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Crime and Urban Flight Revisited: The Effect of the 1990s Drop in Crime on Cities

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Abstract

The 'flight from blight' and related literatures on urban population changes and crime have primarily considered times of high or increasing crime rates. Perhaps the most cited recent work in this area, Cullen and Levitt (1999), does not extend through 1990s, a decade during which crime rates declined almost continuously, to levels that were lower than experienced in decades. This paper examines whether such declines contributed to city population growth and retention (abated flight). Through a series of population growth models that attempt to identify causality through several strategies (including instrumental variables) we find at best weak evidence that overall city growth is affected by changes in crime. We find no evidence that growth is differentially sensitive to reductions in crime, as compared to increases. Focusing more narrowly on within MSA migration, residential decisions that are more likely to be sensitive to local conditions, we do find evidence supporting abatement of 'flight'—that is, lower levels of crime in central cities in the 1990s are associated with lower levels of migration to the suburbs. This greater ability to retain residents already in the city does not appear to be accompanied by a greater ability to attract new households from the suburbs, or from outside of the metropolitan area.

For most of the twentieth century, concerns about safety and high crime rates have beset U.S. cities. Researchers and policymakers pointed to these high urban crime rates as one of the chief 'urban blights' from which middle class, mobile (and typically white) households fled during the post-War period, fueling suburbanization. But this picture changed dramatically in the 1990s, a decade during which the crime rate in the U.S. fell by a remarkable thirty percent, and crime rates in many U.S. cities declined even further.

This paper builds on the 'flight from blight' literature, and considers what effect (if any) the 1990s drop in crime rates had on urban population changes. While the empirical evidence is somewhat mixed about the effect of crime *levels* on population changes, work by Cullen and Levitt (1999) suggests that *changes in crime* during the late 1970s and 1980s may have contributed to central city flight, particularly of more affluent households and those with children. Their work, however, does not extend through the 1990s. Unlike the two previous decades, the 1990s was a period of almost continuous declines in crime rates, to levels that were lower than experienced in decades. Did this drop in crime reverse, or at least abate, urban flight? It is not obvious *a priori* that the relationship between crime and residential decisions would be symmetric – while increases in city crime may push residents away from cities, similarly-sized reductions may not attract or retain them.

We examine this more recent time period using census data for 1980 through 2000, for more than 100 central cities and their surrounding metropolitan areas, in combination with more than 20 years of Uniform Crime Report data. We first consider the effect of crime on overall changes in city population, attempting to identify causality through models with lagged measures of crimes, and through an instrumental variables strategy based on state-level measures of stringency of state criminal justice systems. Relying on models with lagged measures of crime, we find at best weak evidence that overall city growth is affected by changes in crime, and considerable evidence that researchers using similar data need to take seriously a number of measurement and estimation issues. We find no evidence that growth is differentially sensitive to increases in crime, as compared to decreases.

We then examine the relationship between city crime and particular sources of population change (migration into cities from surrounding suburbs and migration out of cities into surrounding suburbs) that are more likely to be sensitive to local conditions. Here we find evidence supporting an abatement of 'flight'—that is, lower levels of crime in central cities in the 1990s are associated with lower levels of migration to the suburbs. This greater ability to retain residents already in the city does not appear to be accompanied by a greater ability to attract households from the suburbs, or from outside of the metropolitan area.

I. Background

High crime rates in cities have long been viewed as one of the chief causes of suburbanization and urban flight (for overviews, see Bradford and Kelejian, 1973; Mieszkowski and Mills, 1993). However, empirical support for the importance of crime rates in shaping residential location decisions is fairly mixed. Indeed, even a cursory review of the literature provides evidence of a positive relationship between central city crime and suburbanization or flight (Marshall, 1979, Grubb, 1982), an insignificant relationship (Frey, 1979; Mills and Price, 1984), and occasionally, a negative relationship (Jordon et al, 1998). There are several possible explanations for these varied results, namely : the particular residential choices examined, the time periods covered, and the extent to which potentially correlated factors are adequately controlled for. We provide a selective review of the literature to expand on these points.

Several authors have considered the impact of city crime levels on rates of central city population growth relative to their suburbs (suburbanization) using density gradients as a proxy for suburbanization. Here, both migration between the central city to the suburbs and where new migrants to the MSA choose to live matter for the shape of the density gradient. But still the evidence is mixed even with this focus. Mills and Price (1984), for example, examine density gradients for thirty-five to fifty-eight metropolitan areas between 1960 and 1970, finding no evidence that high levels of central city crime relative to the surrounding suburbs has contributed to U.S. suburbanization.[1] Also using density gradients, Jordan et al (1998) examine the 1980s and find higher city crime rates are associated with *less* suburbanization, counter to their expectations.

