odds with conventional wisdom (and housing price dynamics).

On the whole, our findings supply evidence in support of two dynamics that push in opposite directions. Overall, and with respect to sex offenses against adults and PPP crimes, the forces that encourage sex offenders to commit crimes away from where RSOs live outweigh any net benefits—on average—of committing those crimes where people and policymakers assume the greatest risk exists. Yet revealing where RSOs live appears to have made relatively safe RSO neighborhoods somewhat less safe with respect to these crimes, perhaps because (1) NonRSOs may prefer, post-notification, to attack where RSOs live, assuming victim precaution is focused

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elsewhere, or (2) RSOs may have become more likely to reoffend in general, and as a result of the normal dynamics that underlie distance decay (perhaps strengthened by notification resulting in more unemployment and social isolation), they tend to do so more closely to their homes. By contrast, for forcible rape and crimes involving children, under registration, distance decay seems paramount, but under notification, the geographic patterns in the data suggest that deterrence or displacement of crime to another neighborhood better characterize reality.

Our goal is to describe the relationship between where RSOs live and where sex offenses are most likely to occur because the public and policymakers make strong assumptions about this relationship, assuming that proximity is tantamount to risk. We find a much more complicated story, but even if we are unable to draw broad behavioral conclusions on the basis of our evidence, the reduced-form effects are clear: even under notification, victimization risk appears to be lower (or at least not higher) for all sex offenses in those neighborhoods where RSOs live.

VI. Extensions and Robustness Checks

In this section of the article, we further probe our empirical work, addressing particular concerns and ensuring the overall robustness of the patterns we observe in the data. In order, we consider in more depth (1) the possibility that victimization risk has a nonlinear relationship with the number of RSOs living in a neighborhood, (2) the possibility that our results may not account for potential spillovers between neighborhoods that are adjacent to each other, (3) the effects of employing two other (less attractive, in our view) definitions of neighborhoods, and (4) whether an alternative approach to evaluating the effects of notification on the relative frequency of sex crime—comparing the victimization risk levels near random addresses to the risk levels near RSO addresses—may provide further insight into our findings.

A. Non-Linear Effects

In general, our inclusion of the squared number of RSOs (RSO_{it}^2) in Equations (1) and (2) produced somewhat inconsistent evidence on whether a nonlinear relationship exists between the number of nearby RSOs and relative victimization risk. Table 3's CBG neighborhood results suggest, in particular for peeping, pornography, and prostitution offenses, that the difference in risk associated with an additional RSO (whether pre- or post-notification) tends to weaken the main effect—that is, victimization risk is a monotonically decreasing (or increasing) function of the number of RSOs, but the incremental effect is smaller as the number of neighborhood RSOs increases. By contrast, our circular neighborhood results (Table 4) provide no confirmatory evidence of these nonlinear relationships.

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To explore this issue further and check for additional or different types of nonlinearity, we use simple nonparametric methods to test for threshold effects. Specifically, we reestimate Equation (2) using our CBG neighborhoods, but we replace RSO_{lt} and RSO_{lt}^2 with three indicator variables to capture separately the effects of moving from zero to at most one RSO in a month, moving from zero to at most three RSOs, and moving from zero to any number more than three RSOs.[45] The results of this exercise, reported in Table 5, provide much useful information about the relationship between RSO concentration and levels of victimization risk.

First, our estimates imply that the risk-reducing effect of additional RSOs (pre-notification) disappears (per RSO) as the number of RSOs in a neighborhood increases. In other words, the reduction in risk in a neighborhood from moving from zero to between one and three RSOs is close to zero and may even be slightly positive.[46] The results for rape and child sex offenses differ, however. With respect to forcible rape, additional RSOs (pre-notification) result in *fewer* rapes, suggesting an important nonlinearity as well as a commonality with adult sex offenses and PPP crimes—at more than three RSOs, a neighborhood's rape victimization risk under registration is below average. One explanation for this threshold effect is that more than one RSO results in disproportionately more police monitoring. Incongruously, with respect to sex offenses against children, additional RSOs are associated with larger incremental *increases* in risk.

Second, and unexpectedly, the effect of notification laws on victimization risk is nonmonotonic: notification appears to *increase* relative victimization risk for many categories of sex offenses when there is just one RSO in a neighborhood, but *decrease* the likelihood of a reported sex crime when there are many RSOs in a neighborhood. Specifically, with fewer than three RSOs in a neighborhood (a large majority of the neighborhoods in our data), the interaction IRRs are above 1 (not including forcible rape or child sex offenses), meaning that, as in Table 3, additional RSOs are associated with fewer—but not as many fewer—sex offenses.[47] Once a neighborhood has more than three RSOs, however, our point estimates indicate that notification reduces crime. Specifically, prior to notification, an average of 6.3 percent fewer sex crimes were

[45] Recall that, as we explain at the end of Section III, because an RSO may live in a neighborhood for only a fraction of a month, the RSO count in a particular neighborhood in a given month need not be a whole number.

[46] Our point estimates further suggest that victimization risk is lower in neighborhoods with between one and three RSOs than it is in neighborhoods with more than three RSOs, although the difference may be due to random sampling variation.

[47] With respect to all sex offenses, up to one RSO pre-notification was associated with an 11.1 percent reduction in victimization risk relative to neighborhoods with zero RSOs. Post-notification, this average reduction declined to 9.2 percent. Similarly, neighborhoods with between one and three sex offenders have 9 percent fewer sex offenses pre-notification, but post-notification, that number dropped to roughly 8 percent. This signifies that moving from one RSO to two RSOs post-notification affects victimization risk very little.

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reported in neighborhoods with more than three RSOs (relative to neighborhoods with zero RSOs), but post-notification, this average difference increases to more than 25 percent. This dynamic is true across the categories of sex offenses we consider (even child sex offenses), strongly suggesting that RSO concentration matters for the geography of victimization.

B. Spillovers

Thus far we have focused on the association between criminals and crime in the *same* neighborhood. However, our conceptual framework—as well as more well-known ways of thinking about sex offender behavior (e.g., buffer zones)—implies that a sex offender may deliberately seek out victims who are far away from his home to avoid detection and, possibly, victim precautionary behavior. If RSOs and NonRSOs are not randomly distributed, our estimates may understate or overstate the true relationship between local RSOs and relative victimization risk. Accordingly, accounting for RSOs in adjacent neighborhoods may shed important light on the geography of victimization.

To explore this possibility, we add to Equation (2) the average number of RSOs in neighboring (contiguous) Census block groups, as well as its interaction with an indicator for whether Internet notification was in place. We chose the average rather than the total number of adjacent RSOs because a CBG that just happens to touch many neighboring CBGs may have an artificially inflated (i.e., unrepresentative) total as compared to a CBG that is surrounded by fewer neighboring CBGs.

Table 6 presents our results. In general, the main effect of additional adjacent RSOs is uniform (in sign and magnitude) across all categories of sex offenses—that is, conditional on the number of own-neighborhood RSOs, an additional adjacent RSO is associated with an overall 2.6 percent decrease in reported sex offenses in the period before Internet notification became available. With respect to the effects of notification on the relationship between RSO residency and victimization risk, the IRR on the relevant interaction terms being less than 1 evinces a similar weakening of the pre-Internet effect just as with own neighborhood RSOs. In fact, for all sex offenses, notification is associated with the attrition of the reduced risk associated with adjacent RSOs. Moreover, accounting for adjacent RSOs post-notification reduces the magnitude of the IRRs on $RSO_{lt} * Internet_t$, with the IRRs on all sex offenses dropping below 1 and lowering others that were above 1.

This pattern is suggestive of geographic displacement. The more RSOs there are in nearby neighborhoods post-notification, the higher the victimization risk, all else equal— even with respect to forcible rape and sex offenses against children. In the case of all sex offenses, the point

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estimates imply that notification reduces the threat of local RSOs, but increases by an almost equal amount the risk posed by adjacent neighborhood RSOs, revealing that some of the measured effect of notification in neighborhoods with RSOs in Tables 3, 4, and 5 may have been attributable to RSOs in adjacent neighborhoods. Although merely suggestive, these patterns may also help explain existing research that claims to find little overall association between registration or notification laws and decreases in overall sex offense rates: these laws may simply displace rather than reduce crime. Unfortunately, our data and methods do not allow us to explore the full geography of sex offense displacement in this article.

C. Additional Neighborhood Definitions

In studying the relationship between RSO residency and sex offense victimization risk, we primarily rely on two definitions of neighborhoods. First, we use a nonoverlapping Census definition, equating to 498 neighborhoods of roughly similar population size. Second, we use the distances Maryland considers relevant to victimization risk, creating circular neighborhoods using 500 random address points in Baltimore County and presenting results for neighborhoods with a one-mile radius. For robustness, we also analyze the relationship of neighborhood RSO residency to sex offense victimization risk using two other nonoverlapping definitions of neighborhoods: voting districts (of which there are 223 in Baltimore County) and zip codes (of which there are 55). The number of voting districts (see Figure 7) is less than half the number of CBGs, and zip code neighborhoods are quite large (see Figure 8), numbering only slightly more than 10 percent of the number of CBG neighborhoods. With respect to voting districts, the pattern of coefficients we estimate is remarkably consistent with our results in Tables 2, 3, and 4.[48] Our zip code analysis, on the other hand, shows little relationship between the number of RSOs and victimization risk. Given how large these neighborhoods are, however, this result is not particularly surprising.

