Do returning parolees affect neighborhood crime?

A case study of Sacramento

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Abstract

This study utilized a unique dataset that combines information on parolees in the city of Sacramento, CA over the 2003-06 time-period with information on monthly crime rates in Sacramento census tracts over this same period, providing us a fine-grained temporal and geographical view of the relationship between the change in parolees in a census tract and the change in the crime rate. We find that an increase in the number of tract parolees in a month results in an increase in the crime rate. We find that more violent parolees have a particularly strong effect on murder and burglary rates. We find that the social capital of the neighborhood can moderate the effect of parolees on crime rates: neighborhoods with greater residential stability dampen the effect of parolees on robbery rates, whereas neighborhoods with greater numbers of voluntary organizations dampen the effect of parolees on burglary and aggravated assault rates. Furthermore, this protective effect of voluntary organizations appears strongest for those organizations that provide services for youth. We show that the effect of single parent households in a neighborhood is moderated by the return of parolees, suggesting that these re-united families may increase the social control ability of the neighborhood.

Keywords: parolees, recidivism, neighborhoods, neighborhood crime, social capital, social control.

Bios

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Over the last 25 years, imprisonment rates in the U.S. have increased dramatically. The number in U.S. prisons has increased from 330,000 in 1980 to over 1.5 million in 2005, a 450 percent increase (Harrison and Beck, 2006; Lynch and Sabol, 2001). One important implication of this mass incarceration is its effect on the communities and neighborhoods these prisoners have left behind. Scholars have suggested that this imprisonment may affect the crime rates in the neighborhoods that these prisoners leave behind, either in a positive, or a negative, fashion (Gottfredson and Taylor, 1988; La Vigne and Parthasarathy, 2005; Levitt, 1996; Raphael and Stoll, 2004; Visher, Kachnowski, La Vigne, and Travis, 2004). More recently, scholars are beginning to realize that another important implication of this mass incarceration is that at some point many of these prisoners will return to communities. As a dramatic example of this impact, the number of parolees annually returning to U.S. neighborhoods has increased from 170,000 in 1980 to about 700,000 in 2005 (Lynch and Sabol, 2001; Sabol and Harrison, 2007). As a consequence, the number of ex-offenders residing in communities has risen from 1.8 million in 1980 to 4.3 million in 2000 (Raphael and Stoll, 2004). Understanding the possible effect these returning parolees will have on neighborhoods is therefore no less important than understanding the effect their incarceration has on neighborhoods, but is nonetheless even less well understood.

One perspective argues that returning parolees may increase crime in the neighborhoods to which they return (Raphael and Stoll, 2004). This assumes that parolees generally are more crime prone than the general public and that their increasing presence in a neighborhood will therefore increase the crime rate. Thus, neighborhoods with an increasing number of parolees—particularly those at high risk to recidivate—will have increasing crime rates. Indeed, a study using a panel dataset aggregated at the level of U.S. states estimated that a net increase in the number of former inmates living in a state accounts for approximately 2.5 percent of violent crimes and 2 percent of property crimes (Raphael and Stoll, 2004). A study using a large survey of several states between 1994 and 2001 estimated that parolees accounted for 15 to 28% of all *arrests* for violent crimes and 10 to 18% of all arrests for property crimes (Rosenfeld, Wallman, and Fornango, 2005). Rose and Clear (1998) also hypothesized that returning parolees will increase crime rates, although they posit that the causal mechanism is the residential instability fostered by this forced incarceration.

Aside from the question of the effect parolees may have on neighborhoods is the effect neighborhoods may have on parolees. That is, some neighborhoods may be better able to handle this influx of parolees than others. Building on the social capital literature (Beyerlein and Hipp, 2005; Messner, Baumer, and Rosenfeld, 2004; Rosenfeld, Messner, and Baumer, 2001), there is reason to suspect that neighborhoods with greater cohesion are better equipped to accommodate a significant influx of parolees into the neighborhood. For instance, parolees face the difficult task of overcoming the negative stigma associated with being convicted of a crime (Petersilia, 2003). Moreover, these convictions are a blemish on their personal record which makes it extremely difficult to secure legitimate employment and other opportunities that can decrease the chances of re-offending. As a consequence, even if parolees in general increase the crime rate in a neighborhood, a neighborhood with a significant number of voluntary organizations providing services both to these parolees as well as all residents of the community may help a neighborhood adapt to any challenges and difficulties that may accompany the reintegration of parolees into the neighborhood. Likewise, a neighborhood with more informal ties and cohesion among residents may be better able to accommodate an influx of parolees, and thus experience little impact on its crime rate.

Do returning parolees affect neighborhood crime?

Despite the importance of these questions given the large increase in incarceration of the last 20 years, answers based on empirical evidence are lacking. We address this void by constructing and analyzing a unique data set to test the effect of parolees on neighborhood crime rates in the city of Sacramento, CA. Although there are always questions of generalizability for a study of a single city, this city has a fairly mixed racial/ethnic composition: about 60% white, 16% Latino, 11% Asian, 9% African-American, and 5% other race. The median income of the city's tracts was about $46,000, with 14% in poverty, compared to about $44,000 and 13% for all U.S. tracts, suggesting that it may well be a fairly representative city. Because returning parolees may have a fairly rapid effect on neighborhood crime rates, fairly fine-grained temporal data is necessary for addressing these questions and we use monthly data to tease out this effect. Given the close relationship between parolees and crime rates--given that more crime in a community frequently leads to more arrests and imprisonments, and these persons are eventually paroled to the same or similar communities—cross-sectional studies will almost certainly find a positive relationship, but are unable to shed light on any possible causal effects (Gottfredson and Taylor, 1988). In addition, although returning parolees may have a direct positive effect on crime rates, we are able to test whether they also have an indirect negative effect by reuniting families. We also test whether neighborhoods with higher levels of informal and formal social control (measured by voluntary organizations and residential stability) are better able to accommodate and integrate parolees. Finally, we test whether the *types* of voluntary organizations in a neighborhood differentially moderate the parolee-crime relationship.

Parolees, Neighborhoods, and Crime

Broadly speaking, the impact of parolees on neighborhoods can be conceived in three fashions: 1) a direct effect through the possible criminal activity of parolees, 2) an indirect effect in which the presence of parolees in a neighborhood causes others to commit more crime through the rekindling of ties with fellow co-offenders and imparting criminal wisdom learned in jail, 3) an indirect effect in which parolees affect the neighborhood's ability to provide social control, either in a positive or a negative fashion. We consider each of these possibilities in turn.

Direct effect of parolees on crime

The first view takes an individual perspective in suggesting that parolees in general are more likely to commit crime, and thus their greater presence in a neighborhood will have a direct effect on crime rates. In the framework of the routine activities theory, parolees entering a neighborhood increase the pool of motivated offenders, and all else equal, will increase the level of crime. This suggests that the relative proportion of parolees in a neighborhood will be important for determining the size of this effect: for instance, releasing a single parolee into a neighborhood will likely have little effect on the crime rate, regardless how much crime this person commits. However, an increasing number of parolees will have a substantial effect on the crime rate. Since each additional parolee would increase the number of potential offenders, this would suggest a linear relationship between parolees per capita in a neighborhood and the crime rate. This assumes either that all parolees are identical in their likelihood of committing offenses, or, as we assume, that there is variability in parolees' likelihood of committing offenses and that a released parolee is randomly selected from this distribution (given that release from prison is often based on predetermined sentences).[1]

Following this logic, the individual characteristics that increase the likelihood of any given individual recidivating should help explain the effect such parolees will have on neighborhood crime rates. This suggests the ability to utilize the insights of a long line of recidivism literature in determining which sorts of parolees are most likely to return to criminal activity. Of course, there is an important caveat in this logic: the recidivism literature has focused on the likelihood of parolees *arrested* for criminal activity. But what we are interested here is the tendency of such parolees to *engage in* criminal activity. The difference is important. Some characteristics that the recidivism literature has identified as important predictors of recidivating may in fact indicate individuals who simply are not very good at evading detection of their criminal activity.

One example of a characteristic that has shown a strong effect on recidivism is the number of prior arrests ("churners") (Beck and Shipley, 1989; Blumstein and Beck, 2005; Langan and Levin, 2002). On the one hand, measurement of churners may simply be capturing a greater likelihood to be detected when committing crime, whether because of a lack of skill in performing the acts, a lack of intelligence in planning to maintain anonymity while committing the act, or various other reasons. Another possibility is that these churners are more likely to commit crimes when released into the community. If the latter is the case, releasing more "churner" parolees into a neighborhood will lead to higher crime rates.

