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Criminological Highlights

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Criminological Highlights is designed to provide an accessible look at some of the more interesting criminological research that is currently being published. Each issue contains “Headlines and Conclusions” for each of 8 articles, followed by one-page summaries of each article.

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This issue of *Criminological Highlights* addresses the following questions:

1. What is the effect of arresting a leader of a gang?
2. Can the police affect ordinary citizens’ level of civic engagement?
3. How can high density police patrols be used to reduce crime?
4. Do police ‘stop, question, and frisk’ activities reduce crime?
5. Do Black and White Americans see crime in the same way?
6. Does the early release of drug offenders affect their likelihood of re-offending?
7. Does getting a job stop serious offenders from re-offending?
8. Does restricting where sex offenders can live affect future offending?

Arresting gang leaders can increase violent crime.

The arrest of the two gang leaders had no impact on *long-term* rates of violent crime, but in the area occupied by the less ‘corporate’ gang, it created a one month spike in violent crime related to the establishment of control over the leaderless gang’s former territory. There was no effect on violent crime of the arrest of the more ‘corporate’ gang leader. “Law enforcement may play an important role in the social structure of gang violence through interventions targeting gang leadership that may trigger episodes of retaliatory violence by disrupting a neighbourhood’s social order” (p. 162). Or, the arrest of the gang leader can have no effect. It depends on the structure of the gang.

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Unproductive police stops of ordinary citizens leads to political alienation, distrust and, more generally, civic disengagement for people living in areas targeted by the police.

The results demonstrate that “when police search a higher number of citizens or deploy more force in their stops of community members, people become much less likely to make claims on local government” (p. 217). The results were, however, different for the density of stops that did not involve ‘surplus’ force, suggesting that it is the “quality of policing, not merely the quantity” (p. 217) that makes the difference.

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Focusing police patrols on high crime areas can reduce the incidence of some types of crimes if the police do more than merely increase the frequency of their patrols.

The study demonstrates that high density police patrols, combined with certain police-initiated activities, can reduce certain firearms crimes. The fact that the reduction in crime was limited to firearms assaults and not firearms robberies is puzzling. “Certainty of arrests and occupied vehicle checks (but none of the other enforcement activities [that were] examined, were associated with reductions in nondomestic firearms assaults” (p. 446) during the period when the high intensity patrols were taking place. The study also underlines the importance of having a control group. Because there was a control group, it was possible to see that the size of the crime reduction in high intensity patrol areas that did not have enhanced police-initiated activities was not significantly greater than the reduction that occurred ‘naturally’ in the control areas. Crime rates are not consistent over time making ‘no treatment’ control groups crucial if one wants to determine whether an innovation has an impact.

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The police practice of “Stop, question, and frisk” appears to be an ineffective way to reduce street crime.

The results “show few significant effects of several ‘stop, question and frisk’ (SQF) measures on precinct robbery and burglary rates” (p. 116) and those results that are significant do not hold across crimes or type of analyses. A cautious conclusion might be that one “cannot conclude from the current investigation that SQF has no impact on crime in New York. But we can be more certain that, if there is an impact, it is so localized and dissipates so rapidly that it fails to register in annual precinct crime rates, much less the decade-long citywide crime reductions that public officials have attributed to the policy. If SQF is effective, but its effects are highly focused and fleeting, policy-makers must decide whether expansions in a policy that already produces nearly 700,000 police stops a year are warranted, especially given the ongoing controversy regarding the disproportionate impact of SQF on racial and ethnic minorities and the possibility that it reduces police legitimacy, which may erode its crime-reduction effects over the long term” (p. 117-118).

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Race and crime are intimately linked in the US: White and Black Americans explain crime in different ways, favour different policies, and experience crime in quite different ways.

Blacks' and Whites' experiences with victimization and the criminal justice system are different. Their perceptions of the system reflect, to some extent, these differences. Given that Whites and Blacks have different implicit 'theories' of the causes of crime, it is not surprising that they come to different conclusions about how best to deal with crime.

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Public safety is not compromised by retroactively shortening sentences.

It would appear that an unexpected reduction of over 2 years in prison for these cocaine offenders did not encourage them to re-offend. Various analyses suggest that the re-offending rates for various subgroups of these offenders did not differ significantly from the re-offending rates for those who served the sentences they expected when they were first sentenced. For these drug offenders, then, unexpected early release did not lead to changes in offending rates.