D. Risk Comparison of Random versus RSO Neighborhoods

Another strategy to examine whether notification may alter the relationship between where RSOs live and where sex crime occurs is to *compare* notification's apparent effects on crime in neighborhoods around RSO addresses to analogous changes in neighborhoods around random residential addresses. We attempt this approach first by creating neighborhoods of various sizes around each RSO and then comparing how sex crime victimization risk in those neighborhoods changes around the time of notification to any similarly timed change in crime levels in

[48] Results are available from the authors upon request.

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neighborhoods of similar sizes built around random addresses. One advantage of this flexible approach is that it allows us to experiment with many different sizes of neighborhoods, the results of which turn out to offer at least some insight into what may be the most appropriate neighborhood size from a policy perspective when the goal is to minimize victimization risk in a particular location.

To carry out this exercise, we geocode the same 500 randomly chosen residential addresses as we did in our circular neighborhood analysis, and then measure how many incidents of each crime type occur (in each month) near these random homes. Separately, we count how many similar incidents occur near RSO addresses. We then compare these two measures. We can make this comparison at any distance. In what follows, we examine this relationship at distances starting at 0.1 miles away from the residence in question and at each 0.1 mile increment further away from the home, up to a total distance of 3 miles.[49]

To evaluate the potential consequences of Internet notification, we ask whether there is a *change* in the fraction of total crimes that are type i near an RSO's home and compare that to any *change* in the fraction of total crimes that are type i near an average random residential address. Specifically, for each crime type i we calculate:

$$\frac{Y_{ilta}}{\sum_{i=1}^{k} Y_{ilta}}.$$

In the context of sex offenses, this fraction would measure the percentage of all crimes attributable to sex offenses within an l mile radius of address a at time t. We then use these fractions as dependent variables in a simple OLS regression:

$$\frac{Y_{ilta}}{\sum_{i=1}^{k} Y_{ilta}} = \beta_0 + \beta_1 RSOAddress_{lta} + \beta_2 Internet_t + \beta_3\, RSOAddress_{lta} \times Internet_t + \varepsilon_{ilta}, \qquad (3)$$

where $RSOAddress_{lta}$ is an indicator for whether address a is an RSO's home (as opposed to a random residential address). Our coefficient of interest, β_3, captures any change after notification in the fraction of crimes that are type i for neighborhoods centered around RSO addresses relative to any such change in the same fraction for neighborhoods centered around random residential addresses. Figure 9 plots our estimates of β_3 from Equation (3) for radii from 0.1 to 3 miles from the house (by 0.1 mile increments) for the fraction of various sex offenses (all sex

[49] These areas do not overlap and each comparison covers a unique geographic area. That is, we make our comparisons at a distance from 0 to 0.1 miles, then from 0.1 to 0.2 miles, etc.

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offenses, forcible rape, nonrape sex offenses, sex offenses against adults, sex offenses against children, and peeping, pornography, and prostitution offenses) relative to all reported crimes (i.e., the number of the relevant category of sex offense divided by the number of all crimes).

The patterns are consistent with our earlier analysis but they also offer additional information on the role neighborhood size plays in understanding the geography of sex offense victimization.[50] First, as expected, the β_3s for sex offenses against children and forcible rape are, for the most part, below zero (suggesting that notification reduced victimization risk) and the β_3s for sex offenses against adults are above zero (suggesting the opposite), with the β_3s for all sex offenses regularly crossing back and forth from positive to negative territory. Second, although the figure is very noisy, it is possible to discern a general arc as the radius of the neighborhood grows. The effect that notification has on victimization risk seems to be most beneficial at around 0.5 miles and least beneficial at about 1 mile. Not surprisingly, the lines begin to run closer together as the radius approaches 3 miles. One can interpret this figure as generally suggesting that choosing the size of the neighborhood (or the scope of notification, assuming that neighborhoods cannot easily be resized) is as important as deciding whether to employ notification in the first place. Another possibility raised by these patterns is that notification may be more advantageous (and thus also more harmful) in certain geographies than in others.

VII. Conclusions

In this article, we explore the interaction of sex offender laws and the geography of victimization. We focus on two questions. First, as policymakers and the public assume, is someone more likely to report being victimized, all else equal, in a neighborhood in which relatively more RSOs (or registered sex offenders) live? Second, whatever the relationship between RSO residency and reported neighborhood sex offense risk, does this relationship change in response to implementing a notification regime in which the public is informed of RSO identity and address information via the Internet?

Using data from Baltimore County, we find evidence that, in general, a neighborhood with an additional RSO is counterintuitively safer than (not just as safe as, but actually safer than) an otherwise comparable neighborhood. Yet the picture is more complicated than it first appears. Under registration, for sex offenses against adults and peeping, pornography, and prostitution offenses, neighborhoods with additional RSOs are safer than comparable neighborhoods. Post-

[50] For purposes of comparison, if we were to construct a circular neighborhood that contained the same area as the average Census block group, that radius would be approximately 0.66 miles.

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notification, these neighborhoods remain safer than comparable neighborhoods, but by a smaller margin. For forcible rape and sex offenses against children, however, the pattern runs in the opposite direction. Additional RSOs in a neighborhood have no relationship or a slightly positive relationship with the frequency of these crimes under a registration regime, but notification appears to reduce these risks significantly, leaving neighborhoods with RSOs, again, safer than comparable neighborhoods.

We interpret these empirical patterns through a conceptual framework that recognizes that NonRSOs (potential or first-time offenders) commit most sex crime *and* that sex offender laws (like notification) can indirectly affect the spatial offending patterns of these individuals. We use our conceptual framework to present hypotheses for why the particular patterns in the data may make sense. Our conceptual model also highlights other concerns that we attempt to address in our empirical work, including the possibility of a nonlinear relationship between the number of RSOs and victimization risk in a neighborhood, spillovers across neighborhoods, and alternative definitions of neighborhoods.

An important caveat to our conclusions is that we study *reported* crime, not *actual* crime. While we are one step ahead of analyses that focus on arrests, our crime data do not represent the true, underlying numbers of crimes and so our victimization risk measures are necessarily inaccurate. Moreover, sex offender laws have the potential to affect reporting behaviors. After learning that an RSO lives nearby, an individual may become more apt to report a suspicious man in the playground; if a local RSO leads parents to discuss how to deal with sexual abuse with their children, those children may be more likely to report something unusual. Once an individual learns that a single RSO resides in her neighborhood, it may not matter how many additional RSOs live in the neighborhood if that potential victim is able to fully protect herself.

In the end, the key descriptive findings that emerge from our neighborhood analysis are (1) that risk of victimization appears at worst *unrelated* to the close proximity of an RSO, and (2) that making the identity and location of RSOs public has the potential to *increase* the likelihood of reported sex offenses in the neighborhoods in which RSOs live, with forcible rape and sex offenses against children as potential exceptions. Both patterns are quite surprising given the seemingly commonsense and intuitive assumption that living near an RSO has simply *got* to make the risk of becoming a victim higher, not lower, and the equally natural idea that notifying the public of where RSOs live can only make their neighbors safer. Our findings also raise important questions for future research, including *why* sex offense victimization risk appears to *increase* with respect to some types of crime when notification publicly identifies RSOs, and how policymakers should respond to this unexpected possibility.

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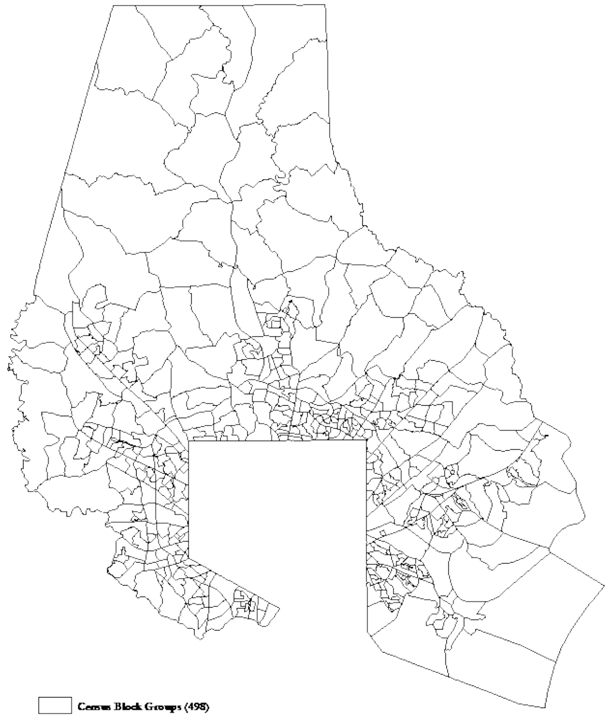

Figure 1: Baltimore County Census Block Groups (CBGs)

(Source: Census Block Groups: 2009 TIGER/Line Shapefiles – Baltimore County Census Blocks)

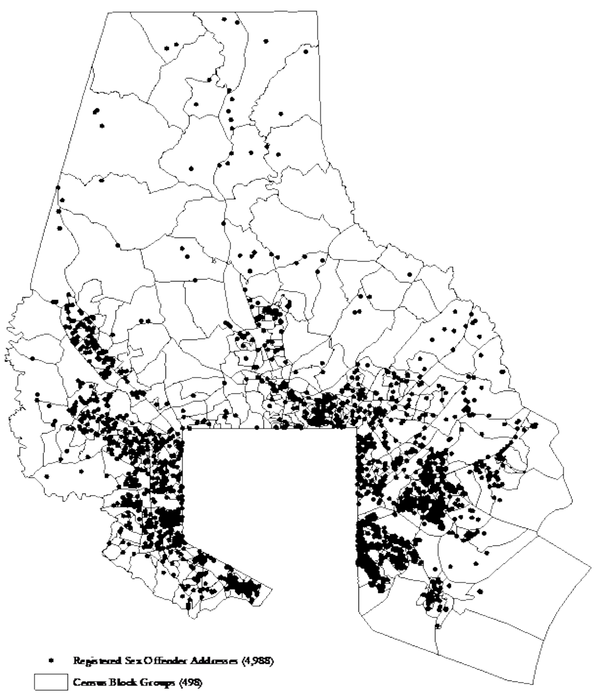

Figure 2: Baltimore County RSO Addresses

(Feb. 2001 through Dec. 2008)

Notes: All matched Baltimore County RSO addresses (4,988) are depicted. In our analysis, we remove detention facility addresses, resulting in a final tally of 4,123 RSO addresses.