Another characteristic that has shown a consistent relationship with recidivism, but whose theoretical meaning is uncertain, is the race/ethnicity of the parolee (Beck and Shipley, 1989; Gendreau, Little, and Goggin, 1996; Kubrin and Stewart, 2006; Langan and Levin, 2002). One possibility comes from the literature suggesting that it is not unheard of for law enforcement personnel to practice selective enforcement and target minority groups, thereby, increasing the level of minority involvement in arrests (Fagan and Davies, 2002; Garrison, 1997; Meehan and Ponder, 2002). This would suggest that minorities may be no more likely to *commit* crimes, but that they will be arrested more frequently. On the other hand, the evidence from victimization surveys that African-Americans have much higher crime rates than other groups (Uniform Crime Reporting Program, 2003), along with the evidence from the recidivism literature of African-Americans' greater likely to recidivate suggests that racial/ethnic minorities for some reason may have a greater proclivity for committing crimes. This could be because these minority parolees are returning to disadvantaged neighborhoods that increase the likelihood of committing crimes, rather than anything about them individually (Lauritsen and White, 2001), though one study found that even when accounting for the economic disadvantage of the neighborhood, minorities were more likely to recidivate (Kubrin, Squires, and Stewart, 2007).

The evidence that conviction for certain types of crimes is a strong predictor of recidivism may provide insight for our question here. In particular, studies have shown that individuals who were arrested for more serious crimes, or more violent crimes, constitute a larger proportion of arrests for such crimes after release than they do for less serious crimes: for instance, parolees constitute a larger proportion of robbery, murder, and burglary arrests than they do of larceny arrests (Beck and Shipley, 1989; Langan and Levin, 2002). Given the prior research on criminal careers (Blumstein and Beck, 2005; Piquero, Farrington, and Blumstein, 2003; Sullivan, 1989) and how individuals frequently "graduate" from minor types of crime into more serious and violent crimes, parolees with a longer record of serious and violent crimes are likely more "hardened" criminals who are more likely to commit crimes in the future. As well, each new conviction and the seriousness of the offense may affect the composition of one's personal networks and signify a level of disrespect for the conventional norms and values that guide non-criminal behavior (Haynie, 2002; Sutherland, 1947). This repeat offending will not only sever ties with community members, but may also impede other types of social bonds responsible for helping individuals desist from crime (Petersilia, 2003; Rose and Clear, 1998).

Whereas these considerations suggest that taking into account the types of parolees entering neighborhoods should allow us to understand their direct effect on the amount of crime, there is very little evidence that the presence of more parolees in a neighborhood—and the characteristics of those parolees—will have such a direct effect on the crime rate. The few studies testing this have focused on large units of analysis. For instance, one study looked at the effect of state-level parolees on the change in homicides and failed to detect a significant effect (Kovandzic, Marvell, Vieraitis, and Moody, 2004). However, it is unclear that the effects of parolees can be detected at such a large aggregate unit. In addition, the study had little statistical power to detect such effects, leaving open the possibility of Type II error.[2] In contrast, another study using state level data found a significant positive relationship between the net number of former inmates living in a state and the property and violent crime rates (Raphael and Stoll, 2004). Nonetheless, the focus on a large unit of analysis in both of these studies arguably makes the connection between the presence of parolees and crime rates somewhat tenuous. Furthermore, using such broad time frames as annual data makes it difficult in such an approach to establish a causal relationship between the presence of such parolees and crime rates. Despite these limitations, other researchers have postulated that parolees reflect a large proportion of arrests for both violent and property crime which implies that they might affect neighborhood crime rates (Rosenfeld, Wallman, and Fornango, 2005).

Indirect effect of parolees on crime through criminal networks

A second possibility is that parolees returning to neighborhoods affect crime rates indirectly by reactivating ties with fellow motivated offenders. This is not necessarily in competition with the direct effect perspective highlighted above. That is, parolees may not only engage in criminal acts themselves, but they may also increase criminal activity through activity with fellow contacts. Relatedly, they might also increase criminal activity by imparting the criminal knowledge they learned while in prison. This suggests a sort of “multiplier” effect in which such parolees foster a greater amount of crime beyond what they commit themselves. The differential association/social learning theory suggests that the criminal behavior of these parolees emboldens their colleagues to behave similarly through this modeling tendency. These particular values and norms favoring criminal behavior may well be enhanced by the return of these parolees to neighborhoods, fostering a greater amount of criminal behavior on the part of all members of these groups. Again, assuming no systematic pattern in the offending level of released parolees, this may suggest a nonlinear effect: the effect of a few parolees on neighborhood crime rates is quite strong as their reactivation of network ties has a ripple effect through the overall neighborhood network (as this multiplier effect kicks in).

Another perspective suggests that increased incarceration and the resulting release of more prisoners might increase crime rates if such releases are somehow perceived to reduce the deterrence effect of prisons. Clear (2007) suggests that as punishment becomes more common, the ability to deter criminal behavior diminishes. On a larger scale, other neighborhood residents may perceive the influx of returning prisoners as an indication of a decrease in the severity of punishment, thereby circumventing the function of prisons as a general deterrent for crime. Going to prison has become so commonplace in some areas that imprisonment may actually provide a level of status in certain neighborhoods (Petersilia, 2003). Therefore, the increased prestige gained upon release may be alluring to would be potential offenders.

Indirect effect of parolees on crime by affecting social control

Up to this point we have considered the possible effect of parolees on neighborhood crime through their positive impact on crime events. An additional particularly important way that returning parolees may affect the neighborhood composition is through their effect on the presence of broken households. This builds on the insight provided by Rose and Clear (1998), who argued that an unintended consequence of the large wave of incarceration in the U.S. is the break-up of households and may thus reduce the neighborhood's ability to provide social control. The logic is straightforward: the lack of fathers in a neighborhood implies lower levels of private social control, since children with a single parent do not face the level of control experienced by those in two-parent families.[3] This lack of oversight makes it easier to engage in delinquent acts, thus fostering a career in crime (Sampson and Laub, 1990). The lack of two-parent households also suggests a diminished level of parochial control in the neighborhood since there are fewer parents in general to watch over the activities of neighborhood children. Indeed, studies have frequently shown that the presence of single parent households is related to higher levels of neighborhood crime (Hipp, 2007b; Ouimet, 2000; Roncek, 1981; Rountree and Warner, 1999; Smith, Frazee, and Davison, 2000). Thus, as more fathers in a neighborhood are imprisoned, the level of private and parochial social control will decline, and youth will engage in more criminal activity. Indeed, a substantial number of prisoners are parents: a 1999 study found that 55 percent of prisoners in state institutions had at least one child under the age of 18, and that 44 percent of these fathers had lived with their children before incarceration (Mumola, 2000). This implies that about 25 percent of male prisoners were separated from families with children less than 18 years of age.

Whereas Rose and Clear (1998) suggested that imprisonment might increase crime by reducing a source of informal social control in neighborhoods, a later extension by Clear, Rose and Ryder (2001) argued that the return of these prisoners to neighborhoods as parolees would further increase crime by exacerbating the residential instability of the neighborhood. The logic is that the imprisonment, and then return, of these adults to the neighborhood will fracture and strain social ties that the social disorganization model posit are important for providing informal social control (Sampson and Groves, 1989). There is some empirical evidence for Rose and Clear's conjecture: non-dynamic analyses have found a positive relationship between the presence of parolees and crime rates for neighborhoods in Baltimore (Gottfredson and Taylor, 1988) and Tallahassee (Clear, Rose, Waring, and Scully, 2003).[4] However, given that higher rates of crime in a neighborhood generally lead to more imprisonments, which eventually lead to more parolees, it is not at all clear that a non-dynamic analysis can capture the effect of parolees on crime rates.

We propose a counter-argument, building on the logic that the lack of fathers in a neighborhood due to incarceration will lead to higher levels of crime: we suggest that returning parolees might actually *reduce* crime by providing more private and parochial social control. Of course, there are certainly some family situations that are arguably improved when the father is sent to prison, and such situations are likely not helped by the returning father who may well bring crime into the household. For instance, some research has explored the challenges faced by parolees in attempting to reestablish bonds with, and authority over, their children (Dyer, 2005; Travis and Waul, 2005). On balance, however, these returning fathers may reduce the level of crime by increasing the level of social control in the neighborhood. Note that Rose and Clear (1998) argued for an opposite effect: they focused on the residential instability caused by incarcerating and later releasing residents and postulated that parolees returning to the neighborhood will linearly increase the crime rate. We test whether returning parolees can indeed moderate the effect of single parent households on neighborhood crime rates.