Consistent with the notion of 'flight,' much of the literature focuses more narrowly on intra metropolitan location decisions of existing residents when considering the potential effects of crime. Frey (1979), for instance, uses aggregate central city migration data on 39 large MSAs from 1967 to 1970, to study the mobility (whether to move) and migration (where to move) decisions of white households who remain within the MSA. While central city crime rates do not appear to affect the likelihood of moving, city crime rates do appear to have a modest effect on the likelihood that white households move to the suburbs, conditional on their changing homes. However, once other measures of city decline and racial composition are controlled for directly, the independent influence of crime is quite small. Focusing on the latter half of this same time period and 112 metropolitan areas with large central cities, Marshall (1979) finds that for white households, crime matters for the location decisions of households who already live in a metropolitan area but not the location choices of new households moving into a metropolitan

[1] They do, however, control for a variety of other city characteristics, including the racial composition of the central city population, variables that are significant and are correlated with crime.

area. Specifically, central city crime appears to have a moderate affect on the decision of white households to move from the central city to the suburbs, while it appears to have no effect on the decision of whether to locate in the city or suburbs for households entering a metropolitan area.[2]

Greenwood and Stock (1990) show how sensitive estimates of the impact of crime on location decisions can be to the time period and type of location decision. They estimate a structural model of the metropolitan location of population, employment and housing to assess the relative importance of these interdependent forces on suburbanization in the 1950s, 1960s and 1970s for sixty-two MSAs.[3] They consider both intra-metropolitan location decisions and in migration to MSA. The authors find some limited support that crime levels affect population flows, but that these effects vary by group (income level), over time, and by type of migration decision. High crime rates do appear to have increased population movement to the suburbs, but only of high-income households and only in the 1950s. Similarly, high crime rates appear to have decreased the share of in-movers to metropolitan area who choose to locate in the central city, but only for lower income households and primarily in the 1950s. In short, over the entire time period and all types of location choices, there is no simple summary of the relationship between city crime rates and changes in household location.

A likely additional reason for the mixed evidence is the possible reverse causality between household location and crime. There may be several channels through which these residential decisions themselves -- and suburbanization in particular -- actually affect crime rates. If models fail to correct for this reverse causality, then their estimates are likely to be biased. Farley (1987) suggests several avenues of reverse causality – reasons, that is, why suburbanization itself may cause crime in the central city to increase. First, assuming

[2] In contrast, South and Crowder (1997) find it is black rather than white households who are sensitive to city crime rates, although during a slightly later time period (1979 to 1985).
[3] The effect of crime, per se, is not the focus of their work.

suburbanites commute to the central city for work or other activities, then the true population of potential victims in the city will be greater than the residential population used for computing crime rates, and thus crime rates computed for cities with greater suburbanization will appear inflated. In addition, because migration to the suburbs is selective, greater suburbanization may result in larger compositional differences between the populations living in central cities and its suburbs. In areas with high rates of suburbanization, the population remaining in the city may be relatively less educated, more disadvantaged, and more likely to be involved in crime (either as a perpetrator or a victim). Finally, suburbanization may contribute to social stratification that itself increases central city crime, even after controlling for any compositional differences. Jargowsky and Park (2006) provide a more recent assessment of these theories and the empirical work attempting to disentangle such 'reverse' causation. They conclude that separate from any compositional effect, suburbanization itself contributes to higher crime rates in cities.[4]

Cullen and Levitt (1999) offer some of the most recent (and widely cited) work in this area. Their work differs in several ways from the bulk of the literature cited. First, the authors broaden the population changes examined. They consider both overall growth in central cities (not relative to their suburbs), and in separate models, the more frequently examined intra metropolitan choices. Second, unlike the vast majority of previous work, they focus on changes in crime rates, rather than levels. Using a series of data sources and models, they find that increases in crime rates lead to central city population losses, and that white households, families with children, and those with greater education are more sensitive to changes in crime. The authors also find that changes in crime rates have their greatest influence on relocation decisions within the metropolitan area. Perhaps most importantly, their work improves on existing

[4] While several of the studies previously cited do attempt to deal endogeniety of employment location decisions and housing prices, they do not address the potential endogeniety of crime.

research by employing an instrumental variables strategy to better identify a causal relationship. Their instrument results suggest the existence of a positive relationship running from city population to crime, one that leads conventional estimates of the negative effect of increases in crime rates on city population growth to be downwardly biased.

Our work adds to this literature by examining the effects of crime during a time period that has gone largely unexamined. More importantly, it includes a time period when crime rates declined in most (though not all) U.S. cities. Earlier work has studied the 1960s, 1970s, and 1980s when crime was generally rising in cities. Focusing on the time period between 1980 and 2000, we are able to investigate the effect of the large 1990s reductions in crime rates on residential location decisions, and to assess whether the effects of crime reductions are equal and opposite to the effects of crime increases. There are several reasons one might expect differential reaction to increases versus decreases. One possibility is that households react less quickly and strongly to decreases in crime than to increases. Risk-averse households may worry that crime may rise again soon. The media may cover decreases in crime less heavily than increases, and more fundamentally, cities may have a difficult time shedding their high-crime reputations. In contrast to these expectations, Pope (2008) finds fairly similar effects on housing prices in Hillsborough, Florida when a registered sex offender moves into a nearby house as when the offender moves out. The effects are similar in magnitude and speed (although such changes in prices do not require the adjustment time and moving costs of population changes).