(Source: Maryland Sex Offender Registry Unit)

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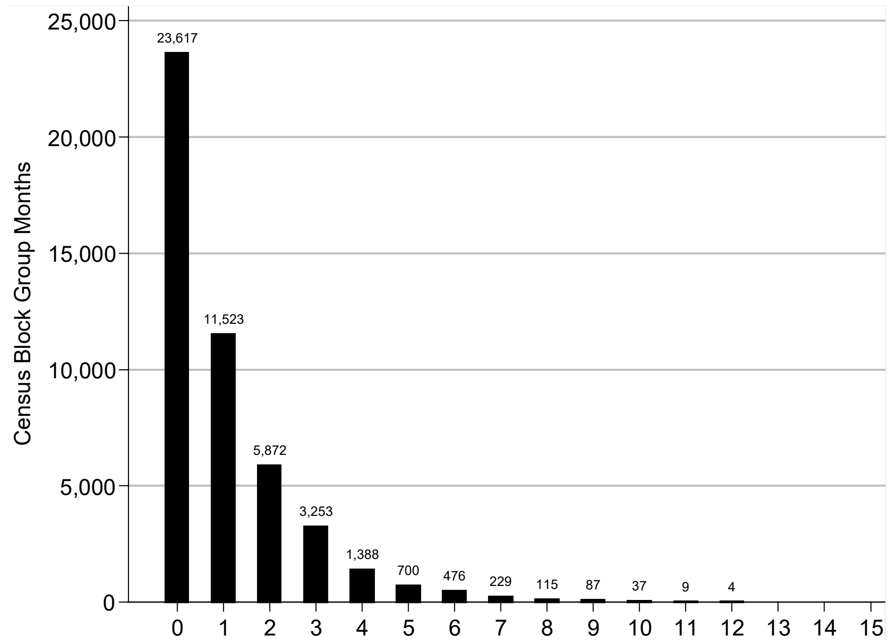

Figure 3: Number of RSOs per CBG-Month
(Baltimore County, Feb. 2001 through Dec. 2008)
(Source: Maryland Sex Offender Registry Unit)

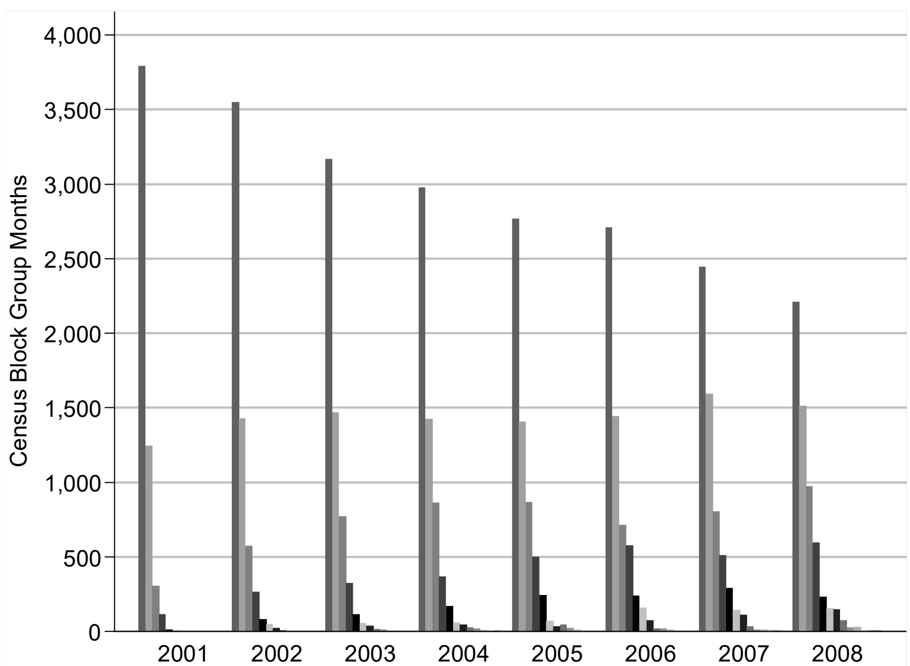

Figure 4: Numbers of RSOs per CBG-Month by Year
(Baltimore County, Feb. 2001 through Dec. 2008)
(Source: Maryland Sex Offender Registry Unit)

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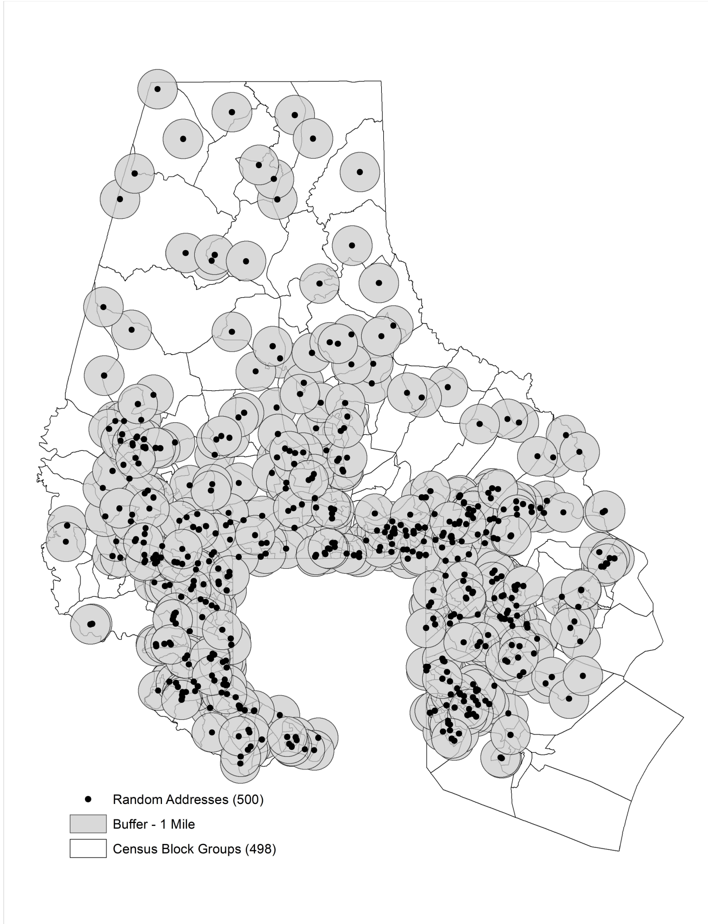

Figure 5: Random Address Circular Neighborhoods
(Source: Address Points Shapefile, Baltimore County Geographic Information Systems (GIS))

Figure 6: Maryland Sex Offender Registry Search Results
(1/2 mile radius, January 2014)
(Source: http://www.dpscs.state.md.us/sorSearch/)

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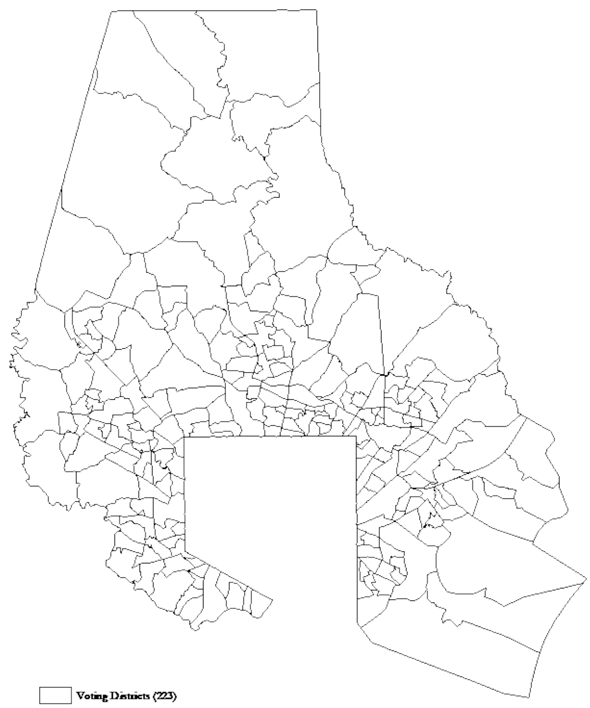