Effect of neighborhoods on parolee integration

Although parolees may be able to affect neighborhood levels of social control, it is also quite possible that the level of social control and social capital in a neighborhood is important for determining its ability to integrate parolees without experiencing an increase in crime. Given the well-documented difficulties that parolees face in attempting to reintegrate into the community (Petersilia, 2003), it is plausible that neighborhood characteristics are important for fostering this reintegration. Parolees that are integrated into the neighborhood through employment and social networks are less likely to recidivate. This suggests that the social capital of the neighborhood may be crucial in allowing the neighborhood to reintegrate parolees without seeing a spike in crime rates.

Whereas one approach focuses on the effect of these neighborhood resources for recidivism, this does not directly address our question of interest. To reiterate, we are not trying to determine the likelihood that an individual parolee will recidivate, but rather the potential impacts parolees can have on neighborhood-level crime in general and how neighborhood characteristics can moderate this effect. That is, we can make a distinction between the actual crime events that occur (which we are most interested in, but can never absolutely measure), the crime events that authorities know about (our outcome measure of the official crime events in the neighborhood), crime events in which the perpetrator is known (which is a subset of the events of which authorities are aware), and crime events known to be committed by a parolee (which are the focus of recidivism studies). Indeed, few studies have actually accounted for neighborhood characteristics when studying recidivism. One exception was the recent study by Kubrin and Stewart (2006) that looked at the effect of neighborhood economic disadvantage on recidivism rates. Whereas this study's finding that economically disadvantaged neighborhoods increase the likelihood of recidivism is important, it still leaves unaddressed the possible role of social capital in helping parolees reintegrate with the community.

It should be highlighted that studies focusing on recidivism are only measuring individuals who are *detected* committing crimes. As Kovandic et al. (2004) point out, parolees may be committing many crimes that are not detected by the authorities, highlighting the need to employ aggregated data for testing such an effect of parolees on neighborhood *crime rates* rather than arrests. As well, to the extent that parolees can affect neighborhood crime rates not only directly by committing crimes (which would be captured to some extent through a recidivism study), but also indirectly by reactivating network links that also lead others into committing crimes, this suggests a stronger effect on neighborhood crime than just the crimes committed by parolees. For all these reasons, we suggest a better approach than using arrest data, and one that we adopt here, is to focus on how the relationship between parolees and aggregated neighborhood crime rates may differ based on the social resources in the neighborhood.

The above considerations suggest that the social resources of a neighborhood may be particularly important for integrating parolees into the neighborhood. In particular, the *formal* social capital as embodied by the voluntary organizations in a neighborhood can moderate the effect these parolees might otherwise have on neighborhood crime rates. By providing resources that help parolees reintegrate into the community, they may be able to discourage a return to criminal activity. Besides directly helping these parolees, these organizations may also provide resources that the entire neighborhood can turn to if problems develop due to other impacts of these returning parolees. These voluntary organizations constitute *formal* social capital in a neighborhood (Beyerlein and Hipp, 2005; Hipp and Perrin, 2006; Morenoff, Sampson, and Raudenbush, 2001; Paxton, 1999; Paxton, 2002; Sampson and Raudenbush, 1999).

It is also possible that the *informal* social capital in a neighborhood may help in integrating parolees into the community. Given the evidence suggesting that neighborhoods with greater residential stability will have more informal ties (Connerly and Marans, 1985; Logan and Spitze, 1994; Ross and Jang, 2000; Rountree and Warner, 1999; Sampson, 1988; Sampson, 1991; Warner and Rountree, 1997), this stability may help the neighborhood respond to the possible negative presence of parolees. In this sense, these informal ties may allow residents to convey information about concerns and produce greater collective efficacy (Sampson, Raudenbush, and Earls, 1997) that enhances the ability to respond to possible disorder fostered by parolees. In such a neighborhood, the effect of returning parolees may be moderated by this ability to provide informal social control. This may have a direct effect on parolees' criminal behavior, dissuading them from such activity. Additionally, this informal social control may dampen the positive effect on crime that would otherwise occur through the reactivated ties of these parolees; that is, the differential association effect of these ties may be moderated by this informal social control.

Data and Methodology

Data

To address these questions, we created a unique dataset that combines information on parolees in the city of Sacramento, CA over the 2003-06 time period with information on monthly crime rates in Sacramento census tracts over this same period. This allows us a fine-grained temporal view of the effect of returning parolees on neighborhood crime rates. We began our study period in 2003 since the parolee data prior to that time point is less reliable due to considerable missing information about addresses. The data on parolees were obtained from the California Department of Corrections and Rehabilitation (CDCR). These data provide information on all parolees during the time period, some characteristics of the parolees, and the effective dates of all known addresses. We geocoded the addresses and placed the parolee into the appropriate census tract based on this geocoding and the timeframe at the specific address. We then aggregated by month the number of parolees in a tract.

Outcome measures

The data on crime events in Sacramento were downloaded from the Sacramento Police Department website (http://www.sacpd.org/databases.asp). We geocoded these events based on the address provided and placed them into the appropriate census tract. We then aggregated the number of different types of crime events that occurred in a given month for each census tract. Our outcome measures are the number of crime events in a tract in a month. We aggregated monthly totals of four Type I crimes: robberies, aggravated assaults, burglaries, and murders. The first three crime types occur relatively frequently and are types that prior research suggests residents consider quite serious types of crimes (Hipp, 2007c; Liska, Lawrence, and Sanchirico, 1982; Zimring, 1997). The murder rate allows us to compare our results with prior work using this as an outcome measure.

Exogenous variables

Our key predictor is the parolees per capita in a tract in a given month. This measure is constructed by first computing the number of known parolees residing in a tract in the given month, and then dividing this by the total population as of 2000. This gives us an indication of the proportionate influence of parolees in a given tract. Our fixed effects estimation technique allows us to focus on how the change in parolees per capita affects the crime rate in a tract; this focus on change is important since our estimates of the total number of parolees are not accurate given the limited quality of address data prior to 2003. By calculating monthly values for both the outcome measure and this key predictor, we are able to estimate the short-term effect of parolees on neighborhood crime rates. Given the rapidity with which parolees may impact neighborhood crime rates either indirectly through re-establishing contacts as well as directly through recidivism, this short time lag is crucial for estimating these effects. We also point out that there is little reason to expect reciprocal effects, since neighborhood crime does not cause more neighborhood parolees in the *short term*. That is, this neighborhood crime may cause more imprisonments, but this will not lead to more parolees until a much later date.[5]

A limitation of studying parolees is that it is notoriously difficult to track where parolees are actually living. It is somewhat encouraging that one study of a sample of parolees found that 72% still lived at the same location one to two years later (La Vigne and Parthasarathy, 2005). Another issue is that a number of parolees abscond, and therefore their whereabouts is unknown.

It is notoriously difficult to estimate rates of absconding: one estimate from CDCR is that 30% of parolees are removed from parole for absconding, whereas an estimate of the number on abscond status *on any given day* was about 17% in 2005 (Petersilia, 2006). Clearly, this is a source of measurement error and likely biases downward the size of the estimated effect given that the parolee is not actually in the tract to which we attribute them.

Although we have no way of knowing whether parolees who might commit crimes after returning to the neighborhood are actually committing those crimes in their own neighborhood, an existing literature has studied the distance typically traveled to crime (Rengert, Piquero, and Jones, 1999). Most studies have suggested that offenders do not travel long distances—generally about one or two miles (Pyle, 1974). Given that the average census tract in Sacramento in 2000 was 0.91 square miles, or just under one mile across, this suggests that counting the number of parolees based on the tract where they live may provide a reasonable estimate of their effect on crime rates. To the extent that they are actually committing crimes in adjacent tracts, our approach underestimates the effect of parolees on crime rates, as other scholars have noted (Vieraitis, Kovandzic, and Marvell, 2004). We therefore directly take into account the possibility that returning parolees may affect the crime rates in nearby tracts by creating spatially lagged versions of our measure of the percentage of parolees—this follows the suggestions of Elffers (2003) and Morenoff (2003) that in certain instances it is most appropriate to model spatial effects in such a direct manner (see also Anselin, 2003: 161). We accomplished this by creating a weight matrix (W) with an inverse distance decay function with a cutoff at two miles (beyond which the neighborhoods have a value of zero in the W matrix) in measuring the distance of surrounding neighborhoods from the focal neighborhood. This resulting weight matrix (W) was then row-standardized, multiplied by the percentage of parolees in these nearby tracts, and summed to create an estimate of the number of parolees in nearby tracts for a focal tract.