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For people with serious criminal records, getting a job is more likely to be a sign that they have stopped offending than it is to be a cause of their decision to stop offending.

It appears that for serious offenders, a period of criminal *inactivity precedes* getting stable employment – a decrease that does not occur for comparable people who do not, subsequently, find employment. Hence it appears that “a significant reorientation in life priorities is a necessary precondition for voluntary job entry among men with an extensive history of criminal offending.... [The] results contradict the turning point hypothesis and [are consistent with] the maturation perspective and the hook-for-change hypothesis, both of which assume individual change prior to job entry” (p. 286).

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Residence restrictions placed on convicted sex offenders are shown, once again, to be ineffective.

It would be difficult to conclude that sex offender residence restrictions are effective since they seem to have little impact on where people actually live. Furthermore, the impact of these restrictions on recidivism varies across measures (rate vs. timing of recidivism) as well as across the two states (Michigan vs. Missouri). These 2-year recidivism rates used overall reoffending (sexual offences and non-sex offences) as the outcome measure for a simple reason: there were very few sexual offences by these offenders after release. The restrictions did not appear to affect this already low rate. Another reason that one would not expect residence restrictions to affect reoffending is that residence restrictions relate to the presence of children and most of the sex offences in these two states (77% to 87%) did not involve children.

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Arresting gang leaders can increase violent crime.

Intervening in the activities of gangs can have adverse effects. For example, research has suggested that interventions can increase gang cohesiveness and strain police-community relations. Given that police sometimes 'target' gang leaders, it is important to understand the impact of decisions by the police to arrest gang leaders.

This study examines the impact on gang activity of police crackdowns on gang leaders. Most importantly, it suggests – and demonstrates – the importance of a gang's 'embeddedness' within larger networks, community structures, and other criminal groups. "Paying attention to criminal group embeddedness when evaluating gang interventions can help uncover unintended consequences because an intervention targeting one gang may have implications for the targeted gang's competitors and allies" (p. 147). More generally, this framework suggests that arrests of gang leaders can have different effects, depending on the nature of the gang.

This study examines the impact of the arrests of the highest ranking group leaders in two gangs in a Chicago neighbourhood. One gang, the Latin Kings, had a more corporate leadership structure; the other, 22 Boys, exhibited charismatic leadership. The leader of the Latin Kings answered to the gang's national leader who operated out of a federal prison. He relied on a 'corporate structure' and insulated himself from carrying out any violence. In fact, his name was not known either to community or to gang members. In contrast, the leader of the 22 Boys gang (with an estimated membership of about 150 people) knew everyone and was seen, locally, as a fixer of local problems. He

was well known for protecting residents and gang members. Each gang leader was arrested by the Chicago police and denied bail.

Two types of data were examined. First, residents were asked whether rates of violence in their neighbourhoods had changed over time. This question was asked without making reference to the arrest of the gang member. Second, monthly violent crime rates in the two gang territories where the leaders were arrested were examined. These were compared to violence rates in seven 'control' areas containing Latino gangs whose leaders were not subject to police intervention (the arrest of the gang leader). The main focus of the analysis was the level of violent crime in the gang territories in each of the 6 months following the gang leader's arrest (compared to any changes in the 'control' areas).

In the Latin Kings' territory (and in fact in adjacent territories) the quantitative data showed that the arrest of the 'corporate' style leader had no effect on violent crime. In contrast, the arrest of the 22 Boys' charismatic leader was followed by an immediate and statistically significant spike in violence in the month after the arrest, and then a reduction to pre-existing levels. Residents' reports as well as police and court documents

"confirmed that the [22 Boys] gang leader's arrest sparked an unprecedented level of violent aggression between rival adjacent gangs attempting to occupy 22 Boys territory" (p. 157). The Latin Kings' prison leadership simply appointed a new leader. Unlike the Latin Kings, the 22 Boys gang had no way to replace their charismatic leader.

Conclusion: The arrest of the two gang leaders had no impact on *long-term* rates of violent crime, but in the area occupied by the less 'corporate' gang, it created a one month spike in violent crime related to the establishment of control over the leaderless gang's former territory. There was no effect on violent crime of the arrest of the more 'corporate' gang leader. "Law enforcement may play an important role in the social structure of gang violence through interventions targeting gang leadership that may trigger episodes of retaliatory violence by disrupting a neighbourhood's social order" (p. 162). Or, the arrest of the gang leader can have no effect. It depends on the structure of the gang.