Figure 7: Baltimore County Voting Districts
(Source: 2010 TIGER/Line Shapefiles – Voting Districts)

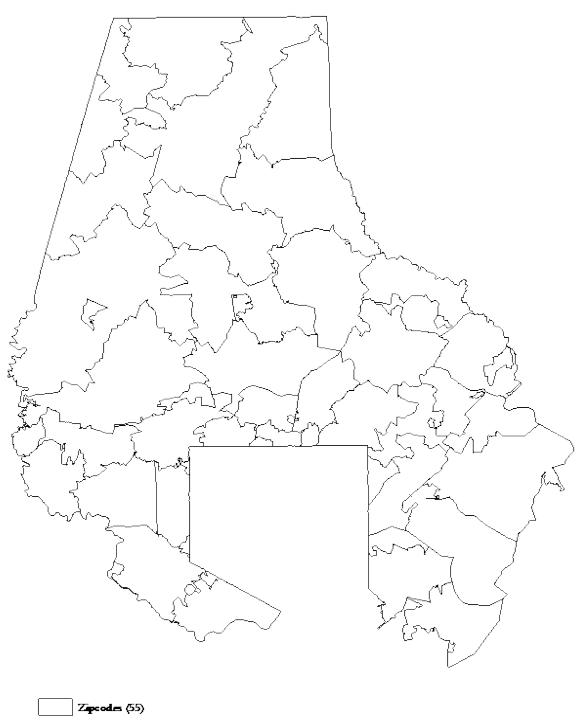

Figure 8: Baltimore County ZIP Codes
(Source: Zip codes: 2010 TIGER/Line Shapefiles –

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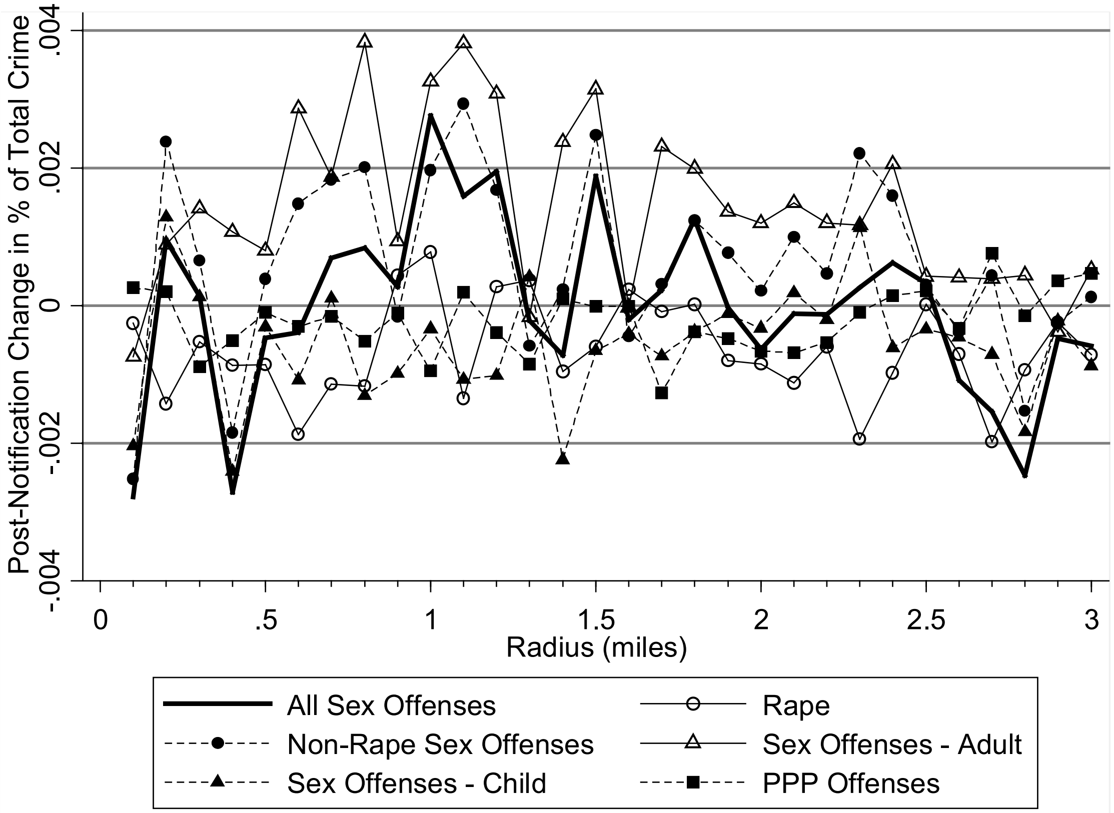
5-digit ZIP Code Tabulation Areas (ZCTAs))

Figure 9: Relative Crime Risk Analysis



Notes: Each symbol represents a β_3 estimated in equation (3) in the text. The regressions are performed for every 0.1 mile band around the RSO and random addresses from 0 to 3 miles.

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Table 1: Summary Statistics

(Census Block Group by Month Cells)

	(1) All CBG-Months	(2) CBG-Months With Resident RSOs	(3) CBG-Months Without Resident RSOs
Number of Resident RSOs	1.021	1.999	0.000
	(1.45)	(1.48)	(0.00)
Sex Offenses Reported	0.106	0.136	0.074
	(0.35)	(0.40)	(0.30)
Rape	0.023	0.031	0.016
	(0.16)	(0.18)	(0.13)
All Nonrape Sex Offenses	0.083	0.106	0.059
	(0.31)	(0.35)	(0.27)
Sex Offenses Against Adults (not including rape)	0.049	0.063	0.034
	(0.23)	(0.26)	(0.20)
Sex Offenses Against Children	0.023	0.030	0.016
	(0.16)	(0.18)	(0.13)
Peeping, Pornography, and Prostitution	0.011	0.013	0.009
	(0.12)	(0.12)	(0.12)
Violent Crimes Reported	0.688	0.903	0.463
	(1.10)	(1.24)	(0.88)
Nonviolent Crimes Reported	3.725	4.494	2.922
	(4.91)	(5.37)	(4.24)
% Students Eligible for Free/Reduced-Price Lunch	8.992	12.587	5.235
	(18.52)	(21.86)	(13.21)
% Tot Pop of Pre-K to 5th Grade Enrolled	6.819	7.717	5.881
	(15.34)	(15.55)	(15.06)
No. of Observations	47,310	24,171	23,139

Notes: Column (1) displays averages and standard deviations (in parentheses) for our sample of Census-block-group-months. Columns (2) and (3) show averages and standard deviations for those CGB-months in which at least one registered sex offender (RSO) lived in the neighborhood for part of the month and for those CBG-months in which no RSO lived in the neighborhood during the month, respectively.

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Table 2: Relationship Between the Number of RSOs and Neighborhood Victimization Risk

(Census Block Group Neighborhoods)

	(1) All Sexual Offenses	(2) Forcible Rape	(3) Nonrape Sexual Offenses	(4) Sexual Offenses (Adult)	(5) Sexual Offenses (Children)	(6) Peeping, Pornography, Prostitution	(7) (Other) Violent Offenses	(8) Nonviolent Offenses
Number of RSOs	0.925**	0.935	0.921**	0.947	0.957	0.755**	1.011	1.001
	(0.029)	(0.058)	(0.034)	(0.044)	(0.062)	(0.096)	(0.015)	(0.012)
Number of RSOs Squared	1.003	1.005	1.002	1.002	0.995	1.013	0.997	1.000
	(0.004)	(0.007)	(0.004)	(0.006)	(0.008)	(0.015)	(0.002)	(0.002)
% Eligible Free/Reduced-Price Lunch	1.006	0.981*	1.014**	1.012*	1.024*	1.007	1.003	1.004**
	(0.006)	(0.011)	(0.006)	(0.007)	(0.013)	(0.015)	(0.002)	(0.002)
School Enrollment	0.996	0.990	0.997	0.998	0.989	1.000	0.998	1.000
	(0.011)	(0.017)	(0.012)	(0.014)	(0.021)	(0.023)	(0.003)	(0.003)
Block Group Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Month-of -Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	45,030	34,105	44,460	40,850	34,865	18,620	47,120	47,310

Notes: The dependent variable for each regression is the number of crimes listed in the column heading in a Census block group neighborhood in a particular month. Incidence rate ratios from conditional fixed-effect Poisson regressions are reported. School Enrollment is the number of children enrolled in pre-K through fifth grade in the neighborhood (but not necessarily residing in the neighborhood) divided by the total population residing in the neighborhood. % Eligible Free/Reduced-Price Lunch is the percentage of enrolled students in the neighborhood who are eligible for free or reduced-price lunches at school. All columns include neighborhood, year, and month-of-year fixed effects. Robust (sandwich) standard errors are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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Table 3: Relationship Between the Number of RSOs and Neighborhood Victimization Risk

(Census Block Group Neighborhoods)