Additionally, we created measures taking into account four characteristics of parolees. For three measures, we first assessed the total number of prior arrests, and the seriousness or violent nature of the prior arrests for each parolee and took this information into account when aggregating monthly totals by tract. For each parolee, we have information on the total number of prior offenses, the number of serious offenses that were not violent, and the number of violent offenses on their permanent criminal record. In the California coding scheme, all murders are classified as violent offenses, 80% of rapes are classified as violent whereas 20% are classified as serious only (not violent), 50% of assaults are classified as violent and 50% as serious only, 40% of robberies are classified as violent whereas 60% are classified as serious only, and 5% of rapes are classified as violent with the balance classified as serious only. About 60% of burglaries are classified as serious only, whereas the other 40% are not considered serious (For a complete description of these categories, see pages 44-47 in Greenwood, Rydell, Abrahamse, Caulkins, Chiesa, Model, and Klein, 1994). We thus weight each individual by the number of serious property or violent offenses on their permanent record (though capped at 10 to minimize the possibility of outliers), or the total number of prior offenses. We then summed these weighted measures for all parolees in a tract in a given month to obtain: 1) an aggregated measure of serious non-violent prior offenses of parolees in a tract in a given month, per capita; 2) an aggregated measure of violent prior offenses of the parolees in a tract in a given month, per capita; 3) the number prior offenses of the parolees in a tract in a given month, per capita. Finally, we also computed the number of African-American parolees per capita in a tract.

We accounted for the possible moderating effect of certain neighborhood characteristics with three key measures. Two measures come from the 2000 U.S. Census. First, since returning parolees may moderate the effect of broken families that reduce oversight capability, we included the percent single parent households in the tract. Second, since the informal social capital provided by neighborhoods with greater levels of residential stability may reduce the impact of parolees, we included the average length of residence of households in the tract in 2000.[6] Although it would be ideal to have measures of these constructs coterminous with our parolee data, the relative stability over time of neighborhood measures suggests that there is likely minimal effect on our estimates of this gap: for instance, the correlation is .85 between residential stability in 1990 and 2000 for all U.S. tracts, and .82 between the percent single parent households in 1990 and 2000. Furthermore, the correlation over the decade for the total voluntary organizations per capita measure (described next) is somewhat higher at .91, and it is instructive that the correlation of this measure from 2000 to 2002 is .998. This suggests considerable stability over a short period of time. Indeed, the ground-breaking Chicago Project on Human Development in Chicago Neighborhoods (PHDCN) study that has spawned a large number of important papers likewise assumed such stability in neighborhood measures over a five-year period (Morenoff, Sampson, and Raudenbush, 2001; Morenoff, 2003; Sampson, Morenoff, and Earls, 1999; Sampson and Raudenbush, 1999; Sampson, Raudenbush, and Earls, 1997).[7]

Third, since formal organizations help a neighborhood reintegrate parolees with the community, we included a measure of the number of voluntary organizations in the tract per 10,000 population in 2002 (the most recent year for which we have data). Given that we expect additional voluntary organizations will have a diminishing positive effect for a neighborhood, we natural log transformed these measures (models estimated using the untransformed measures consistently showed weaker effects). These data come from the National Center for Charitable Statistics (NCCS), and contain information on exempt organizations from the Internal Revenue

Service's Business Master File. These data are limited in that they do not include all voluntary organizations in a neighborhood, nor do they contain information on the number of members. However, prior evidence that this data source disproportionately captures larger voluntary organizations—precisely the sort most likely to provide resources to the community—suggests that they may be a reasonable proxy for the construct of interest (Gronbjerg and Paarlberg, 2002). We geocoded these organizations based on the provided address and placed them into the appropriate census tract. For organizations without a valid address (about 20 percent of the organizations), we placed them into the appropriate zip code, and then apportioned the organizations in a zip code to the tracts it overlays based on the population contained within each tract.[8] We thus created a measure of the total number of voluntary organizations per capita in a tract. We also categorized each voluntary organization based on the services they provide to the community. Up to three activity codes are provided for each organization, allowing us compute the number of voluntary organizations for a given type of service. We created measures of organizations per capita addressing the following types of issues: social services, family services, youth services, economic resources, crime issues, neighborhood disorder issues.

We list the summary statistics of the variables used in the analyses in Table 1. To get a sense of the degree of change in our key measures, we computed the absolute difference in month to month values of parolees and crime events. The average month-to-month change was 1.29 aggravated assaults, 0.78 robberies, 1.66 burglaries, 0.04 murders, 5.57 parolees, and 0.44 logged parolees per capita. To get a sense of the yearly change we computed the absolute difference in year to year values of parolees and crime events for specific months. The average year-to-year change by months was 1.34 aggravated assaults, 0.80 robberies, 1.88 burglaries, 0.04 murders, 1.48 parolees, and 0.12 logged parolees per capita.

<<<Table 1 about here>>>

Do returning parolees affect neighborhood crime?

Methodology

Our fixed effects approach uses both a fine-grained time period as well as a fine-grained geographic unit: we are essentially estimating whether the *change* in parolees per capita in a month leads to a *change* in the crime rate. Since our outcome measures are counts of crime events, we estimated fixed effects negative binomial regression models. This approach treats the outcome measure as a Poisson distribution with an additional parameter with an assumed gamma distribution to account for the overdispersion created by the nonindependence of events. This parameter captures both overdispersion created by heteroskedasticity over time, as well as that created by autocorrelation in the errors (Taibleson, 1974). We adopt the fixed effects approach advocated by Allison and Waterman (2002), since it appropriately conditions out differences across census tracts, allowing only within-tract comparisons.[9] This implies the following model:

(1) $$y = \alpha + \mathrm{P}\beta + (\mathrm{TRACT})\beta_{\mathrm{TRACT}}$$

where y is the number of crime events that month, α is an intercept, P is the parolees per capita that have β effect on the crime rate, TRACT is a matrix of K-1 indicators for the K tracts in the sample, β_{TRACT} is a vector of the effects of each of these tracts, and the tract population is used as an offset variable (logged, with a coefficient constrained to one), to transform the outcome measure into a rate. The block of tract indicators was jointly highly significant in all models, suggesting the importance of its inclusion in the models. Whereas this strategy of accounting for differences across tracts by including indicator variables results in 'incidental parameters' problem for logistic regression models, Allison and Waterman (2002) highlight that such is not the case in the negative binomial regression model.

Given the possibility of additional crime processes in these tracts over this period, our models also accounted for changes over time, seasonal effects, and changing dispersion over time.[10] We accounted for change over time and possible seasonal effects by including indicator variables for all months in the study period except one.[11] These augmentations imply the following model:

(2) $$y = \alpha + P\beta + S\beta_S + (TRACT)\beta_{TRACT}$$

where all variables are defined as before, S is a vector of the 47 indicator variables for the month of the study (with the initial month as the reference category), and β_S captures these effects on the crime rate. We allowed variability in the error variance by allowing the dispersion parameter to vary by year (Hausman, Hall, and Griliches, 1984).[12]

To account for possible moderating effects of these neighborhoods, we estimated additional models in which we included an interaction between the parolees per capita and the various key characteristics of the neighborhoods identified above. In fixed effects models, it is not possible to include the main effects of the neighborhood characteristics (since the matrix of tract indicators conditions out differences across neighborhoods), but it is appropriate to include these interactions to assess how these characteristics modify the effect of parolees on crime rates (Allison, 2005; Wooldridge, 2002: 269; Wooldridge, 2003: 444). The model estimated is thus:

(3) $$y = \alpha + P\beta + (P*X)\eta + S\beta_S + (TRACT)\beta_{TRACT}$$

where all terms are as defined above, P*X is the matrix of these interaction variables between parolees (P) and the key exogenous measures (X), which have an η effect on the outcome.[13] These models again include the tract population as an offset measure. We point out that whereas there might be endogeneity concerns in a cross-sectional model using a measure of voluntary organizations to predict crime rates (since voluntary organizations may well form *because of* the need in some more disadvantaged, crime-prone, neighborhoods) in our models here we are not estimating a main effect, but only estimating a possible moderating effect on the relationship between parolees and crime rates. Given that our measure of voluntary organizations is prior to the study time period, and the fact that voluntary organizations do not form very rapidly—

especially given our fine-grained monthly time periods—there is little reason to expect possible confounding here.