Reference: Vargas, Robert (2014). Criminal Group Embeddedness and the Adverse Effects of Arresting a Gang's Leader: A Comparative Case Study. *Criminology*, 52(2) 143-168.

Unproductive police stops of ordinary citizens leads to political alienation, distrust and, more generally, civic disengagement for people living in areas targeted by the police.

In some cities, the police have stepped up their stop-and-frisk activities. New York City, for example, increased the number of such stops from about 90,000 in 2002 to 700,000 in 2011; nonetheless, the proportion of stops leading to arrest was essentially unchanged at about 5-6%. One problem with large numbers of police stops is that these can be seen as a form of public shaming of those stopped, suggesting disrespect or unneeded harassment by the police.

Previous research has found that “police-initiated contacts are strongly and negatively related to the probability that an individual will later call the police in times of need” (p. 205-6). More generally, it seems that contact with the criminal justice system reduces civic engagement (*Criminological Highlights* 14(4)#1). It appears, then, that police interactions with citizens “shape how communities interact with the state more broadly” (p. 206). Police stops can range from those in which citizens only identify themselves to stops that lead to arrest, summons, etc. In this paper, stops were labeled “surplus” if they involved a frisk, search, or use of force but did not result in an arrest, summons, or finding of contraband. From the perspective of the target of the stop as well as those who observed or heard about it, the stop was likely to be seen as gratuitous.

In this paper, the researchers used as an indicator of civic engagement the rate of citizen calls to their municipal governments regarding their needs, concerns, and demands of the local government. These calls may involve broken streetlights, graffiti, complaints about city services, or other concerns.

Calls to local government can be seen as “a way of connecting citizens to their municipal government... and fostering citizens’ confidence in the public sector” (p. 207). “By voicing their complaints, however mundane, citizens interact with government at the local level and obtain a response” (p. 208). Alternatively, people in communities who do not use this service may have disengaged from and lost trust in their civic communities. In other words, they would see their city as not having an interest in their concerns.

For this study, New York City was divided into small neighbourhood groupings (or blocks) involving about 1000 residents each. Data – on 311 calls to municipal governments and police stops – were examined during a 24 month period. In New York City, there are on average about 224 calls per 100 residents per year. The “stop rate”, the proportion of stops that were ‘surplus’, and the number of 311 calls all varied considerably across blocks.

After controlling for measures of disadvantage in the neighbourhood, high rates of ‘surplus’ stops were associated with lower rates of 311 calls (overall, and calls concerning crime and safety). In another analysis, adjacent neighbourhoods that were in different police precincts and had very different rates of surplus stops were compared. Again, those living in areas with high rates of surplus stops were less likely to make 311 calls.

Conclusion: The results demonstrate that “when police search a higher number of citizens or deploy more force in their stops of community members, people become much less likely to make claims on local government” (p. 217). The results were, however, different for the density of stops that did not involve ‘surplus’ force, suggesting that it is the “quality of policing, not merely the quantity” (p. 217) that makes the difference.

Reference: Lerman, Amy E. and Vesla Weaver (2014). Staying out of Sight? Concentrated Policing and Local Political Action. *ANNALS of the American Academy of Political and Social Science*, 651, 202-219.

Focusing police patrols on high crime areas can reduce the incidence of some types of crimes if the police do more than merely increase the frequency of their patrols.

Recent research has suggested that high density police patrols targeting high crime areas (hot spots) can reduce crime, at least temporarily (*Criminological Highlights* 12(3)#3, 13(3)#2). However, little is known about what kinds of activities by police are necessary to have any impact.

This paper reports the results of an experiment in which small geographic areas (an average of 8 one-block segments with an average of 128 residents per area) in St. Louis, Missouri, were randomly assigned to one of three conditions. For the 'control' areas, policing was carried out in the manner in which it normally had been. In the 'high density only' areas, police spent a disproportionate amount of their time in these areas, but did nothing unusual while there. In the 'enhanced high density' patrol areas, police not only spent a disproportionate amount of time in the area, but also engaged in self-initiated activities while there. These activities included arrests, pedestrian checks, building checks, occupied and unoccupied vehicle checks, foot patrols, and problem solving. The special patrols took place over a 9 month period daily between 3 p.m. and 7 a.m. The goal was to reduce certain firearms violence (non-domestic firearms assaults and firearms robberies).