	(1) All Sexual Offenses	(2) Forcible Rape	(3) Nonrape Sexual Offenses	(4) Sexual Offenses (Adult)	(5) Sexual Offenses (Children)	(6) Peeping, Pornography, Prostitution	(7) (Other) Violent Offenses	(8) Nonviolent Offenses
Number of RSOs	0.897	1.115	0.834*	0.823	1.019	0.525**	1.019	1.019
	(0.076)	(0.190)	(0.082)	(0.118)	(0.151)	(0.137)	(0.042)	(0.028)
Number of RSOs Squared	1.024	0.969	1.041*	1.037	1.009	1.116*	0.998	0.993
	(0.019)	(0.047)	(0.023)	(0.038)	(0.031)	(0.069)	(0.012)	(0.008)
Notification (Internet)	1.012	1.151	0.959	1.010	1.070	0.654*	1.050	0.901***
	(0.102)	(0.232)	(0.113)	(0.177)	(0.230)	(0.165)	(0.043)	(0.024)
RSOs × Notification	1.026	0.827	1.104	1.159	0.917	1.457	0.990	0.984
	(0.091)	(0.139)	(0.112)	(0.175)	(0.142)	(0.356)	(0.039)	(0.029)
RSOs Squared × Notification	0.980	1.040	0.962*	0.966	0.990	0.905	0.999	1.007
	(0.018)	(0.049)	(0.022)	(0.037)	(0.031)	(0.055)	(0.012)	(0.008)
% Eligible Free/Reduced-Price Lunch	1.006	0.980*	1.014**	1.012*	1.023*	1.007	1.003	1.004**
	(0.006)	(0.011)	(0.006)	(0.007)	(0.013)	(0.015)	(0.002)	(0.002)
School Enrollment	0.996	0.989	0.998	0.999	0.989	1.001	0.998	1.000
	(0.011)	(0.017)	(0.012)	(0.014)	(0.021)	(0.022)	(0.003)	(0.003)
Block Group Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Month-of -Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	45,030	34,105	44,460	40,850	34,865	18,620	47,120	47,310

Notes: The dependent variable for each regression is the number of crimes listed in the column heading in a Census block group neighborhood in a particular month. Incidence rate ratios from conditional fixed-effect Poisson regressions are reported. School Enrollment is the number of children enrolled in pre-K through fifth grade in the neighborhood (but not necessarily residing in the neighborhood) divided by the total population residing in the neighborhood. % Eligible Free/Reduced-Price Lunch is the percentage of enrolled students in the neighborhood who are eligible for free or reduced-price lunches at school. All columns include neighborhood, year, and month-of-year fixed effects. Robust (sandwich) standard errors are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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Table 4: Relationship Between the Number of RSOs and Neighborhood Victimization Risk

(Circular Neighborhoods — One Mile Radius)

	(1) All Sexual Offenses	(2) Forcible Rape	(3) Nonrape Sexual Offenses	(4) Sexual Offenses (Adult)	(5) Sexual Offenses (Children)	(6) Peeping, Pornography, Prostitution
Number of RSOs	0.976**	1.024	0.960***	0.917***	1.02	1.014
	(0.010)	(0.019)	(0.012)	(0.017)	(0.021)	(0.038)
Number of RSOs Squared	1.000	0.999	1.000	1.002**	0.997***	0.998
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Notification (Internet)	0.951	1.692***	0.793***	0.688***	1.267**	0.538***
	(0.050)	(0.174)	(0.047)	(0.057)	(0.129)	(0.080)
RSOs × Notification	1.012	0.950***	1.032***	1.087***	0.944***	1.015
	(0.010)	(0.015)	(0.012)	(0.018)	(0.018)	(0.031)
RSOs Squared × Notification	1.000	1.001*	1.000	0.999**	1.004***	1.000
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
% Eligible Free/Reduced-Price Lunch	1.003	1.009*	1.001	1.007	1.003	0.982
	(0.003)	(0.006)	(0.004)	(0.005)	(0.008)	(0.015)
Block Group Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
Month-of -Year Fixed Effects	✓	✓	✓	✓	✓	✓
Observations	45,410	41,135	45,125	43,890	41,990	36,100

Notes: The dependent variable for each regression is the number of crimes listed in the column heading in a circular neighborhood (radius of one mile) in a particular month. Incidence rate ratios from conditional fixed-effect Poisson regressions are reported. School Enrollment is the number of children enrolled in pre-K through fifth grade in the neighborhood (but not necessarily residing in the neighborhood) divided by the total population residing in the neighborhood. % Eligible Free/Reduced-Price Lunch is the percentage of enrolled students in the neighborhood who are eligible for free or reduced-price lunches at school. All columns include neighborhood, year, and month-of-year fixed effects. Robust (sandwich) standard errors are presented in parentheses. Standard errors calculated in line with Bertanha and Moser (2014) are reported in Appendix AIII.*** p<0.01, ** p<0.05, * p<0.1.

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Table 5: Relationship Between the Number of RSOs and Neighborhood Victimization Risk

(Census Block Group Neighborhoods — Alternative Nonlinearity Specification)

	(1) All Sexual Offenses	(2) Forcible Rape	(3) Nonrape Sexual Offenses	(4) Sexual Offenses (Adult)	(5) Sexual Offenses (Children)	(6) Peeping, Pornography, Prostitution
1(0 < RSO ≤ 1)	0.899	1.019	0.857	0.826	1.103	0.517**
	(0.088)	(0.188)	(0.098)	(0.126)	(0.214)	(0.163)
1(1 < RSO ≤ 3)	0.902	0.972	0.866	0.813	1.149	0.510*
	(0.104)	(0.237)	(0.112)	(0.145)	(0.235)	(0.181)
1(RSO > 3)	0.937	0.734	1.000	0.884	1.391	0.469
	(0.168)	(0.292)	(0.199)	(0.314)	(0.544)	(0.355)
Notification (Internet)	0.993	1.067	0.956	0.966	1.104	0.727
	(0.103)	(0.213)	(0.117)	(0.173)	(0.251)	(0.183)
1(0 < RSO ≤ 1) × Notification	1.022	0.922	1.061	1.238	0.841	1.071
	(0.113)	(0.194)	(0.135)	(0.202)	(0.176)	(0.399)
1(1 < RSO ≤ 3) × Notification	1.012	1.049	1.015	1.227	0.753	1.057
	(0.123)	(0.255)	(0.145)	(0.239)	(0.165)	(0.381)
1(RSO > 3) × Notification	0.773	0.921	0.733	0.953	0.519*	0.881
	(0.140)	(0.387)	(0.146)	(0.340)	(0.202)	(0.642)
% Eligible Free/Reduced-Price Lunch	1.006	0.981*	1.013**	1.012*	1.022*	1.001
	(0.006)	(0.011)	(0.007)	(0.007)	(0.013)	(0.014)
School Enrollment	0.996	0.990	0.998	0.999	0.989	1.003
	(0.011)	(0.017)	(0.013)	(0.015)	(0.020)	(0.022)
Block Group Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
Month-of -Year Fixed Effects	✓	✓	✓	✓	✓	✓
Observations	45,030	34,105	44,460	40,850	34,865	18,620

Notes: The dependent variable for each regression is the number of crimes listed in the column heading in a Census block group neighborhood in a particular month. Incidence rate ratios from conditional fixed-effect Poisson regressions are reported. School Enrollment is the number of children enrolled in pre-K through fifth grade in the neighborhood (but not necessarily residing in the neighborhood) divided by the total population residing in the neighborhood. % Eligible Free/Reduced-Price Lunch is the percentage of enrolled students in the neighborhood who are eligible for free or reduced-price lunches at school. All columns include neighborhood, year, and month-of-year fixed effects. Robust (sandwich) standard errors are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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Table 6: Relationship Between the Number of RSOs and Neighborhood Victimization Risk

(Census Block Group Neighborhoods — Spillover Correction)

	(1) All Sexual Offenses	(2) Forcible Rape	(3) Nonrape Sexual Offenses	(4) Sexual Offenses (Adult)	(5) Sexual Offenses (Children)	(6) Peeping, Pornography, Prostitution
Number of RSOs	0.919	1.120	0.861	0.859	1.037	0.509**
	(0.081)	(0.196)	(0.086)	(0.124)	(0.158)	(0.134)
Number of RSOs Squared	1.022	0.969	1.039*	1.033	1.008	1.127**
	(0.018)	(0.047)	(0.022)	(0.037)	(0.031)	(0.066)
Avg. Adjacent RSOs	0.974	0.991	0.967*	0.960	0.971	0.974
	(0.016)	(0.034)	(0.018)	(0.024)	(0.029)	(0.037)
Notification (Internet)	0.917	1.122	0.851	0.862	0.981	0.622*
	(0.098)	(0.255)	(0.109)	(0.160)	(0.236)	(0.169)
RSOs × Notification	0.997	0.824	1.062	1.101	0.901	1.515*
	(0.092)	(0.144)	(0.110)	(0.168)	(0.143)	(0.355)
RSOs Squared × Notification	0.983	1.040	0.965	0.969	0.991	0.896*
	(0.018)	(0.049)	(0.021)	(0.036)	(0.032)	(0.051)
Avg. Adjacent RSOs × Notification	1.026*	1.006	1.033**	1.044**	1.022	1.008
	(0.014)	(0.031)	(0.016)	(0.022)	(0.027)	(0.032)
% Eligible Free/Reduced-Price Lunch	1.005	0.980*	1.013**	1.011*	1.023*	1.008
	(0.006)	(0.011)	(0.006)	(0.007)	(0.013)	(0.015)
School Enrollment	0.996	0.989	0.998	0.998	0.989	1.000
	(0.011)	(0.017)	(0.012)	(0.014)	(0.021)	(0.023)
Block Group Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
Month-of -Year Fixed Effects	✓	✓	✓	✓	✓	✓
Observations	45,030	34,105	44,460	40,850	34,865	18,620