Results

Relationship between returning parolees and crime

We begin by focusing on the models testing whether the monthly change in the tract parolees per capita leads to higher levels of crime that month. We tested three different possible functional forms for this relationship between parolees per capita and crime rates by specifying a linear effect, a quadratic effect (to determine if there is an inflection point above which the direction of the effect reverses), a cubic effect (to test the s-shaped curve hypothesized for an indirect effect of parolees), and a logarithmic effect (to determine if the relationship diminishes at larger numbers of parolees per capita). The particularly strong effects for the logarithmic specification, as well as the fact that the quadratic specification showed that the rate of increase only slowed over the range of the data (consistent with the logarithmic specification), suggested that the logarithmic specification most closely approximates this relationship. The results presented in Table 2 highlight that there is indeed a positive relationship between returning parolees and neighborhood crime rates. For instance, a one standard deviation increase in the logged parolees per capita in a month increases the aggravated assault rate 8.6% (exp(.07*1.174)=1.086), the robbery rate 20.5% (exp(.159*1.174)=1.205), and the burglary rate 9.7% (exp(.079*1.174)=1.097) that same month. Whereas the murder rate also appears to increase, because of the infrequency of this type of crime we lack the statistical power to make a more confident statement regarding this effect. The size of these effects is consistent with literature using larger units of analysis: for instance, one study using states as the unit of analysis found that a 10 percent increase in prison releases led to a long-term increase of 1.76 percent for robberies, 0.95 percent for burglaries, and 0.7 percent for homicides (Vieraitis, Kovandzic, and Marvell, 2004), whereas we find comparable values in our study of 1.6%, 0.79% and 0.6%.[14]

<<<Table 2 about here>>>

In the second model presented in Table 2, we tested the spatially lagged effect of parolees and the results are suggestive of this expected positive effect for robbery (with marginal significance) and murder (though not statistically significant). The size of the effect of this spatially lagged measure is about double that of the effect in the focal tract for robberies and murder; given that the variability of this measure is about half as large as for the measure of parolees in the focal tract, this suggests that the size of the effect on these crimes may be about the same for parolees in the tract and parolees in surrounding tracts. The correlation of .54 between the parolee variable and its spatially lagged version although not excessively high, likely affects our statistical power to detect such an effect. We include the spatial lag in the robbery models in the remainder of the analyses despite its marginal significance.

We next assess whether the *types* of parolees in a neighborhood differentially affect the crime rates. In these models, in addition to the parolee per capita measure, we also included a measure taking into account the type of parolees to determine if it has an additional effect on monthly crime rates. In the models for specification 1 in Table 3, we see some evidence that a greater proportion of violent parolees increases tract crime rates. This measure of violent parolees has a significant positive effect on the burglary rate beyond the effect of total parolees. That is, whereas a one unit increase in non-violent parolees increases the burglary rate 4.6%, a one unit increase in violent parolees increases it 7.5% (exp(.045 + .029)=1.075). There is also some modest evidence that more violent parolees result in more murders.

<<<Table 3 about here>>>

On the other hand, there is no evidence here that serious non-violent prior arrests of neighborhood parolees have an effect on crime rates (see specification 2). For no type of crime does the measure taking into account serious non-violent prior arrests positively impact the crime rate after accounting for the total number of parolees. Thus, it appears that the violence of crimes leading to prior arrests, rather than serious property crimes, is what is important for determining the effect of parolees on neighborhood crime rates. Likewise, there is no evidence that the presence of "churners", as proxied by the number of previous incarcerations per parolee, increase the amount of crime (see specification 3).

Finally, we observe effects when taking into account the race of parolees. An increase in the number of African-American parolees per capita results in an increase in the aggravated assault rate and the burglary rate, even when accounting for the total parolees per capita (see specification 4). A one unit increase in non-black parolees increases the aggravated assault rate 2.4%, whereas a one unit increase in black parolees increases it 6.6%. And a one unit increase in non-black parolees increases the burglary rate 5.2%, whereas a one unit increase in black parolees increases it 7.7%. Note that we do not observe significant effects for the other two types of crime, which is somewhat surprising given prior evidence that African-Americans are more likely to commit robberies and murders than assaults or burglaries: the ratio of non-white to white robbery rates (7.25) and murder rates (6.31) are considerably higher than virtually all other Type I crimes, including aggravated assaults (3.17) and burglaries (2.29) (Uniform Crime Reporting Program, 2003).

Moderating effect of neighborhood characteristics on the relationship between returning parolees and crime

We next ask whether there are interaction effects between parolees and certain neighborhood characteristics. Consistent with our hypothesis that returning parolees may moderate the effect of broken households on crime, specification 1 in Table 4 shows an interaction effect. Although increasing the parolees per capita increases the amount of crime, this effect is dampened for aggravated assaults and burglaries by the proportion of single parent households in the tract. No such significant effect is found for robberies and murder. For instance, a one standard deviation increase in parolees usually increases the aggravated assault rate 12.2% (.098*1.174 = .122), whereas an equal increase in parolees in a tract with many single parent households (one standard deviation more) increases it just 4.5% (.098*1.174 + -.004*17.75 = .045). Likewise, a one standard deviation increase in parolees usually increases the burglary rate 10.3%, but a similar increase in a high single parent household tract increases it just 3.6%.

<<<Table 4 about here>>>

We also see support for the hypothesis that the formal social capital provided by voluntary organizations in a neighborhood can moderate the effect of returning parolees. The models in specification 2 of table 4 highlight that whereas returning parolees increase the amount of crime in a neighborhood, this effect is dampened for aggravated assaults and burglary by greater numbers of voluntary organizations per capita. In these models, a one standard deviation increase in parolees usually increases the aggravated assault rate 18.4%, but this increase is just 8.6% in a tract with more voluntary organizations per capita (one standard deviation increase); these values are 16.2% and 8.3% for burglary rates. This is consistent with the hypothesis that the resources provided by these organizations help a neighborhood cope with returning parolees who might otherwise increase crime rates.

We also see evidence in specification 3 that the informal social capital fostered by residential stability may be important for moderating the effect of returning parolees on robbery rates. Whereas a one standard deviation increase in parolees generally increases the robbery rate

9.2%, they actually *decrease* it 15.5% in a residentially stable tract (one standard deviation increase in stability). However, there is no evidence that this residential stability moderates the effect of parolees on the other types of crime.

Finally, given the importance of our measure of total voluntary organizations for moderating the effect of returning parolees on aggravated assaults and burglaries, we also estimated models separately including measures of different *types* of voluntary organizations per capita in the tract. The results in Table 5 highlight three key points. First, there is some evidence that organizations geared towards reducing crime and disorder moderate the effect of parolees on aggravated assault rates, but show little effect for the other three types of crime (specifications 1 and 2). Second, the strongest effects here appear for organizations geared towards helping youth (specification 6). We see evidence that increasing numbers of youth organizations in a neighborhood moderate the effect of returning parolees on both aggravated assault and burglary rates. This implies that these organizations may work in concert with returning parolees to provide private social control that reduces delinquent behavior. If burglaries are often committed by unsupervised youth, these youth organizations may be particularly effective in providing private social control to reduce this type of crime. Finally, we see little evidence that the other types of organizations—those geared towards economic issues, family issues, or general social issues—significantly moderate the effect of returning parolees on neighborhood crime rates.

<<<Table 5 about here>>>

Conclusion

Whereas recent scholarship has noted the large increase in prison incarceration over the last 20 years, and a number of studies have speculated on possible effects this may have on neighborhood crime rates in coming years, little empirical evidence exists regarding this issue.

This study therefore fills an important lacuna by providing a test of the effect of returning parolees on neighborhood crime rates. By linking data on the location and timing of parolees returning to neighborhoods with monthly crime rate data, we have provided a particularly rigorous test of this relationship. Our unique dataset allowed us to estimate fixed effects models and view the relationship between monthly changes in parolees per capita and monthly changes in crime rates. Because such models condition out all differences across neighborhoods, they are far less susceptible to specification error due to failing to account for important unchanging neighborhood characteristics. Only omitted *changing* neighborhood characteristics pose a challenge to obtaining unbiased estimates, and we suggest there is little reason to suspect that substantial levels of such change are occurring at such a fine-grained time frame as months.

Thus, our study provides particularly robust evidence that returning parolees in a neighborhood increase the rate of crime. We saw that a monthly increase in the parolees per capita in a tract resulted in significantly higher levels of aggravated assaults, robberies, and burglaries. Despite this consistent evidence, we caution that we are unable to say precisely *why* these neighborhood crime rates increased. As we discussed above, three equally plausible processes may be at work. One possibility is that such returning parolees directly increase the crime rate through their own criminal behavior. A second possibility is that these returning parolees increase the crime rate indirectly by reactivating ties with fellow co-conspirators, who then engage in more criminal activity. A third possibility is an even more indirect effect in which such returning parolees affect the structural conditions of the neighborhood—such as increasing the level of residential instability, as hypothesized by Rose and Clear (1998)--which then impacts the crime rate.