Differences in adjustment costs, both at the housing market and household level, may also matter. Prompted in part by increases in crime during 1960s, 1970s and 1980s, housing development at the suburban fringe makes suburbanization hard (or at least slow) to reverse. As city crime falls, we might expect to see price declines for the sizable stock of suburban housing, could keep the suburbs relatively attractive. At the household level, this may be particularly true for those already located in the suburbs, who would face moving costs to access the improved safety in the center city. However, for households already in the central city, declines in city crime rates of similar magnitude may be a large enough inducement to remain in the city. Indeed, this last argument would suggest that decreases in central city crime would have larger effects on the retention of residents than would increases (all else the same).

Finally, we also pay far more attention to issues of measurement and estimation problems than most of the existing work. We identify several sources of bias that appear to be significant and may cloud interpretation of causal relationships.

Building on the work of Cullen and Levitt (1999), we structure our empirical work around two sets of questions. First, we examine whether changes in central city crimes rates lead to overall population changes for cities during times of crime reductions. Second, we examine the effect of crime on city-to-suburb and suburb-to-city moves.

II. Data and Empirical Strategies

To address our two primary research questions, we employ two slightly different data sets. Our first data set is a city-level panel extending from 1980 through 2000, for all central cities in 1980 with populations of at least 100,000. Annual data on city and county crime rates are taken from the Federal Bureau of Investigation's *Uniform Crime Reports* (UCR). We use data on violent crimes, property crimes, and all index crimes. The annual city population data used to determine the UCR per capita crime rates (and annual changes in city population) are provided to the FBI by the Census. The population estimates for non-decennial years, which rely on such information as the Current Population Survey and birth and death records, are clearly imprecise. Appendix A provides more details on the sources of all data used.

This annual model permits us to examine year-to-year population changes for a large number of cities (145) over a twenty-year period. The panel nature permits us to control for constant city characteristics with fixed effects, and provides enough variation for an instrumental variable strategy. However, there are only limited city-level controls available on an annual basis, and as noted, the annual city population estimates themselves may be measured with error. Thus, we also estimate a 1980-2000 decade model of city population change, which utilizes more extensive and accurate data from the decennial census.

These models help us to understand the predictors of net changes in city population from all sources, including births, deaths and migration from outside the country. As noted, previous empirical work suggests varying sensitivities to crime for different types of residential moves. Indeed, much of the prior literature focuses not on aggregate city growth but rather on the effects of crime on city to suburb and suburb to city migration decisions. We use a different data set to examine the effect of crime on these intra-metropolitan migration decisions. Specifically, we rely on city-level migration data from the 2000 census. For all central cities, the Census reports the number of residents in 2000 who were also residents of the city in 1995, the number of residents who have newly moved to the city in the past 5 years, and the number of 'new' residents who moved from elsewhere in the metropolitan area (that is, from outside the central city but within the MSA). For a subset of central cities – the 111 central cities that are the only central city in their metropolitan area, these data combined with similar migration data for the entire MSA allow us to fully capture all intra metropolitan area migration –to and from the central city - and thus to separately examine the role of crime in 'flight' and attraction.

Unfortunately, there is not enough cross-state variation in these data to use our state-level instrument strategy.[5]

III. Do Crime Rates Affect Overall Population Changes for Cities?

To start, we examine annual changes in central city population, over a twenty-year time period beginning in 1980. As Figure 1 shows, average central city crime rates varied quite a bit during this period, experiencing both increases and decreases.[6] After 1992, however, mean crime rates declined almost continuously. To our knowledge, no paper examining the links between crime and urban growth or flight has extended the analysis beyond 1993, and thus this literature has to date missed this period of significant decline.[7]

[Figure 1 here]

It's worth raising two issues related to the measurement of crime in models of urban growth or flight. The first is whether to consider crime levels or changes. As previously noted, almost all of the empirical literature on crime and flight (or suburbanization) examines crime levels. By contrast, Cullen and Levitt (1999) argue that it is changes in crime rates that should matter for flight. They posit that the level of crime has already been factored into decisions of residents to live in a city (and presumably capitalized into housing prices), and thus that it would take a change in crime rates to push households from central cities. They also maintain that

[5] Specifically, our first stage results are not strong enough to justify reporting instrumented second stage results.
[6] Our graph depicts the average index crime rates for our sample of central cities. We find similar patterns for property and violent crime rates, separately.
[7] While not directly focused on crime, Hymel's (2009) study of the effect of congestion on MSA employment growth from 1982 to 2003, includes metropolitan crime rates as one control. While he finds no cross-sectional relationship between crime and employment growth, he does find a significant negative association in his panel model (which resembles our annual population model).

relying on changes in crime rates has the added benefit of minimizing differences in reporting practices across police jurisdictions.

However, once a household makes a decision to move, decisions that are frequently prompted by changes in a household's housing needs due to changes in household size or aging, the choice of which jurisdiction to settle in (and in particular, whether to choose the city or a jurisdiction outside) may well depend on the current assessment of the relative attractiveness of different location alternatives, which may be based on crime levels in addition to recent changes in crime. Thus, it is unclear a priori whether reductions in crime, or simply low levels of crime, should be more likely to induce households to move to or choose to stay in central cities. For this reason, we test for effects of both levels and changes.