Notes: The dependent variable for each regression is the number of crimes listed in the column heading in a Census block group neighborhood in a particular month. Incidence rate ratios from conditional fixed-effect Poisson regressions are reported. School Enrollment is the number of children enrolled in pre-K through fifth grade in the neighborhood (but not necessarily residing in the neighborhood) divided by the total population residing in the neighborhood. % Eligible Free/Reduced-Price Lunch is the percentage of enrolled students in the neighborhood who are eligible for free or reduced-price lunches at school. All columns include neighborhood, year, and month-of-year fixed effects. Robust (sandwich) standard errors are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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Appendix

AI. Data Construction

AI.A. Sex Offender Registry Data

We obtained registered home address information from the Maryland Sex Offender Registry Unit via a Maryland Public Information Request. The data we received included the offender's name, date of birth, registered addresses, county of registered addresses, date (and sometimes time of day) an address was reported, and initial registration date. These data contain all registration reports for registered offenders made after January 1, 2001—i.e., we received previous as well as current registered home address information.[51] July 8, 2010, marks the last report in our data, around the time we acquired the information.

Maryland's Sex Offender Registry Unit compiles its sex offender address data by collecting information from each of Maryland's counties. Although Maryland's registered sex offenders (RSOs) must verify their registration information as often as several times each year, some counties appear only to report address information to the Unit when the registrant actually moves while others report each registration event even if the offender re-registers the same address. In our work, because we are interested only in where registrants lived at particular points in time, we eliminated any address report in which an RSO registered an address that the offender had previously registered, so long as the registrant did not become incarcerated or move to another location in between those two address verification reports.

AI.B. Crime Data

From the Baltimore County Police Department, we obtained all Uniform Crime Report Part I crimes and Part II sex crimes reported between January 1, 1990, and early 2009. The data include the type of crime, the date reported, the time reported (if available), and the address of the incident. The 2009 data contained implausibly low counts of crimes. Consequently, we dropped this period in our analysis, using reported crime data only through the end of 2008. Roughly 740,000 total crimes were reported between January 1, 1990, and December 31, 2008, of which approximately 15,750 were sex crimes. To facilitate the geocoding process, we also dropped all incidents from the 1990s (we have no sex offender registrant information during this period). We geocoded the remaining reported incidents, approximately 304,000 crimes.

AI.C. School Enrollment Data

We acquired student enrollment and school demographics data from the National Center for Education Statistics (NCES). Using NCES's Public Elementary/Secondary School Universe Survey for the 2000 through 2009 school years, we built a school-level panel dataset for Baltimore County schools that includes the number of students per grade and the percentage of students who are eligible for free and reduced-price lunches.[52]

[51] January 1, 2001, is the date Maryland began using its current electronic system, and so no records of past addresses exist (that we were able to locate) before this date despite the fact that Maryland's registry began in the 1990s.

[52] The Baltimore County schools student enrollment and free and reduced-price lunch eligibility data we used in

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AI.D. ArcGIS: Geocoding, Joining, and Adjacency Calculations

We used ArcGIS to translate address fields into coordinate points and then placed these points into neighborhoods defined by geographic boundaries such as census block groups, voting districts, and zip codes. The tools used in ArcGIS to prepare the data for analysis included geocoding, spatial joining, and adjacency calculations. To summarize the process, the geocoding tool in ArcGIS takes address fields as inputs and, using an address locator, provides latitude and longitude coordinates as outputs.[53] ArcGIS then places the location of each RSO reported address and each reported crime on a Maryland map.[54]

Overall, during the geocoding step, we were able to match 73% of registrant addresses, with matched addresses located both within and outside of Baltimore County, and 81% of the Baltimore County crimes. Reasons for failing to match particular RSO addresses include: the address was located outside of Maryland, the address indicated that the individual was homeless (e.g., "00000 Homeless Street, Baltimore County, MD"), and the address was simply misspelled and ArcGIS was unable to match the reported address with the correct address confidently.[55] If we remove unmatchable cases—e.g., homeless addresses, outside-of-Maryland addresses, and addresses containing clearly invalid information—the 73% match rate for RSO addresses (33,594/46,193 = 72.7%) improves to approximately 83% (33,594/40,597 = 82.7%).

The next step, spatial joining, took the coordinate points produced by geocoding and placed them onto boundary maps of Baltimore County, using a different map for each neighborhood definition (e.g., census block groups).[56] We successfully joined 4,988 RSO addresses. Roughly 10% to 15% of Maryland residents live in Baltimore County, which lines up well with the percentage of all Maryland RSO addresses that appear to belong in Baltimore County (5,723/46,193 = 12.4%). Of the cases that appear to belong in Baltimore County, we were able to match approximately 87% (4,988/5,723 = 87.2%). [57]

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our analysis can be found at http://nces.ed.gov/ccd/pubschuniv.asp (last visited August 10, 2014).

[53] For specific information regarding the address locator and steps for geocoding address data, please refer to http://imap.maryland.gov/Documents/Training/HowToAddMDiMap1.0WebServicesInArcGISDesktop.pdf and http://imap.maryland.gov/Documents/Training/CompositeLocatorInstructions.pdf (last visited August 10, 2014).

[54] The Baltimore County schools were not geocoded as we obtained each school's coordinates from a public schools shapefile. This shapefile can be downloaded using the Baltimore County GIS FTP server, located at http://www.baltimorecountymd.gov/Agencies/infotech/GIS/datadownload.html (last visited August 10, 2014).

[55] ArcGIS's geocoding tool has a built-in tolerance for spelling errors. ArcGIS is often able to match an address reported with misspelled words, yet sufficiently discernable information (e.g., "Baltmore," Baltimore without an "i"), but more significant misspellings may not result in matches. We used the default setting in ArcGIS for "Spelling Sensitivity," which is a score of 80 (out of a possible 100).

[56] We did not join addresses located outside of Baltimore County. For our purposes, because we only have crime data from Baltimore County, we only needed to know when an RSO moved out of the county, not where outside of the county the individual moved. Addresses that were not successfully geocoded also could not be joined.

[57] We only received crime data for incidents reported in Baltimore County, and the address information for these incidents contained few clearly invalid entries (e.g., "0000 Unknown Street"). As a result, the match and join rates for this data were similar: 80.53% and 80.37%, respectively.

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Finally, we used polygon neighbors in ArcGIS, which provides, for each neighborhood, a list of all adjacent neighborhoods. Knowing which neighborhoods share borders allowed us to calculate adjacent neighborhood RSO numbers, which we used in our spillover analyses.

AI.E. Creating the Final Neighborhood Panel Datasets

Using the results of our ArcGIS work, we built a panel dataset that included the number of registered sex offenders, the number of crimes by type, and basic school demographics by month for each fixed neighborhood definition.

We took the following additional steps in assembling our monthly data. First, we removed duplicate reports of the same address with no intervening moves by comparing coordinate points and eliminating those that were the same. Second, because many RSOs do not move at the end of a calendar month, we allocated RSOs across their reported addresses within a given month by the fraction of the month they lived at each address. Specifically, we attributed a fraction of an RSO, equivalent to the number of days spent in a neighborhood divided by the days in that month, to that neighborhood for that month. Then, to get the total number of RSOs for each month in each neighborhood, we summed the fractions of months spent by all resident RSOs. Third, in some instances, RSOs reported multiple addresses on the same date, so we treated these individuals as having spent equal time in each location throughout the time they reported these addresses. We simply divided the RSO fraction by the number of multiple addresses for each month (e.g., if an RSO moved from address A to addresses B and C on September 21, neighborhood A received 20/30 or .6666 RSOs, and neighborhoods B and C each received (10/30)/2 or .1666 RSOs for the month of September). With respect to crime, the process was more straightforward because a crime in our data only happened in one place on a specific date. We calculated the sum of each crime type for each neighborhood for each month.

The school enrollment data we obtained were structured as panel data organized at the school-year level. We assumed that enrollment was constant for the months during the school year and that the school year ran from August 1 through July 31. We matched each school location to a neighborhood, and then aggregated the enrollment and eligibility for free and reduced lunch numbers to the neighborhood level. For neighborhoods without schools, these monthly numbers were set to zero.

We then combined the RSO address data, the crime data, and the school enrollment data into a neighborhood-by-month panel dataset. We used this data for our main census block group analyses (and voting district and zip code robustness checks).

AI.F. Creating the Circle Neighborhood and Relative Risk Analysis Dataset

To create the circle neighborhoods for our analysis, we first selected 500 random addresses from a shapefile of all residential addresses in Baltimore.[58] These addresses came already geocoded with coordinate points in the shapefile. Using the coordinate points from the shapefile, and the coordinate points of the RSO addresses and crimes geocoded previously, we calculated how many RSOs and crimes were contained within varying radii measures (0.25, 0.5, 1, 3, and 5

[58] This shapefile for all addresses in Baltimore County can be downloaded through the Baltimore County GIS FTP server, located at http://www.baltimorecountymd.gov/Agencies/infotech/GIS/datadownload.html (last visited August 10, 2014). The building codes "Residential High Density" and "Residential Low Density" were used to restrict this file to residential addresses only.