The importance of having a control group was demonstrated by the fact that there was a substantial reduction in firearms crime in the control areas (in which policing style and patrol density

had not changed) during the 9-month period in which the study was carried out. In the 'high density only' patrol areas, the reduction in firearms violence was *not* significantly different from the reduction that took place in the control areas. However, in the 'enhanced high density' patrol areas, there was a significantly larger reduction in firearms violence than in the control areas. This was a result, completely, of the effect of the 'enhanced high density' patrols on non-domestic firearms assaults. For reasons that are not at all clear, there was no effect of increased police patrols (enhanced or not) on firearms robberies.

The effectiveness of the 'enhanced high density' patrols appears to be linked to two self-initiated activities by the police: arrest and checks on occupied vehicles. The other police-initiated activities appeared to be unrelated to drops in non-domestic firearms assaults. There did not appear to be displacement of crime into adjacent areas, or other time periods, or to other (e.g., non-firearms) offences. In other words, these police activities did not 'push' the crime to other times or locations.

Conclusion: The study demonstrates that high density police patrols, combined with certain police-initiated activities, can reduce certain firearms crimes. The fact that the reduction in crime was limited to firearms assaults and not firearms robberies is puzzling. "Certainty of arrests and occupied vehicle checks (but none of the other enforcement activities [that were] examined, were associated with reductions in nondomestic firearms assaults" (p. 446) during the period when the high intensity patrols were taking place. The study also underlines the importance of having a control group. Because there was a control group, it was possible to see that the size of the crime reduction in high intensity patrol areas that did not have enhanced police-initiated activities was not significantly greater than the reduction that occurred 'naturally' in the control areas. Crime rates are not consistent over time making 'no treatment' control groups crucial if one wants to determine whether an innovation has an impact.

Reference: Rosenfeld, Richard, Michael J. Deckard, and Emily Blackburn (2014). The Effects of Directed Patrol and Self-Initiated Enforcement on Firearm Violence: A Randomized Controlled Study of Hot Spot Policing. *Criminology*, 52(3), 428-449.

The police practice of “Stop, question, and frisk” appears to be an ineffective way to reduce street crime.

Stop, question, and frisk (SQF) approaches to policing urban areas have often been criticized because they target innocent people and are sometimes used in a racially biased fashion. In New York City, the documented number of police stops increased dramatically in the first decade of this century. In 2010, there were about 26 stops of Black people per 100 Black residents compared to about 3 stops of White people per 100 White residents. Because crime dropped between 2000 and 2010, it is sometimes suggested that SQF approaches were responsible for this decrease. Between 2003 and 2010 about 6.6% of stops in New York City resulted in arrest.

This study examines the impact of police stops on rates of robbery and burglary in 75 New York City precincts between 2003 and 2010. The annual rate of police stops varied from 33 to 4,381 stops per 10,000 people in the general population. Various controls were used in multivariate analyses including neighbourhood disadvantage, neighbourhood stability, the percent Black in the neighbourhood, as well as the overall trend in crime rates. In addition, the analyses were carried out examining the impact of police stops on crime in the current year as well as the impact of stops in each of the two previous years. The research question was straightforward: Do SQF activities in a given year reduce crime in that year and/or the two following years?

There was a small, but somewhat inconsistent effect of police stops on robbery rates in the precinct. Depending on the specific analysis, larger numbers of police stops in the current year or in the year before were associated with

a decrease *or an increase* in robbery rates. The results for burglary suggest that police stops were not associated with a reduction in this form of crime. These same analyses were repeated to determine if there was a consistent effect of SQF *arrests* (the *percent* of SQF events leading to arrest and the SQF arrest *rate*). There were no effects. When the effect of misdemeanor arrests were examined, it was again found that there were no consistent effects on the robbery or burglary rates when full controls were included (a finding similar to previous research: see *Criminological Highlights* 8(4)#1, 8(5)#8).

Conclusion: The results “show few significant effects of several ‘stop, question and frisk’ (SQF) measures on precinct robbery and burglary rates” (p. 116) and those results that are significant do not hold across crimes or type of analyses. A cautious conclusion might be that one “cannot conclude from the current investigation that SQF has no

impact on crime in New York. But we can be more certain that, if there is an impact, it is so localized and dissipates so rapidly that it fails to register in annual precinct crime rates, much less the decade-long citywide crime reductions that public officials have attributed to the policy. If SQF is effective, but its effects are highly focused and fleeting, policy-makers must decide whether expansions in a policy that already produces nearly 700,000 police stops a year are warranted, especially given the ongoing controversy regarding the disproportionate impact of SQF on racial and ethnic minorities and the possibility that it reduces police legitimacy, which may erode its crime-reduction effects over the long term” (p. 117-118).