A second measurement issue concerns the appropriate temporal relationship between crime and population changes. Many researchers, including Cullen and Levitt (1999), have relied on contemporaneous changes in crime and population. However, if it takes time to learn about changes in crime rates and takes added time to move to a new housing unit, one would expect a lag between the observed change in crime and the shift in population it provokes. Indeed in recent work examining changes in housing prices and changes in neighborhood crime rates, Ihlanfeldt and Mayock (2009) find that only crime changes lagged at least two years have any significant relationship to changes in housing prices, even when crime is instrumented.

Lagging crime rates also helps to address endogeneity -- if crime and population changes are measured contemporaneously, many of the residential moves underlying any changes in population could potentially precede any changes in crime. Thus, we focus on lagged changes in crime and/or crime rates at the start of the period in our models.

We begin with an annual model of city population growth.[8] The basic models are as follows:

$$\Delta\ln \text{population}_{it\,(t+1)} = \alpha + \beta_1\Delta\, \text{CityCrime}_{i\,(t-1),\,t} + \gamma X_{it} + \delta_t + \lambda_r + \varepsilon_{it} \quad (1)$$

$$\Delta\ln \text{population}_{it\,(t+1)} = \alpha + \beta_2 \text{CityCrime}_{it} + \gamma X_{it} + \delta_t + \lambda_r + \varepsilon_{it} \quad (2)$$

where subscripts i, t, and r reference city i, year t, and region r. The dependent variable is the change in the log of city population from year t to t+1. The independent variables include changes in city crime rates over the previous year (equation one) or city crime rates at the start of the time period (equation two). X is a vector of characteristics of the city or the larger area in which it resides, including the percentage of the city's population that is black, the percentage that is foreign born, the city median family income,[9] the metropolitan area unemployment rate, state per capita income, measures of the age distribution of the state population, and the percentage change in the state's population (lagged one year).[10] All baseline variables are measured at the start of the time period.

We estimate each of these equations with three different set of fixed effects. First, we include year and region dummies. Second, we include year dummies and city fixed effects in place of the region dummies, and third, we include city fixed effects and region-year interactions to capture time trends that differ across regions. We think the models with the city fixed effects are preferable, as they clearly do a better job at controlling for unobserved, time-invariant

[8] This model is very similar in structure to Cullen and Levitt (1999), although as noted, we lag change in crime rate one year to allow for time for households to learn about crime rates and make adjustments and to minimize concerns about endogeneity.
[9] We calculate percentage black and percent foreign born in the city for inter-censal years by using simple linear interpolation. For median family income, we interpolate based on annual changes in state per capita income.
[10] We only include the percentage change in the state population in the models that do not include city fixed effects, though the results are largely the same when we include changes in state population.

characteristics of cities.[11] We estimate these models using weighted least squares to account for heteroskedasticity, though results are almost identical using OLS. Table 1 presents a summary of our findings, employing different controls for crime.

[Table 1 here]

The first three columns of Table 1 report results for models using changes in crime over the prior year. As shown, the coefficient on the change in crime is not significantly different from zero in any of the models. When we instead include per capita crime rates at the start of the period (models four through six), we find that the coefficient on the number of crimes per capita is positive and statistically significant in two of the three specifications. In other words, contrary to expectations, city growth rates are *higher* in cities with higher initial crime levels. (When we include both the levels of crime and the change in crime variables, we obtain very similar coefficients on the crime level variables.)

These results hold up to multiple robustness tests. For example, when estimating these same models but also incorporating controls for suburban crime, we obtain essentially the same results.[12] When estimating the models in Table 1 with all the independent variables measured in difference form, we again obtain the same results. We have also experimented with discrete measures of crime change, such as top and bottom quintiles of change, to permit non-linear and threshold effects. The results are again qualitatively the same. When we stratify our sample by city size, we find essentially the same results in both large and small cities. Finally, to test whether crime matters differently in cities that face other challenges, we interact our crime variable with unemployment rates (one set of models), and with the share of the population that

[11] While Cullen and Levitt (1999) do conduct one robustness test with city fixed effects, their main model specification and additional tests on this are without city fixed effects.

[12] Given our concerns about the quality of data underlying suburban crime rates, we prefer to present the models that rely solely on city crime. But when we replicate our analyses with models including suburban crime or ratios of city to suburban crime, we obtain essentially the same results.

is black (another set of models).[13] Again, we get consistent results. Across the board, where the coefficients on crime rate levels are significant, the coefficients are positive, and the coefficients on changes in crime remain consistently insignificant.[14]

Finally, we test whether increases in crime have the same effects as reductions. As noted previously, there are theoretical reasons to believe that increases in crime should have larger impacts than decreases. With this in mind, we estimate a series of spline models that allow increases in city crime to have a different effect than decreases. Contrary to our theoretical priors, we find no difference. Neither increases nor decreases in crime in the prior period appear to have any impact on city growth rates in the current period.

Despite the robustness of our findings, they raise two puzzles. First, it seems implausible that higher crime rate levels are actually attracting residents. Second, it is unclear why our results for changes in crime should be so divergent from those of Cullen and Levitt (1999), who use a very similar approach but consistently find that larger increases in crime are significantly associated with lower rates of city growth.[15] By contrast, we find that changes in crime have no discernible effect. Yet the only substantive differences between our annual model and theirs are the time period covered and the relative timing of the crime change measures.