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miles) around each random address. This allowed us to create a panel data set at the random address by month level that included the number of crimes and RSOs within each radius for use in our circular neighborhood analysis. We used the same method and information for the relative risk comparison analysis. With coordinate points of random addresses, RSO addresses, and crimes, we calculated the number of crimes that occurred within varying radii (from 0.1 mile to 3 miles) of both random addresses and RSO addresses.

AII. Poisson IRR interpretation

For the Poisson regressions in our paper, we report exponentiated coefficients, also known as incidence rate ratios (IRRs). In this Appendix Section we simplify and clarify the interpretation of these coefficients.

AII.A. IRRs in Simple Poisson Regressions

Let Y be the outcome of interest expressed as counts—i.e., the number of crimes. A basic Poisson regression can be written as:

$$E(Y|X) = exp(\beta_0 + \beta_1 X) \tag{A1}$$

Here β_1 is the coefficient and $exp(\beta_1)$ is the exponentiated coefficient or IRR. If we add an additional unit of our independent variable X, we can write:

$$\begin{aligned} E(Y|X = X + 1) &= exp\big(\beta_0 + \beta_1(X + 1)\big) \\ &= exp(\beta_0 + \beta_1 X)exp(\beta_1) \\ &= E(Y|X)\, exp(\beta_1) \end{aligned}$$

This implies that an increase of one unit of X is associated with $exp(\beta_1)$ times as much of the dependent variable Y. That is, the IRR has a multiplicative interpretation.

Note if $exp(\beta_1) = 1$, then $E(Y|X = X + 1) = E(Y|X)$. That is, an additional unit of X is not associated with any change in the number of Y. If $exp(\beta_1) > 1$, then $E(Y|X = X + 1) > E(Y|X)$. That is, one additional unit of X is associated with a higher expected value of Y; similarly, if $exp(\beta_1) < 1$, then $E(Y|X = X + 1) < E(Y|X)$.

AII.B. Interpreting Interaction Terms

Interaction terms in non-linear models are generally difficult to interpret (Ai & Norton (2010). Following Buis (2010), we interpret the interaction terms in our work using an IRR framework—that is, the estimates are calculated multiplicatively. Consider a simplified version of the Poisson model with interaction terms we estimate (equation (2), Section IV) where Y is the number of crimes:

$$E(Y|RSO, POST) = exp(\beta_0 + \beta_1 RSO + \beta_2 POST + \beta_3 RSO \times POST) \tag{A2}$$

When $POST = 0$ (before internet notification), we can rewrite this as:

$$E(Y|RSO, POST = 0) = exp(\beta_0 + \beta_1 RSO)$$

The interpretation of the effect of an additional RSO on our outcome variable Y can then be worked out exactly the same as above:

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$$E(Y|RSO = RSO + 1, POST = 0) = E(Y|RSO, POST = 0) exp(\beta_1)$$

That is, before internet notification (when $POST = 0$), one additional RSO is associated with $exp(\beta_1)$ times as much crime.

When $POST = 1$ (after internet notification), we can rewrite equation (A2) as:

$$E(Y|RSO, POST = 1) = exp\big((\beta_0 + \beta_2) + (\beta_1 + \beta_3)RSO\big)$$

Adding an additional RSO implies:

$$\begin{aligned} E(Y|RSO + 1, POST = 1) &= exp\big((\beta_0 + \beta_2) + (\beta_1 + \beta_3)(RSO + 1)\big) \\ &= exp\big((\beta_0 + \beta_2) + (\beta_1 + \beta_3)RSO\big)\, exp(\beta_1)\, exp(\beta_3) \\ &= E(Y|RSO, POST = 1)\, exp(\beta_1)\, exp(\beta_3) \end{aligned}$$

In other words, following the implementation of internet notification, one additional RSO is associated with $exp(\beta_1) \times exp(\beta_3)$ times as much crime. Recall that the effect before the internet was $exp(\beta_1)$, so this effect is $exp(\beta_3)$ times as much after notification as before.

AII.C. Interpreting Squared Terms

Squared terms are a special case of interaction terms. Consider an expansion of equation (A1) to include a squared term:

$$E(Y|X) = exp(\beta_0 + \beta_1 X + \beta_2 X^2) \tag{A3}$$

Using the same type of analysis, if we add one additional unit of X we can write:

$$\begin{aligned} E(Y|X = X + 1) &= exp(\beta_0 + \beta_1(X + 1) + \beta_2(X + 1)^2) \\ &= exp(\beta_0 + \beta_1 X + \beta_1 + \beta_2 X^2 + \beta_2 2X + \beta_2) \\ &= exp(\beta_0 + \beta_1 X + \beta_2 X^2)\, exp(\beta_1)\, exp(\beta_2 + 2X\beta_2) \\ &= exp(\beta_0 + \beta_1 X + \beta_2 X^2)\, exp(\beta_1)\, exp(\beta_2(1 + 2X)) \\ &= E(Y|X)\, exp(\beta_1)\, exp(\beta_2)^{1+2X} \end{aligned}$$

When $X = 0$, we have the same conclusion as above: an additional RSO is associated with $exp(\beta_1) \times exp(\beta_2)$ times as much crime, or $exp(\beta_2)$ times the main effect. However, when $X \neq 0$, the multiplicative association is dependent on X: an additional RSO is associated with $exp(\beta_1)\, exp(\beta_2)^{1+2X}$ times as much crime.

In terms of the "sign" of any association between X and Y, a similar intuition as above holds: if $exp(\beta_2) > 0$, an additional offender is associated with *more* crime. However, to calculate the *magnitude* of the effect involves additional calculations. For example, for $X = 1$, an additional offender is associated with $exp(\beta_1) \times exp(\beta_2)^3$ times as much crime—i.e., $exp(\beta_2)^3$ times the main effect.

AIII. Spatial Correlation-Corrected Standard Errors

In our empirical analysis, spatial (or cross-sectional) correlation can bias our standard error estimates. Bias will occur if unobservable shocks to reported crime rates in a geographic area are

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not contained within neighborhoods as we define them (e.g., a census block groups). As our neighborhood boundaries may cut through true “neighborhoods” as their residents define and experience them, the threat of bias in our standard error estimates appears significant.

Bertanha and Moser (2014) recently studied this issue in the context of conditional fixed effects Poisson models like the ones we employ. They show that if any spatial correlation is time-invariant, then a typical robust (sandwich) estimator of the standard errors is consistent because the time-invariant spatial correlation is indistinguishable from the fixed effect. They also provide a test statistic to evaluate the null hypothesis that any spatial correlation in a conditional fixed effect Poisson model is time invariant. Finally, they make available analysis and code for consistent spatial variance estimators in the case of time-variant spatial dependence. For a full explanation of the details, see their working paper as well as the Stata and Matlab code available at https://www.sites.google.com/site/mbertanha/code (last visited August 10, 2014).

AIII.A Test Statistics and Spatial Standard Errors for Census Block Groups

To evaluate the reliability of our standard error estimates, we employed Bertanha and Moser’s (2014) procedure to calculate a test statistic for the null hypothesis that any spatial correlation in crime reports across census block groups (CBGs) is time invariant.

In order to estimate this test statistic, one needs to choose a bandwidth or distance to distinguish which neighborhoods are to be considered nearby. To make these calculations, we identified the centroid of each CBG and calculated the distance from each neighborhood to each other neighborhood. As suggested by Bertanha and Moser (2014), we then used percentiles of this distribution of distances to define several choices of bandwidth because, as these authors note, the researcher’s choice of bandwidth can affect the results. We use the areas within the 1st, 5th, and 10th percentiles of the distance distribution to define nearby neighborhoods.

Table A1 shows what distances these represent and how many CBGs on average are counted as “nearby” under each bandwidth definition. At the 1st percentile of latitude degree differences between centroids, approximately 2.4 CBGs are counted as nearby and at the 10th percentile, approximately 24.6 CBGs are considered nearby. In terms of longitude degree differences, these numbers are slightly smaller: at the 1st percentile of longitude degree differences between centroids, approximately 1.5 CBGs are counted as nearby and at the 10th percentile, the number is approximately 15.8 CBGs.

Table A2 shows the p-values for the test statistic that evaluates the null hypothesis of time invariant spatial correlation for the different categories of crimes for the three different bandwidths of distance between CBG centroids. None of the p-values fall below 0.10. Thus, we fail to reject the null hypothesis of time invariant spatial correlation, which indicates that robust (sandwich) standard errors are appropriate. Nevertheless, in Table A3, we reproduce the Poisson coefficients for column (1) of Table 2, [59] and, for purposes of comparison, we include standard errors that are consistent with time variant spatial correlation (calculated at three bandwidths) in addition to the more traditional sandwich standard errors. The correlation-consistent standard

[59] Because we focus here not on the interpretation of the coefficients but rather the possibility of bias in our standard errors, we report Poisson coefficients rather than IRRs.

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errors (in brackets) are very close to the sandwich standard errors (in parentheses), as one would predict given the results in Table A2.