Reference: Rosenfeld, Richard and Robert Fornango (2012). The Impact of Police Stops on Precinct Robbery and Burglary Rates in New York City, 2003-2010. *Justice Quarterly*, 37(1), 96-122.

Race and crime are intimately linked in the US: White and Black Americans explain crime in different ways, favour different policies, and experience crime in quite different ways.

Explanations of the popularity of punitive crime policies in the US without taking into account issues related to race are almost certain to be inadequate. Black and White Americans view crime differently. Understanding the differences in the perceptions of these two groups is important if one wants to understand levels of support for various policies related to crime.

White Americans are more likely than Black Americans to believe that “people commit crime because they don’t care about the rights of others or their responsibilities to society” (p. 31). Black Americans are more likely to explain crime by reference to social or structural factors (e.g., disadvantage). Not surprisingly, therefore, Whites are more likely than Blacks to support capital punishment, three strikes laws, and trying juveniles as adults. More generally, Whites are more likely than Blacks to believe that sentences are not harsh enough.

Blacks, on the other hand, are more likely than Whites to be victims of crime. For example, they are considerably more likely than Whites to experience household burglary and motor vehicle theft, sexual and non-sexual assaults and robberies. The rate of homicide victimization for Blacks is 6.2 times higher than the rate for Whites, a difference that has existed for more than 30 years. Not surprisingly, therefore, people of colour are considerably more likely to report that they avoid certain areas in their neighbourhoods because of crime and that they would be afraid if they did have to venture into these areas.

There are data suggesting that the decisions by the police about which crimes to target disadvantage Black Americans. For example, one study found that Blacks, compared to Whites, were 1.3 times more likely to report using marijuana in the month prior to being interviewed, but were 3.7 times more likely to have been arrested for marijuana possession. Not surprisingly, over the past 20 years, Blacks are considerably more likely than Whites to believe that the American justice system is biased against Black people. This difference does *not* disappear when social class is controlled for. “In fact, highly educated Blacks and Whites are more sceptical of the criminal justice system than their less-educated counterparts” (p. 33). Even simple ‘factual’ information about crime can have surprising effects. One study showed that people who were given information about the over-representation of Blacks among those who are executed were *more* likely to support the death penalty for murder than those who did not receive this information.

Although White Americans are much more likely to suggest that the best way to reduce crime is to invest in police and prisons (10% of Whites endorsed this

view compared to only 1% of Blacks), large numbers of both Whites and Blacks did suggest that investment in education and job training would be the most effective way (Whites: 35%; Blacks 58%). More Whites than Blacks (45% vs. 35%) thought that equal investments should be made for both approaches. A small number of each group did not like either approach.

Conclusion: Blacks’ and Whites’ experiences with victimization and the criminal justice system are different. Their perceptions of the system reflect, to some extent, these differences. Given that Whites and Blacks have different implicit ‘theories’ of the causes of crime, it is not surprising that they come to different conclusions about how best to deal with crime.

Reference: Ghandnoosh, Nazgol (2014). *Race and Punishment: Racial Perceptions of Crime and Support for Punitive Policies*. Washington, D.C.: The Sentencing Project.

Public safety is not compromised by retroactively shortening sentences.

From time to time, people are released from prison sooner than was originally expected. In France, for example, groups of prisoners are released from time to time before their normal release dates (often on a national holiday prisoners are released simply to keep prison numbers in check). On occasion, prisoners also have been given the opportunity to petition for sentence reductions. For example, when the United States Sentencing Commission amended the guideline for offences involving crack cocaine in 2007, the Commission allowed judges to hear motions from existing prisoners, who had already been sentenced, for a retroactive reduction in the sentences they were serving.

This study examines a simple question: Were those who received an unexpected benefit – early, and unexpected, release from prison – more likely to reoffend than those who served their full sentence. Said differently, did the decision to allow these prisoners to return to the community earlier than expected put the community at risk? Within about 40 months of the decision allowing the courts to give existing prisoners the benefit of the reduced guideline sentence, 25,736 prisoners had applied to have their sentences shortened. Most of these applications (64%) were granted. Most of those prisoners whose motions for a sentence reduction were denied were not legally eligible for a sentence reduction.