In terms of the time period, it is possible that households were more sensitive to crime during the pre-1993 time period that Cullen and Levitt (1999) studied. To test for this possibility, we re-estimate our models for 1980-1993 only. Our results again remain essentially

[13] This would also capture the extent to which higher crime rates make non minority households less willing to live in areas with large minority or black population, akin to Gautier et al's (2009) finding with respect to large Muslim communities in Amsterdam after the highly publicized murder of Theo Van Gogh.
[14] All results available from authors upon request.
[15] More accurately, Cullen and Levitt find that *changes* in crime are associated with population *changes*, Since crime rates increased for most cities in most years (but not all, and not during the early 1980s), the authors emphasize increases in crime and growth.

the same. Differences in the time period covered do not appear to drive the divergent empirical results.

The second key difference with Cullen and Levitt (1999) is that they include contemporaneous changes in crime rates in their regression rather than lagged changes in crime. To test whether this is the source of the difference in results, we re-estimate our models, regressing the change in city population as a function of changes in city crime over that same year. Results are reported in table 2.

[Table 2 here]

Looking across the first row of Table 2, the coefficient on the change in crime (when measured contemporaneously with city population growth) is now significantly *negative* in all three models. These results are consistent with Cullen and Levitt (1999, 1996); indeed even the magnitude of the coefficients is similar. Clearly, the difference between our findings and theirs is the temporal relationship between measured changes in crime and population changes. However, the new puzzle is why we should obtain such different results when lagging the measured changes in crime by just one year.

Measurement Error and Endogeneity

We argued previously that that there are theoretical reasons to focus on changes in crime rates that precede the location decisions they are presumed to affect. Theory does not predict, however, that changing the lag by one year should change the relationship between population and crime so dramatically. Rather, it seems likely that some estimation problems are driving this reversal in the sign of the coefficient on crime. One potential source of bias could be caused by the measurement error in city population. As noted, we use population estimates from FBI crime report data, which are estimated for intercensal years using birth and death records and CPS data. While measurement error in the dependent variable does not cause a bias, city population estimates are used in both the denominator of the dependent variable and the denominator of an independent variable (city crime rates), which can lead to a bias from the correlation in errors between the denominator of the dependent variable and the denominator of crime rates. In models employing contemporaneous changes in crime rates (like those presented in table 2), this bias is negative. However, in models with lagged changes, or levels at the start of the year, the bias will be positive.[16] Thus, this measurement error might explain the difference in results across Tables 1 and 2.

A second possible concern with city population appearing in the denominator of crime rates is that it may create a mechanical relationship between population and crime.[17] Specifically, when variables share terms (e.g., crime rates have population in the denominator, while population change effectively has population in the numerator), they are associated *by design*. If the population increases, the crime rate will decrease through a mathematical relationship rather than any behavioral changes.[18] If the number of crimes were independent of

[16] Specifically, we regress Ln population $_{t+1,}$ - Ln population $_t$ on #Crimes$_{,t}$/ population $_t$ and on [Crimes$_{,t}$/ population $_t$ - Crimes$_{,t-1}$/ population $_{t-1}$]. If population at time t is measured with error, the correlation in errors will be between the negative of pop$_t$ in the dependent variable, and pop$_t$ in the denominator of crime rates at time t, and will thus result in a positive bias. By contrast, if we regress Ln population $_{t+1,}$ - Ln population $_t$ on [Crimes$_{,t+1}$/ population $_{t+1}$ - Crimes$_{,t}$/ population $_t$], errors in measurement of pop$_t$ will cause a negative bias.

[17] This concern is similar to the ratio artifact issue raised by sociologists and criminologist in the crime deterrence literature, with respect to the negative association between arrest rates and crime rates (see Gibbs and Firebaugh, 1990)

[18] Ted Joyce has raised a similar concern, which we take to be in essence the reverse of his argument supporting abortion rates rather than number of abortions as the dependent variable in empirical work crime deterrence. See footnote 12, Joyce 2006

population size, then crime *rates* will be negatively correlated with population growth by construction.

A third possible concern is that the change in crime is endogenous to the growth in city population, for the reasons suggested above. We use several approaches to address these various sources of potential bias.

Different Specifications of Crime

One possible response to these estimation issues is to use a specification of crime on the right hand side that does not include population estimates. Specifically, estimating the relationship between population changes and the number of crimes would remove concerns about ratio bias. While convenient, crime rates are likely to be a more effective proxy for individuals' perceived levels of risk. Moreover, information about overall crime is usually reported per capita, and is publicly available in this form. However, for violent crimes, and homicides in particular, perceptions of safety may be more heavily affected by the number of reported incidents rather than rates. Awareness of homicides may come through media coverage of specific events, and news reports typically highlight the total number of homicides in a given year (rather than rates). Thus, the number of homicides might be a better measure of worries about crime than homicide rates.[19]

With this in mind, we estimate a model in which we 'unbundle' homicide rates. Specifically, rather than focusing on per capita murders, we control separately for population and the number of homicides:

[19] Note that homicides are also measured more accurately than other forms of crime, and thus by examining homicides, we also address measurement error in the number of reported crimes.

$$\Delta \ln population_{it\,(t+1)} = \alpha + \beta_1 NumberHomicides_{it} + \beta_2 Population_{i,t} + \gamma X_{it} + \delta_t + \lambda_r + \varepsilon_{it} \quad (3)$$

Table 3 summarizes our results. For comparison purposes, Table 3 also shows results of models using homicides rates. All controls from the previous models are included, but for brevity of presentation, we only report coefficients on the key variables and only show models with city fixed effects. The first two columns show that, similar to the results for crime rates, we find positive and significant coefficients on homicide rates.