As evidence for the first possibility—that returning parolees directly impact the crime rate through their own criminal activity—we found that the relative violence of previous convictions for returning parolees significantly affects the level of burglaries and murders. Our finding regarding the effect of violent parolees on murder rates in neighborhoods suggests that the non-significant finding of a study using a general measure of parolees on murders in states (Kovandzic, Marvell, Vieraitis, and Moody, 2004) may have simply failed to measure the violent nature of released parolees, or lacked the fine-grained geographic and temporal gradations needed to adequately detect this effect. Given that the type of parolees returning to the neighborhood affects these rates of crime, this is suggestive that it is these parolees themselves who are increasing the rate of crime. Of course, it is also possible that these parolees are affecting the criminal behavior of those they are tied to, suggesting an additional indirect effect. Future studies viewing the behavior of such parolees would be necessary for teasing out such effects.

We saw evidence consistent with the hypothesis that returning parolees can affect the neighborhood structure as measured by the presence of broken households. Although mass imprisonment affects the household structure in neighborhoods and can have the unintended consequence of increasing crime rates, as suggested by Rose and Clear (1998), an unexplored possibility is that returning parolees will reunite families and thus reduce the number of broken households. We found that although returning parolees increase the rate of crime in neighborhoods, this effect is dampened in neighborhoods with high levels of broken households. We emphasize that we make no assumption that the process of incarceration changes these parolees, but suggest that reuniting families can accomplish this effect. Of course, it should be acknowledged that returning parolees are not always a positive force in families, as some cause strife. We also acknowledge that ours was not a direct test of this hypothesis, and other competing hypotheses could also explain these results: for instance, it is possible that neighborhoods with a high proportion of single-parent households already have such high base rates of crime that the marginal effect of adding parolees will have less effect than in other neighborhoods. Furthermore, if crime reporting is less consistent in neighborhoods with high levels of single-parent households, this could also bring about this observed effect. Our findings should be interpreted with caution, and suggest an avenue for future research to directly test these possibilities.

Our study also highlighted that the formal and informal social capital in a neighborhood can moderate the effect of parolees. We found that the formal social capital provided by a large number of voluntary organizations in the neighborhood moderated the effect of returning parolees on aggravated assault and burglary rates. This suggests that the resources provided by these organizations may be useful when reintegrating these parolees into the neighborhood. To test whether the impact of such voluntary organizations is due to their direct effect on parolees, future research will need to test whether such organizations directly affect the recidivism rate of parolees. It is nonetheless quite possible that the effect we are detecting is not simply due to the resources these organizations provide directly to returning parolees, but also due to the resources they provide to neighborhood residents. This may be particularly important for neighborhoods experiencing an influx of parolees: to the extent that such parolees may strain the level of social organization in a neighborhood, the presence of such formal resources may help in responding to problems when they arise. Although speculative, the findings of this study suggest this might be a fruitful direction for future research. Furthermore, it is possible that the size of this effect of this proxy for social capital was underestimated to the extent that neighborhoods with higher levels of social capital are more likely to report crime events to the police. Studies using other measures of neighborhood crime could better test this possibility. The fact that our ancillary models found that organizations geared towards helping youth seemed most important for moderating this relationship also suggests fruitful avenues for future research.

Additionally, we found evidence that the informal social capital in a neighborhood—as proxied by the level of residential stability—moderates the effect of returning parolees on the neighborhood robbery rate. It may well be that neighborhoods with more residential stability provide less anonymity, helping reduce this form of crime. If this is the case, returning parolees may be particularly unlikely to impact this type of crime in neighborhoods in which residents are more tightly linked. Although speculative, this suggests another direction for future work: in particular, studies will want to test this relationship using direct measures of the degree of cohesion and social interaction in a neighborhood, rather than using residential stability as a proxy for this construct.

Despite the uniqueness of our data and the importance of our findings, certain limitations should be acknowledged. First, our data only contained information on parolees and crime rates for one city. Although these data allowed us to carefully explore the effects of these returning parolees on neighborhood crime rates, the generalizability of our findings hinges on the extent to which this city is representative of other cities. Although we know of no reasons why this city is especially unique compared to other cities, confidence in the findings will nonetheless be increased by replications on other cities. Second, our data were limited to a particular four-year period. Our ability to generalize our findings to other time points thus should be treated with caution. Nonetheless, the recency of the data at least provides important evidence on the effect of parolees at this important juncture towards the tail end of the long run-up in incarceration.

Another limitation was our lack of information on the incapacitation effect of imprisonment. Although our study was able to account for parolees entering a neighborhood after exiting prison, and able to take into account when those same parolees either moved out of the neighborhood or returned to prison, we lacked information on other neighborhood residents who were arrested during this period. To the extent that imprisonment sends a signal that reduces criminal behavior and hence crime rates, we are unable to account for this effect. On the other hand, whereas studies have tested the effect of imprisonment on crime using large units such as states or the nation (Levitt, 1996; Vieraitis, Kovandzic, and Marvell, 2004), there may be less reason to expect that imprisonments of residents in the neighborhood will have a particularly strong effect on the crime committed by their fellow residents. Nonetheless, this is a useful avenue for future research.

Despite these limitations, it should be highlighted that the uniqueness of our data allowed us to explore important questions that have heretofore not been addressed. With more parolees returning to neighborhoods after a long period of mass incarceration, understanding the effects these returning parolees will have on neighborhoods is crucial. Our findings using a particularly rigorous method on rich data provide important insights from neighborhoods in one city in recent years.

The evidence that returning parolees can increase crime rates is certainly important especially when considering issues of public safety. This is underscored by the fact that areas receiving parolees with more violent criminal histories displayed increases in the rates of certain violent crimes. This raises important questions regarding current policies regarding decision to release. Whereas mandatory release guidelines have taken priority over discretionary parole practices, it may be useful to reconsider the benefits that discretionary parole can offer at the individual and community level.

Because certain types of parolees were found to affect neighborhood crime rates, it would also be advantageous to initiate policy interventions geared towards providing services for those parolees most at risk of recidivating. For example, the evidence that the formal resources of neighborhoods—as measured by the presence of voluntary organizations—can moderate the effect of parolees on crime rates suggests that positive gains can be made if these services are available. Nevertheless, caution should be used to make sure neighborhoods are not being overwhelmed by the demands and needs of returning parolees. Not all neighborhoods are similarly situated and the ability or inability to secure and provide resources for returning parolees undoubtedly influences the impact parolees will have on receiving neighborhoods (Kubrin & Stewart 2006). Releasing prisoners by means of discretionary parole practices may help to alleviate the immediate impacts of parolees on communities and foster timely reintegration.

In an era when more ex-prisoners are returning to our communities, the need for innovative and nuanced approaches is of utmost importance in order to lessen the negative impacts of parolees. Rethinking the way services are provided is one aspect that falls within this framework. Whereas previous approaches have routinely targeted individuals, targeting communities or specific areas may prove more beneficial (Rose, Clear, and Ryder, 2001). Doing so ultimately benefits individuals residing in communities most affected by the negative impacts of incarceration. It has also been suggested that more collaboration is needed between community members, institutions, and the criminal justice system to create an environment tailored for successful reintegration (Petersilia 2003). Regardless of the approach, policy interventions should make every effort towards providing services to neighborhoods most impacted by returning parolees. The findings thus provide important insights into a prominent social issue, as well as possible directions for policy reform and interventions.

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Endnotes

[1] The decision to prematurely release prisoners who are deemed less of a threat to public safety could have some bearing on the results of the study. To the extent that prisoners are released for good behavior or other mitigating factors, this might result in the marginal parolee being less criminal than the average parolee released in a given neighborhood. However, this would need to occur systematically over different neighborhoods to bias our results, and we know of no reason to expect such a systematic process.
[2] For instance, while this study had multiple years of data for each state, the modeling strategy of employing state fixed effects, time fixed effects, and a time trend, as well as a lagged effect of crime, resulted in a model explaining 95% of the variance. In such a model, there is little variation to be explained by virtually any other measures.
[3] While imprisoning mothers also should exhibit a similar effect, the fact that women are much more rarely imprisoned suggests this is largely a father effect. For instance, at mid-year 2006 just 7.1% of the prisoners under state or Federal jurisdiction were women (Sabol and Harrison 2007: 5).
[4] Whereas the Clear et al. (2003) study used a one-year lag of parolees to predict neighborhood crime rates, the failure to account for the prior year's crime rate reduces this to effectively viewing the levels of these two measures. Although informative, it therefore does not directly address the question of the relationship between parolees entering neighborhoods and changing crime rates.
[5] The effect of crime rates on parolees is much less direct. For instance, because there is not a direct relationship between crime events and crime arrests, there can be variability in the number of crime events, and the time frame, before the offender is arrested. After arrest, there is then variability in the time spent in the legal system, and then variability in time spent in the correctional system (based on the length of the sentence, etc.). As a consequence, any given crime begins to affect the generation of parolees over a rather wide period of time. Therefore, it is unlikely that this diffuse effect is significant. Whereas the use of suitable instrumental variables would be one manner to illustrate the direction of this effect, unfortunately no suitable instruments were available to us.
[6] We use untransformed measures of these constructs rather than log-transformed measures for three reasons: 1) we have no theoretical expectation of a diminishing effect for these measures on crime rates (as would be implicit in a log transformation); 2) the vast majority of the existing literature has similarly specified such a linear formulation; 3) ancillary models estimated with log transformed measures consistently showed similar or weaker relationships with crime rates (an empirical justification for our decision).
[7] A reviewer suggested that as an additional test of the stability of our tract measures computed in 2000 we compute estimates at the level of public use microdata area (PUMA's) units, given that this allows using data from the American Community Survey (ACS) that is contemporaneous with our crime data. A large limitation to this approach is that a PUMA is defined to have at least 100,000 persons. Thus they are 25 times larger than a census tract: one