Those whose motions for a reduced sentence were successful had their sentences reduced by an average of 30 months (from an average of 12 years, 9 months to 10 years 3 months). Federal prisoners normally serve 85% of their sentences. A sample of 836 prisoners released in 2008 after serving sentences related to crack cocaine offences were matched with 483 similar crack cocaine prisoners who served their full sentences but were released in the 12 months immediately before the change came into effect. Both groups were largely male (91%) and Black (87% and 86%

for the ‘early’ and ‘regular’ release groups, respectively) and similar in age (36.3 years and 35.4 years). They had similar criminal history scores and similar proportions had been sentenced below the guideline (31%, 33%) on a motion by the prosecutor. Most had been sentenced during a time when the ‘guidelines’ were mandatory.

The recidivism rates (defined as a re-arrest or revocation of supervised release) were similar for the two groups at all points in time up to the end of the 5-year follow-up period. At the end of 2 years, the recidivism rate for the ‘early release’ group was 30.4%; this was not significantly different from the rate for the comparison group (32.6%). Those with longer criminal histories tended to have higher recidivism rates, but there were no significant differences between the ‘early release’ and ‘full sentence’ prisoners at any level of criminal history. Five years after release, 43.3% of the early release prisoners had been re-arrested or revoked. This was not significantly different from the comparison group figure (47.8%). Looking only at re-arrest, the groups were similar (33.9% and 37.3% for the early release and full release groups, respectively).

Though the two groups were similar on all measured dimensions, it is possible that the groups were different on other characteristics. In addition, it is possible that, for other reasons, social conditions in the community were different for those released between July and November 2008 than for the comparison group (released March 2007-February 2008). This seems implausible, however, when one considers that for most of the 5-year follow-up, the “at risk” periods overlapped.

Conclusion: It would appear that an unexpected reduction of over 2 years in prison for these cocaine offenders did not encourage them to re-offend. Various analyses suggest that the re-offending rates for various subgroups of these offenders did not differ significantly from the re-offending rates for those who served the sentences they expected when they were first sentenced. For these drug offenders, then, unexpected early release did not lead to changes in offending rates.

Reference: Hunt, Kim Steven and Andrew Peterson (2014) Recidivism Among Offenders Receiving Retroactive Sentence Reductions: The 2007 Crack Cocaine Amendment. Washington, D.C.: United States Sentencing Commission. (Related documents were also consulted.)

For people with serious criminal records, getting a job is more likely to be a sign that they have stopped offending than it is to be a cause of their decision to stop offending.

The link between unemployment and offending has received a good deal of attention. The idea that ‘getting a job’ leads some people not to offend has received some support (see *Criminological Highlights* 3(4)#2, 4(3)#6, 6(4)#5, 8(6)#4, 10(2)#3, 13(3)#5, 14(2)#7). The alternative hypothesis – that being employed or successfully completing job programs may signal an end to offending and is not, therefore, a ‘cause’ of the cessation of offending – has also received some support (*Criminological Highlights* 12(4)#8). It may be that those looking for change in their lives may stop offending first and eventually get a job. Alternatively, a job may be a ‘hook for change’ which, for those who have decided to change, “has the potential to sustain and reinforce the emerging process of desistance [from crime]” (p. 264).

This paper examines the employment careers and reoffending rates of 783 Norwegian serious male offenders. Each had committed at least five felonies between 1992 and 2000, at least one of which occurred after 1998. In addition, each was unemployed or had only a marginal employment history between 1998 and 2000. Between 2001 and 2007 everyone in this sample got jobs lasting at least 6 months. A comparison group was constructed of offenders who were similar up until 2000, but who did not enter into stable employment in 2001 or thereafter.

Data on employment (and income) were obtained from the national employment registry and from the national tax authorities. Offending was defined as having committed at least one felony that resulted in a criminal justice sanction in a given month. Using these data, offending for individuals could be measured for up to 36 months before and after the transition to employment. In addition, offending rates could be compared to offending rates in the group that did not enter stable employment during this period.

In the three year period before the men got stable employment, offending rates decreased quite dramatically up to the point when the men got stable employment. For those whose employment continued for at least 36 months, this relatively low offending rate was maintained. The fact that offending rates did *not* decrease *after* the men got jobs suggests that employment did not cause the decrease in offending. The comparison group – men who never achieved stable employment – maintained a high and fairly constant rate of offending throughout the period. These data would suggest, then, that decreased offending *followed* decisions made by these men to change their lives or changes which occurred to them *prior* to getting jobs.