[Table 3]

As for the coefficient on the number of homicides, it is not statistically different from zero in either model. Once we take population out of the denominator of homicides, in other words, we no longer see any positive correlation between crime and population growth. Moreover, the coefficient on population is significant and negative in both models, which is consistent with a correlation in errors in city population in the dependent and independent variables, which would help to explain both our positive coefficients on initial crime rates and also the difference between our coefficients on change in crime and those of Cullen and Levitt (1999), leading to an upward bias in our results and a downward bias in theirs.

Finally, as an additional attempt to remove error in measurement bias, we also experiment with models using longer lags on crime rates (say, crime changes between year t-1 and year t-2, and changes between year t-2 and year t-3). We consistently obtain insignificant coefficients. When crime changes do not include crime in year t, they do not appear significantly related to population growth between year t and year t+1.

Instrumental Variables Estimation

A second approach to addressing the issue of correlation in measurement error is an instrument strategy, which also helps to correct for endogeneity. We use the same instrument strategy employed by Cullen and Levitt (1999), namely to use data on the severity of state criminal justice systems to instrument for changes in crime rates. A valid instrument for crime should be correlated with crime but not otherwise affect city population. Here, the severity of the criminal justice system affects crime rates through deterrence or/and incapacitation, but should have no other effect on city populations. We proxy for the severity of the state system by using data on annual state-level commitment to prison (positively associated with severity) and annual releases from prison (negatively related). To control for state differences in crime rates, these annual flows are expressed *per crime reported in the same year*. Although these crime counts are for the entire state and they are used as instruments for city crime rates, this again raises the potential of correlation in errors between the dependent and independent variables, as city crimes are a component of state crimes, and therefore are a component of the numerator of an independent variable.[20] We attempt to address this by relying on lagged measures of our instrument. We instrument for both city crime rates and change in city crime rates.

Appendix Table B1 presents the results of our first stage estimates using lagged measures of commitments and releases, and the full set of controls included in the annual models previously estimated. We find the expected relationship between crime rates (and changes in crime rates) and our lagged measures of severity. Specifically, we find that all significant coefficients on releases are positive and all significant coefficients on admissions are negative.

[20] In addition, our instrument is based on all crimes reported in a state, while our city crime rates include only index crimes, and does not include lower level crimes such as many drug-related crimes.

In all models, these proxies are jointly significant at the .001 level or higher. That said, while validity tests suggest that the instruments in the crime level models are both strong instruments and exogenous, the validity test results for the change in crime models are somewhat less consistent.[21] Thus, 2SLS results for the crime change models should be interpreted with some caution.

[Table 4]

Results are presented in Table 4. Unlike our previous results (in which the coefficients on initial levels of crime rates are significantly positive), the coefficients on the instrumented initial levels of crime (Models 1 and 2) are no longer significant. As for the models including instrumented crime changes, we now obtain negative coefficient estimates, and one is statistically significant. When including city fixed effects, the coefficient remains negative but does not quite reach statistical significance.

We have also experimented with many robustness tests of these instrumented results. Appendix Table B2 summarizes these results for models with year and region fixed effects and for models with year and city fixed effects. We again divide the sample into large and small cities and into the two separate time periods and see little difference in the coefficients on changes in city crime. We also experiment with different specifications of crime. We separately test for effects of property and violent crime and include measures of suburban crime. We

[21] All of our models pass the standard test for weak instruments (whether F value of the first stage regression is above 10), except for the change in crime model with region and year fixed effects. We also employ three tests of exogeneity. The tests suggest that the instruments in the crime level models are exogenous. but the changes in crime level models do not pass all three exogeneity tests. Specifically, both change in crime models fail Hansen's J test, and the change in crime model with region and year fixed effects also fails the Greene (2008) exogeneity test. Note that this is not just a problem for our time period (Cullen and Levitt,1996).

generally find consistent results – the coefficients on crime are generally not significant, but where they are significant, they are negative. The coefficients on crime levels are insignificant in every regression, with the exception of the models estimated for the 1990s. For the 1990s, we do find that higher (lower) initial crime levels are associated with smaller (greater) subsequent growth.

In short, these instrumented results suggest that once we address the econometric problems (whether endogeneity of crime or correlation in errors), we find no evidence of a significantly positive relationship between crime rates and population growth, and some mixed evidence suggesting that larger increases (greater declines) in crime rates may depress (encourage) population growth. (Again, however, these crime change results should be interpreted with some caution given that instruments in these models do not pass all validity tests.) When modeling population changes as a function of lagged rather than contemporaneous changes in crime, there is limited evidence suggesting changes in city crime affect overall city population growth.