consequence is that these ancillary models therefore only have a sample of 8 units, and a second consequence is that we are likely aggregating rather different tracts into a single geographic unit. Given recent work suggesting that aggregating to such units may obscure otherwise detectable effects, this may be a considerable limitation (Hipp, 2007a). Another limitation is we lose half of our sample time period given that the ACS first made the PUMA-level data available in 2005 (Missouri Census Data Center, 2006). To assess the effect of aggregating to this large unit of analysis, we first estimated multilevel models in which level one is the crime rate per capita in a given month, level two is the census tract, and level 3 is the PUMA. This allows us to partition the variance into these three levels to determine the extent to which tracts within a PUMA vary compared to the extent to which PUMA's vary across the city. The results suggest that the tracts being aggregated into PUMA's are quite heterogeneous: for instance, in the aggravated assault model, 31.5% of the variance is across months within a particular tract, 52.5% of the variance is across tracts within a PUMA, and just 15.9% of the variance is across PUMA's within the city. Thus, the variability in aggravated assault rates across tracts *within* a PUMA is three times the size of the variability *across* PUMA's. The pattern is similar for the other crime measures: for robberies, 52% of the variance is across months within a particular tract, 34.6% of the variance is across tracts within a PUMA, and 13.4% of the variance is across PUMA's within the city. For burglaries, the values are 39%, 49.6%, and 11.4%. And for murders the values are 97.1%, 2.3%, and 0.6%. Thus, there is generally about three to four times as much variability in crime across the tracts *within* a PUMA as *across* PUMA's, questioning the assumption that these are homogeneous geographic units. It is thus not clear how much faith to place in these PUMA-level analyses for 2005-06. Nonetheless, the results of the ACS models were consistent with those presented in the text. Given that PUMA fixed effects models generally showed non-significant results (as we are estimating a model with just 8 aggregated units over just two years of data), we also estimated models without the fixed effects. These latter models yielded results consistent with those in the study: while returning parolees have a positive effect on all four types of crime, this effect is moderated in tracts with more single parent households, or tracts with more residential stability. The direction of these effects is the same as our tract-level models in this study (results available upon request).

[8] We obtained this information from the MABLE/GEOCORR website at the University of Missouri that places zip codes into tracts based on population at http://mcdc2.missouri.edu/websas/geocorr2k.html.

[9] As Allison and Waterman (2002) discuss, the conditional fixed effects negative binomial regression of Hausman, Hall, and Griliches (1984) does not appropriately account for differences across units, as it only accounts for the difference in the distribution of the overdispersion across units, rather than accounting for the differences in the parameters.

[10] To assure the stationarity of the time process in our models, we estimated additional specifications. To test whether we had indeed accounted for possible autocorrelation effects, we estimated models that also included the lagged effect of the crime of interest. This lagged effect was not significant in the robbery or murder models. And while the effects were significant in the burglary and aggravated assault models, the substantive results of our measures of interest remained essentially unchanged. Given that including the lagged outcome creates additional complications as it is likely correlated with the disturbance term (Greene, 2000: 583-84), we present the models without these lagged endogenous measures. As another way of testing for autocorrelation effects, we conducted an instrumental variable (IV) test suggested to us by Richard McCleary (personal communication on 3/26/2008). In this test, we first estimated our model of interest, we then computed the linear prediction of the outcome variable for each case

based on the parameter estimates, we then created a one-month lagged version of this predicted value, and then we re-estimated the model including this lagged predicted value of the outcome as a predictor. Thus, we are essentially computing an instrumental variable version of our lagged outcome, and testing whether it has an additional effect. In none of the four models was this measure significant, suggesting again little evidence of remaining autocorrelation.

[11] We estimated several different models to assess the robustness of our results. We estimated models that included: 1) just the tract fixed effects; 2) the tract fixed effects and 11 indicator variables for the months in the year; 3) the tract fixed effects and three indicator variables for seasons of the year; 4) tract fixed effects and a linear trend variable as well as a quadratic trend variable; 5-7) models 1-3, augmented by the linear and quadratic trend. For the seasonal-effects models (models 2, 3, 6, and 7) although the seasonal measures were significant, they did not approximate a cosine function with a peak in the summer, a result that is consistent with a previous study of cities using a random cosine function finding that cities in mild-weather California showed the weakest such seasonal effects for both violent and property crime compared to cities in other parts of the country (Hipp, Bauer, Curran, and Bollen, 2004). We also attempted to estimate models with a time trend for each tract, but the models would not converge for three of the four outcomes. Only for the robbery outcome did the model converge; although the size of the coefficient for parolees was somewhat smaller than in the models presented in the text, it was still statistically significant.

[12] Estimating the same models constraining this dispersion parameter to be equal over all four years yielded substantively similar results. We nonetheless freed this additional constraint to further increase confidence in the findings.

[13] To interpret these results, we plotted the predicted values from these models. This is a common practice suggested by Aiken and West (Aiken and West, 1991) for interpreting interactions.

[14] A reviewer suggested an additional intriguing possibility: perhaps the effect of parolees on crime is not contemporaneous, but rather lagged. That is, parolees may initially attempt to avoid recidivating, but the lack of opportunity for legitimate employment may cause them to turn to crime in subsequent months. We thus tested whether the change in returning parolees not only affects the change in crime in the current month, but also in the following month. No significant effects for this lagged measure were detected in the models of our four crime outcomes (results not shown).

Tables and Figures

Table 1. Summary statistics for measures used in analyses

	Sample statistics	
	Mean	SD
Monthly measures		
Assaults	2.18	2.97
Robberies	1.03	1.67
Burglaries	2.96	3.74
Murders	0.03	0.18
Parolees	27.25	28.02
Parolees per capita (logged)	-0.88	1.17
Violent parolees per capita	0.33	0.81
Serious parolees per capita	0.29	0.57
Churner parolees per capita	2.55	4.45
African-American parolees per capita	0.30	0.55
Time invariant measures		
% single parent households	18.01	8.83
Residential stability - average years of residence	7.22	3.44
Total organizations per 1,000 persons (logged)	0.39	1.19
Social organizations per 1,000 persons (logged)	-1.59	2.39
Crime organizations per 1,000 persons (logged)	-5.43	2.34
Disorder organizations per 1,000 persons (logged)	-5.81	2.14
Economic organizations per 1,000 persons (logged)	-5.54	2.48
Family organizations per 1,000 persons (logged)	-6.73	0.90
Youth organizations per 1,000 persons (logged)	-0.28	1.10

Note: N=152 tracts in 48 months from 2003-06

Table 2. The effect of parolees per capita (logged) on various types of crime by month, negative binomial regression models for Sacramento census tracts, 2003-06

	Aggravated assault		Robbery		Burglary		Murder
Model without spatial lag							
Parolees per capita, logged	0.070	*	0.159	**	0.079	**	0.615
	(0.032)		(0.046)		(0.028)		(0.421)
Pseudo R-square	0.339		0.310		0.334		0.259
Model with spatial lag							
Parolees per capita, logged	0.044		0.139	**	0.073	*	0.617
	(0.032)		(0.046)		(0.029)		(0.434)
Spatial lag of logged parolees per capita	0.006		0.268	†	-0.124		1.158
	(0.089)		(0.142)		(0.086)		(1.083)
Pseudo R-square	0.339		0.310		0.334		0.259
Number of tract months	7,296		7,296		7,296		7,296
Log likelihood	-9,535.4		-7,011.8		-11,220.2		-658.2
Number of unit fixed effects	151		151		151		151
Number of monthly fixed effects	47		47		47		47