For those who lost their job after having one for at least 6 months, there was a significant ‘rebound’ in offending. It is difficult to know whether this increase in offending occurred because the men’s offending caused them to lose their jobs, or because those who lost their jobs were different in some other way from those who maintained their jobs.

Conclusion: It appears that for serious offenders, a period of criminal *inactivity precedes* getting stable employment – a decrease that does not occur for comparable people who do not, subsequently, find employment. Hence it appears that “a significant reorientation in life priorities is a necessary precondition for voluntary job entry among men with an extensive history of criminal offending.... [The] results contradict the turning point hypothesis and [are consistent with] the maturation perspective and the hook-for-change hypothesis, both of which assume individual change prior to job entry” (p. 286).

Reference: Skardhamar, Torbjørn and Jukka Savolainen (2014). Changes in Criminal Offending Around the Time of Job Entry: A Study of Employment and Desistance. *Criminology*, 52, 263-291.

Residence restrictions placed on convicted sex offenders are shown, once again, to be ineffective.

“Changes in the philosophies of the criminal justice system have virtually separated the sexual offender from every other type of criminal” (p. 155). Even though they have recidivism rates as low as or lower than most other offenders, many jurisdictions have special laws ostensibly designed to protect society from sex offenders. Many of these – residence restrictions, registries, notification systems, etc. – seem to be designed for a small subset of all sex offenders – predatory strangers. Existing data suggest that these approaches do nothing to protect society (see the *Criminological Highlights* collection on sex offenders at <http://criminology.utoronto.ca/criminological-highlights>).

This study was designed to examine the efficacy of residential restrictions in two US states, Missouri and Michigan, that in 2006 prohibited convicted sex offenders from living, working, or loitering near schools, playgrounds, etc. By tracking sex offenders released from prison before and after the residency restrictions became law, it was possible to evaluate whether the restrictions changed where sex offenders lived and whether they changed sex offenders' re-offending rates.

Before-after changes in residence locations and re-offending rates were examined not only for sex offenders in these states but also for non-sex offenders in order to determine whether any changes that might be discovered could be explained by broader changes taking place in these two states.

The enforcement of residence requirements is not straightforward since determining the locations of all facilities that trigger restrictions may not be easy (e.g., daycares may change locations). Furthermore, some sex offenders' registered addresses may have been 'close' to a restricted location, but an exception may have applied (e.g., living in transitional housing). Nevertheless, the comparison of the residential

addresses of 1200 released sex offenders with restrictions on where they could live with the residential locations of 2125 sex offenders released before the imposition of residential restrictions showed no significant difference in the proportion of residents who lived close enough to one of a location like a school that was prohibited by the new law. In Michigan 22% of the released sex offenders were found to be living in areas in which they were apparently prohibited to live; in Missouri the comparable figure was 21%.

Residential restrictions also did not appear to reduce reoffending by the sex offenders. In Michigan, after controlling for various known predictors of recidivism (e.g., age, prior convictions), there were comparable sized *increases* in reconvictions for both sexual offenders and non-sexual offenders. In Missouri, there were significant decreases in the rate of technical violations of conditions of release for *both* sex offenders and non-sex offenders, but no changes in the reconviction rates. However, the sex offenders who had restrictions placed on them appeared to be crime-free for a somewhat longer period of time even though their *overall* rate of reconviction was the same.

Conclusion: It would be difficult to conclude that sex offender residence restrictions are effective since they seem to have little impact on where people actually live. Furthermore, the impact of these restrictions on recidivism varies across measures (rate vs. timing of recidivism) as well as across the two states (Michigan vs. Missouri). These 2-year recidivism rates used overall reoffending (sexual offences and non-sex offences) as the outcome measure for a simple reason: there were very few sexual offences by these offenders after release. The restrictions did not appear to affect this already low rate. Another reason that one would not expect residence restrictions to affect reoffending is that residence restrictions relate to the presence of children and most of the sex offences in these two states (77% to 87%) did not involve children.

Reference: Huebner, Beth M. and five others (2014). The Effect and Implications of Sex Offender Residence Restrictions: Evidence from a Two-State Evaluation. *Criminology & Public Policy*, 13, 139-168.