Finally, we return to our core question of whether increases or decreases in crime had greater effects. Specifically, we estimate spline models, which allow for separate estimates of the effects of crime increases and crime decreases. As shown in Table 5, the coefficients on the crime reductions are not significant. The coefficients on the crime increases are negative and one is nearly statistically significant, providing very weak evidence that population growth may be more sensitive to increases in crime. But none of these coefficients reaches conventional levels of significance, so there is little support here for any differential impacts. Differences in the strength of our findings relative to Cullen and Levitt (1999, 1996) do not arise from differential sensitivity to increases versus decreases in crime.

[Table 5]

Decade Model

A third way to address the measurement error is to estimate a model that only uses data from decennial census years, when population estimates are far more accurate. Of course, a decade model also has the advantage of more accurately measured control variables. Our basic regression model is as follows.

$$\Delta \ln \text{population}_{it\,(t+10)} = \alpha + \beta_1 \Delta\, \text{CityCrime}_{i\,(t-1),\,t+4} + \gamma X_{it} + \delta_t + \lambda_r + \varepsilon_{it} \quad (4)$$

$$\Delta \ln \text{population}_{it\,(t+10)} = \alpha + \beta_2 \text{CityCrime}_{it} + \gamma X_{it} + \delta_t + \lambda_r + \varepsilon_{it} \quad (5)$$

where subscripts i and r reference city i and region r. The dependent variable is the change in the log of city population over a decade (either the 1980s or the 1990s). The independent variables include changes in city crime rates over the first half of the decade and city crime rates at the start of the time period. (Note that with a decade models, including changes in crime that precede the start of the decade as an independent variable creates a very long lag between the change in crime and moves over the second half of the decade. Thus, we measure change in crime during the first four years of the decade.)[22]

X is a vector of characteristics of the city at the start of the period, which includes: the initial population, unemployment rate, median family income, the percentage of the city's

[22] We measure crime changes for the 1990s between 1989 and 1994. We do not have crime data for 1979, so we measure crime changes for the 1980s population growth as between 1980 and 1984.

population that is black, the percentage that is foreign born, the percentage with college degrees, and the percentage that own their homes. We also include measures of the age distribution of the population, the average temperatures in January and July, annual precipitation, and regional dummy variables to capture variation across the nine census regions. All baseline variables are measured at the start of the time period, to minimize endogeneity.

Table 6 shows our basic results. The coefficients on the non-crime variables generally have the expected signs. But none of the coefficients on crime levels or changes are statistically significant. We find no evidence here that crime rate levels or changes in crime affect subsequent population growth. This is consistent with Cullen and Levitt (1999). They too find an insignificant relationship between changes in crime and population changes in their decade model with city fixed effects (even when using contemporaneous measures of changes in crime).

[Table 6]

In sum, we use several alternative approaches to addressing the problem of measurement error and in all cases, we eliminate the positive coefficients on initial crime rates. When we use 2SLS, and also attempt to control for endogeneity, we find at best weak evidence that changes in crime contribute to overall city growth. We find little evidence that crime increases have any larger an effect on population changes than crime reductions. In the next section of the paper, we turn to the question of whether and how intra-metropolitan migration decisions are affected by crime.

IV. Do Reductions in Crime Rates Reverse Central City Flight?

The annual models of city population changes consider net changes in populations, and thus include some changes potentially less likely to be related to crime. Moreover, this aggregate measure combines the two key residential decisions that we think are likely related to crime – retention and attraction -- into a single net effect. Yet it is quite possible that decisions to remain in the central city are more/less sensitive to changes in crime than decisions to move into a central city from outside. Remaining in place entails no moving costs, while relocation is quite costly. We would expect that the level or decline in crime rates needed to retain a central city household might be quite different from that needed to induce a suburban household to incur the cost of moving. We turn next to these distinct channels of central city population change – retention of existing residents and attraction of new residents.

We model central city 'retention' and 'attraction' using city and metropolitan migration data from the decennial census, which collects data on current residence, whether a household has moved in the past five years, and information on a household's residence five years ago. Due to increased suppression of geographic data in the 2000 Census, however, we are unable to use these data for many of the central cities in our sample. Specifically, migration data for cities and metropolitan areas simply identifies whether a household that moved in the past five years resided in a central city in that metropolitan area five years previously. Many metropolitan areas include several central cities so we cannot know for sure whether households who previously resided in a central city actually lived in the same particular central city. We only know that they lived in a central city within the same metropolitan area. However, for 111 of our central cities (those that are the only central city within their metropolitan area), we can uniquely capture migration between the central city and suburbs. In particular, we know the number of people who lived in the city in 1995 and remained there through 2000. We also know the number of people who lived in the city in 1995 but had moved to the suburbs of that city by 2000.[23] Finally, we have information on new entrants to the central city, and whether they moved most recently from another part of the same metropolitan area. Together, these data permit us to estimate a series of models of central city population retention and attraction, focusing on the migration patterns most likely to be influenced by central city crime rates. [24]