AIII.B Test Statistics and Spatial Standard Errors for Circular Neighborhoods

One of our alternative approaches to defining neighborhoods was to draw circles of different radii around random addresses in Baltimore County. This inevitably leads to overlap in the definition of certain neighborhoods (unlike with CBGs, in which the neighborhoods, by definition, do not physically overlap), ensuring the existence of spatial correlation and, presumably, increasing the likelihood of time-variant spatial correlation.

Table A4 shows the calculated test statistics for the null hypothesis of time invariant spatial correlation, again for three different percentile bandwidths defined as in Section AIII.A. In this approach to defining neighborhoods, we found some evidence of *time-variant* spatial correlation with respect to some crimes, particularly when all neighborhoods within the 5th percentile of distances are used to define what is "nearby." At the 1st and 10th percentiles of distances, we fail to reject the null of time-invariant spatial correlation. To evaluate the robustness of our circular neighborhood results, we recalculated the results presented in Table 4 using the consistent spatial variance estimator. We report these results in Table A5. For the most part, our results do not change in terms of ultimate statistical significance, except in a few cases which we believe do not change the qualitative interpretation of our findings.

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Table A1: Degree Distance, Feet Distance, and Average Number of Neighbors for Different Bandwidths of Distances between CBG Centroids

	Latitude			Longitude		
	(1)	(2)	(3)	(4)	(5)	(6)
	Degree Distance	Feet Distance	Average Number of Neighbors	Degree Distance	Feet Distance	Average Number of Neighbors
1st Percentile	0.0012	437.11	2.40	0.0012	425.69	1.50
5th Percentile	0.0061	2221.99	12.00	0.0066	1859.82	7.90
10th Percentile	0.0129	4698.96	24.60	0.0131	3691.46	15.80

Notes: For the spatial standard error analysis, we calculate the distance from the centroid of each CBG to the centroid of each other CBG. We use the 1st, 5th, and 10th percentiles of these differences for our spatial standard error analysis. "Degree Distance" is the distance in degrees of latitude or longitude represented by each percentile, "Feet Distance" shows the results of converting the degree distance into feet using calculations from http://www.csgnetwork.com/degreelenllavcalc.html at 39.6 degrees of latitude, and "Average Number of Neighbors" reports how many CBGs on average are considered nearby at each bandwidth using this analysis.

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Table A2: Testing Null Hypothesis of Time Invariant Spatial Correlation: CBG Neighborhoods and Neighborhood Victimization Risk

(P-values for Test Statistic)

	(1) All Sexual Offenses	(2) Forcible Rape	(3) Nonrape Sexual Offenses	(4) Sexual Offenses (Adult)	(5) Sexual Offenses (Children)	(6) Peeping, Pornography, Prostitution	(7) (Other) Violent Offenses	(8) Nonviolent Offenses
1st Percentile	0.447	0.402	0.618	0.402	0.549	0.206	0.641	0.442
5th Percentile	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
10th Percentile	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000

Notes: The p-values are calculated using Stata code provided by Bertanha and Moser (2014). Each entry is a p-value for the test statistic that the spatial correlation across CBGs for the crime in question is time invariant. Thus high p-values represent a failure to reject the null hypothesis of time-invariant spatial correlation of crime across CBGs.

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Table A3: Robustness of Standard Errors in Column (1) of Table 2 to Spatial Correlation Across CBGs

	(1) All Sexual Offenses
Number of RSOs	-0.0779
	(0.0311167)
	[0.0311167] $_{1}$
	[0.0311656] $_{5}$
	[0.0309452] $_{10}$
Number of RSOs Squared	0.0029
	(0.0036006)
	[0.0036006] $_{1}$
	[0.0036197] $_{5}$
	[0.0036318] $_{10}$
Block Group Fixed Effects	✓
Year Fixed Effects	✓
Month-of -Year Fixed Effects	✓
Observations	45,030

Notes: This table re-calculates column (1) of Table 2. The coefficients are Poisson coefficients, not IRRs. The dependent variable is all sex offenses. Robust (Sandwich) standard errors are reported in parenthesis. The brackets contain spatially adjusted standard errors estimated at the 1st, 5th, and 10th percentiles of distances between centroids of CBGs.

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Table A4: Testing Null Hypothesis of Time Invariant Spatial Correlation: Circular Neighborhoods and Neighborhood Victimization Risk

(P-values for Test Statistic)

	(1) All Sexual Offenses	(2) Forcible Rape	(3) Nonrape Sexual Offenses	(4) Sexual Offenses (Adult)	(5) Sexual Offenses (Children)	(6) Peeping, Pornography, Prostitution
1st Percentile	0.337	0.355	0.368	0.252	0.089	0.089
5th Percentile	0.001	0.011	0.002	0.019	0.020	0.020
10th Percentile	0.371	0.679	0.456	0.711	0.327	0.412

Notes: The p-values are calculated using Stata code provided by Bertanha and Moser (2014). Each entry is a p-value for the test statistic that the spatial correlation across circular neighborhoods for the crime in question is time invariant. Thus high p-values represent a failure to reject the null hypothesis of time-invariant spatial correlation of crime across circular neighborhoods.

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Table A5: Relationship Between the Number of RSOs and Neighborhood Victimization Risk with Spatially Adjusted Standard Errors

(Circular Neighborhoods — One Mile Radius)

	(1) All Sexual Offenses	(2) Forcible Rape	(3) Nonrape Sexual Offenses	(4) Sexual Offenses (Adult)	(5) Sexual Offenses (Children)	(6) Peeping, Pornography, Prostitution
Number of RSOs	-0.0245	0.0240	-0.0403	-0.0869	0.0183	0.0136
	(0.010162)**	(0.0187304)	(0.0129)***	(0.018275)***	(0.0210939)	(0.0376429)
	[0.0104124]**	[0.0192736]	[0.0132722]***	[0.0187481]***	[0.0217018]	[0.0391844]
	[0.0124553]**	[0.0246435]	[0.0159021]**	[0.0233422]***	[0.0252446]	[0.0497923]
	[0.0144665]*	[0.030358]	[0.0185597]**	[0.0284566]***	[0.0297577]	[0.057888]
Number of RSOs Squared	-0.0003	-0.0008	-0.0001	0.0015	-0.0032	-0.0015
	(0.0004442)	(0.0007574)	(0.000583)	(0.0007639)**	(0.0010098)***	(0.0014342)
	[0.0004595]	[0.0007779]	[0.0006036]	[0.0007776]*	[0.0010437]***	[0.0014471]
	[0.0005428]	[0.0009817]	[0.0007062]	[0.000949]	[0.0012342]**	[0.0018163]
	[0.000646]	[0.0011594]	[0.000837]	[0.0011103]	[0.0014949]**	[0.0021027]
Notification (internet)	-0.0506	0.5260	-0.2321	-0.3740	0.2366	-0.6196
	(0.0525066)	(0.1027325)***	(0.0588884)***	(0.0828301)***	(0.10167)**	(0.1494415)***
	[0.0540772]	[0.1051825]***	[0.0611425]***	[0.0854268]***	[0.105242]**	[0.1577763]***
	[0.0695579]	[0.1354118]***	[0.0793137]***	[0.1119806]***	[0.1339162]*	[0.1878392]***
	[0.0901329]	[0.1794066]***	[0.1012957]**	[0.1453138]**	[0.1747304]	[0.2193778]***
RSOs × Notification	0.0116	-0.0516	0.0315	0.0834	-0.0581	0.0150
	(0.009492)	(0.0160552)***	(0.0118702)**	(0.0166175)***	(0.019067)***	(0.0309005)
	[0.0098318]	[0.0165194]***	[0.0124084]**	[0.0172247]***	[0.0196326]***	[0.031839]
	[0.0119919]	[0.0208309]**	[0.0151903]**	[0.0220812]***	[0.0227318]**	[0.0394287]
	[0.0143501]	[0.0257131]**	[0.0181395]*	[0.0273501]***	[0.0271711]**	[0.0453522]
RSOs Squared × Notification	0.0003	0.0013	0.0001	-0.0015	0.0036	0.0002
	(0.0004486)	(0.0007286)*	(0.0005866)	(0.0007494)**	(0.0009946)***	(0.0013826)
	[0.0004655]	[0.000749]*	[0.0006101]	[0.0007642]*	[0.0010266]***	[0.0013941]
	[0.0036197]	[0.0009445]	[0.0007173]	[0.0009447]	[0.0012064]***	[0.001714]
	[0.0006689]	[0.0011126]	[0.0008592]	[0.0011103]	[0.00147]**	[0.0019804]
Block Group Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
Month-of -Year Fixed Effects	✓	✓	✓	✓	✓	✓
Observations	45,410	41,135	45,125	43,890	41,990	36,100

Notes: This table re-calculates Table 4 with Poisson coefficients shown rather than IRRs. The dependent variable for each regression is the number of crimes listed in the column heading in a circular neighborhood (radius of one mile) in a particular month. Robust (Sandwich) standard errors are shown in parenthesis. The brackets contain spatially adjusted standard errors (calculated in line with Bertanha and Moser (2014)) estimated at the 1st, 5th, and 10th percentiles of distances between centroids of CBGs. *** p<0.01, ** p<0.05, * p<0.1.

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