*Note: ** p < .01; * p < .05; + p < .1. Standard errors in parentheses. N=152 tracts in 48 months. Fixed effects negative binomial regression models contain 151 indicators for tracts, 47 indicators for month of study, and one-month lag of crime type. Dispersion parameter allowed to vary over years.*

Table 3. The effect of parolees per capita (logged) and various types of parolees per capita (logged) on various types of crime by month, Sacramento census tracts, 2003-06

	Aggravated assault		Robbery		Burglary		Murder	
Specification 1								
Parolees per capita (logged)	0.059	†	0.116	*	0.045		0.357	
	(0.034)		(0.049)		(0.030)		(0.447)	
Violent parolees per capita (logged)	0.010		0.021		0.029	**	0.227	†
	(0.012)		(0.018)		(0.010)		(0.135)	
Spatial lag of logged parolees per capita			0.269	†				
			(0.142)					
Specification 2								
Parolees per capita (logged)	0.085	*	0.201	**	0.108	**	0.810	†
	(0.035)		(0.050)		(0.031)		(0.477)	
Serious parolees per capita (logged)	-0.017		-0.062	**	-0.026	*	-0.122	
	(0.014)		(0.020)		(0.011)		(0.133)	
Spatial lag of logged parolees per capita			0.274	†				
			(0.141)					
Specification 3								
Parolees per capita (logged)	0.235	**	0.239	**	0.169	**	0.147	
	(0.065)		(0.093)		(0.055)		(0.678)	
Churner parolees per capita (logged)	-0.151	**	-0.092		-0.081	†	0.495	
	(0.051)		(0.074)		(0.043)		(0.574)	
Spatial lag of logged parolees per capita			0.265	†				
			(0.141)					
Specification 4								
Parolees per capita (logged)	0.024		0.094	†	0.050		0.569	
	(0.037)		(0.055)		(0.031)		(0.461)	
African-American parolees per capita (logged)	0.041	*	0.041		0.025	*	0.037	
	(0.018)		(0.028)		(0.012)		(0.154)	
Spatial lag of logged parolees per capita			0.266	†				
			(0.141)					

*Note: ** p < .01; * p < .05; + p < .1. Standard errors in parentheses. N=152 tracts in 48 months. Fixed effects negative binomial regression models contain 151 indicators for tracts, 47 indicators for month of study, and one-month lag of crime type. Dispersion parameter allowed to vary over years.*

Table 4. The effect of parolees per capita (logged) moderated by various neighborhood characteristics on various types of crime by month, Sacramento census tracts, 2003-06

	Aggravated assault	Robbery	Burglary	Murder
Specification 1				
Parolees per capita (logged)	0.098 **	0.142 **	0.084 **	0.622
	(0.035)	(0.050)	(0.028)	(0.419)
Percent single parent households X parolees per capita (logged)	-0.004 *	0.000	-0.004 *	0.014
	(0.002)	(0.003)	(0.002)	(0.036)
Spatial lag of logged parolees per capita		0.267 †		
		(0.142)		
Specification 2				
Parolees per capita (logged)	0.144 **	0.159 *	0.128 **	0.257
	(0.043)	(0.064)	(0.038)	(0.523)
Voluntary organizations per 1,000 (logged) X parolees per capita (logged)	-0.046 *	-0.012	-0.037 †	0.360
	(0.018)	(0.027)	(0.019)	(0.316)
Spatial lag of logged parolees per capita		0.272 †		
		(0.142)		
Specification 3				
Parolees per capita (logged)	0.091 **	0.075	0.076 **	0.677
	(0.035)	(0.050)	(0.029)	(0.429)
Residential stability X parolees per capita (logged)	0.015	-0.048 **	-0.004	-0.122
	(0.010)	(0.014)	(0.008)	(0.118)
Spatial lag of logged parolees per capita		0.267 †		
		(0.141)		

*Note: ** p < .01; * p < .05; + p < .1. Standard errors in parentheses. N=152 tracts in 48 months. Fixed effects negative binomial regression models contain 151 indicators for tracts, 47 indicators for month of study, and one-month lag of crime type. Dispersion parameter allowed to vary over years.*

Table 5. The effect of parolees per capita (logged) moderated by types of voluntary organizations in the tract on various types of crime by month, Sacramento census tracts, 2003-06

	Aggravated assault		Robbery		Burglary		Murder
Specification 1							
Crime organizations per 1,000 (logged) X parolees per capita (logged)	-0.026	†	-0.003		-0.012		0.040
	(0.014)		(0.019)		(0.012)		(0.152)
Specification 2							
Disorder organizations per 1,000 (logged) X parolees per capita (logged)	-0.026	*	0.010		0.011		-0.007
	(0.012)		(0.019)		(0.015)		(0.173)
Specification 3							
Social organizations per 1,000 (logged) X parolees per capita (logged)	-0.018		-0.006		-0.005		0.090
	(0.013)		(0.020)		(0.014)		(0.167)
Specification 4							
Economic organizations per 1,000 (logged) X parolees per capita (logged)	-0.004		0.028	†	0.012		0.128
	(0.010)		(0.014)		(0.010)		(0.127)
Specification 5							
Family organizations per 1,000 (logged) X parolees per capita (logged)	0.118		0.065		0.085		-1.752
	(0.101)		(0.175)		(0.098)		(1.651)
Specification 6							
Youth organizations per 1,000 (logged) X parolees per capita (logged)	-0.055	**	-0.029		-0.043	*	0.506
	(0.019)		(0.028)		(0.020)		(0.339)

*Note: ** p < .01; * p < .05; + p < .1. Standard errors in parentheses. N=152 tracts in 48 months. Fixed effects negative binomial regression models contain 151 indicators for tracts, 47 indicators for month of study, and one-month lag of crime type. All models contain main effect for logged parolees per capita. Dispersion parameter allowed to vary over years.*

Appendix

Table A1. The effect of parolees per capita (logged) on various types of crime by month, using linear, quadratic, and cubic specifications, Sacramento census tracts, 2003-06

	Aggravated assault		Robbery		Burglary		Murder
Specification 1: linear							
Parolees per capita	0.048	†	0.136	**	0.059	†	0.164
	(0.025)		(0.041)		(0.033)		(0.274)
Specification 2: quadratic							
Parolees per capita	0.121	**	0.248	**	0.092	*	0.379
	(0.035)		(0.053)		(0.039)		(0.390)
Serious parolees per capita squared	-0.008	**	-0.015	**	-0.005	†	-0.022
	(0.003)		(0.004)		(0.003)		(0.028)
Specification 3: cubic							
Parolees per capita	0.104	*	0.316	**	0.042		0.680
	(0.047)		(0.076)		(0.068)		(0.991)
Serious parolees per capita squared	-0.010	*	-0.008		-0.010		-0.001
	(0.005)		(0.008)		(0.006)		(0.070)
Serious parolees per capita cubed	0.000		-0.001		0.001		-0.004
	(0.001)		(0.001)		(0.001)		(0.011)

*Note: ** p < .01; * p < .05; + p < .1. Standard errors in parentheses. N=152 tracts in 48 months. Fixed effects negative binomial regression models contain 151 indicators for tracts. Dispersion parameter allowed to vary over years.*

Table A2. The effect of parolees per capita (logged) moderated by various neighborhood characteristics on various types of crime by month, Sacramento PUMA's, 2005-06

	Aggravated assault		Robbery		Burglary		Murder	
Specification 1								
Parolees per capita (logged)	3.762	**	6.776	**	5.248	**	5.903	**
	(0.325)		(0.621)		(0.389)		(1.377)	
Percent single parent households X parolees per capita (logged)	-0.109	**	-0.303	**	-0.205	**	-0.280	**
	(0.021)		(0.038)		(0.025)		(0.081)	
Specification 2								
Parolees per capita (logged)	2.409	**	2.403	**	2.757	**	1.517	**
	(0.182)		(0.207)		(0.220)		(0.359)	
Voluntary organizations per 1,000 (logged) X parolees per capita (logged)	-0.332	*	-0.432	*	-0.668	**	-0.244	
	(0.161)		(0.175)		(0.175)		(0.300)	
Specification 3								
Parolees per capita (logged)	4.759	**	9.389	**	6.653	**	6.268	**
	(0.407)		(0.715)		(0.448)		(1.746)	
Residential stability X parolees per capita (logged)	-0.322	**	-0.860	**	-0.535	**	-0.554	**
	(0.048)		(0.080)		(0.052)		(0.190)	

*Note: ** p < .01; * p < .05; + p < .1. Standard errors in parentheses. N=8 public use microdata areas (PUMA's). Negative binomial regression models*