Retention of City Residents

Our primary model focuses on the retention of central city residents over the 1995-2000 time period. Our dependent variable in this model is the share of 1995 residents who remained in the central city five years later.[25]

$$CCRetained_{it\,(t+5)} = \alpha + \beta_1 \Delta\, CityCrime_{i(t-3)\,t} + \gamma X_{it,} + \lambda_r + \varepsilon_i \quad (6)$$

$$CCRetained_{it\,(t+5)} = \alpha + \beta_1 CityCrime_{it} + \gamma X_{it,} + \lambda_r + \varepsilon_i \quad (7)$$

where subscripts i and r reference city i and region r, t is 1995. The independent variables include changes in city crime rates over the three preceding years (1992-1995) and city crime rates at the start of the time period (1995).[26] We include all independent variables used in our previous models of central city population change, together with some additional city characteristics available from the decennial census. Specifically, we include measures of

[23] Specifically, we know the number of people who lived in the city in 1995, but left the central city by 2000 and remained in the metropolitan area. Note, this undercounts movement to the suburbs, as we do not include residents who moved to the suburbs but left the MSA (or died) before 2000.

[24] The central cities in our migration sample comprise a somewhat larger share of the metropolitan population than was true in our full set of central cities in the annual model. To test whether such cities differ in other systematic ways, we estimated all models on a smaller sample of cities for which the share of population in the central city was limited to less than forty percent of the metropolitan area (just about the mean for cities in our annual sample), and found essentially the same results as for our full sample.

[25] The denominator of the dependent variable is city population estimated as the midpoint between the population reported in the 1990 and 2000 census rather than the 1995 estimate of city population used in calculating UCR crime rates. While this will contain measurement error, this error should be less highly correlated with error in the 1995 city population estimates in the city crime rate, although we get similar results using this alternative denominator.

[26] We have tried measures of cumulative change and different lagged time periods, with similar results.

industry mix in the city, homeownership rates, educational attainment of the population, and measures of city climate. We now measure the age distribution of the population at the city level. Our non-crime control variables are measured as closely to 1995 as feasible (for those variables only available in the decennial census, we use 1990 data). In some models, we also include the share of the MSA population residing in the central city in 1990, to control for the relative size of the central city in terms of residential options, differences in the share of MSA land within the city boundaries and historical differences in development patterns. Table 7 presents the results.

[Table 7 here]

The first two columns of Table 7 present results for models using lagged changes in crime, and the last two for those using 1995 crime levels. While crime has a negative coefficient when measured in levels, in no model is the crime coefficient significant. Cross-sectionally, central cities with declining or lower crime rates in 1995 were not significantly more likely to retain residents over the next five years. We get the same results if we also include measures of suburban crime.

As noted previously, not all residential decisions –even stages of decisions—may be equally affected by a city's crime rate. Surely, we would expect declines in crime rates to have a greater impact on the residential decisions of households who are committed to remaining within a metropolitan area than on those considering leaving the metropolitan area altogether. For while out migration from a central city to an entirely different metropolitan area may primarily be driven by larger economic and labor market factors, we expect that movements from the central city to the suburbs may be more significantly affected by local public services and amenities.[27] Indeed, the bulk of literature on flight focuses on such intra metropolitan migration. To consider this more directly, our second model of retention focuses on within metropolitan area moves, the retention of central city residents who choose to remain somewhere in the MSA over this five year period.

$$CCRetained_{it\,(t+5)}\ (AmongMSAStayers) = \alpha + \beta_1 \Delta\ CityCrime_{i(t-3)\,t} + \gamma X_{it,} + \lambda_r + \varepsilon_i \quad (8)$$

$$CCRetained_{it\,(t+5)}\ (AmongMSAStayers) = \alpha + \beta_1 CityCrime_{it} + \gamma X_{it,} + \lambda_r + \varepsilon_i \quad (9)$$

In this case, our dependent variable is the ratio of the number of city residents in 1995 who were still in the central city in 2000 to the number of people who lived in the central city in 1995 and lived somewhere in the metropolitan area in 2000 (either city or suburb).[28] Estimates from the 2000 CPS on moves during the last year of our time period suggest that nearly half of moves that originate from a central city end within the same metropolitan area.[29]

Our final model of retention considers the subset of 1995 residents who chose to remain in the MSA but who change housing units between 1995 and 2000. The residential choice literature has long argued that household location decision may well have two components: choosing to move (change housing) and then deciding where to locate.[30] There is an extensive literature suggesting that while these decision stages are inter-related, the factors affecting the first stage may differ from those factors affecting the second (Kim et al, 2005; Quigley and Weinberg 1977, for summaries). In essence, these models suggest a two (or three) stage process

[27] Similarly, we might expect that nearby suburban residents, facing the lowest relocation costs and having the greatest local knowledge, would be more responsive to decreases in crime rates.
[28] This is very similar to Frey (1979), at least in terms of the denominator.
[29] For our sample, 90 percent of central city residents in 1995 remain somewhere in the MSA as of 2000.
[30] For example, Knapp et al (2001) considers the MSA retention decision with a two-stage choice process.

in which the decision to move *at all* precedes a decision on where to locate. In the second stage, a household might decide on a metropolitan area to live in, while in the third stage, a household will decide where to live within the metropolitan area. It is the final stage of this process where location attributes should matter most. Our final retention model focuses on this final decision. Specifically, we estimate the share of central city residents retained among 1995 central city residents who changed houses and remained in the metropolitan area.

$$CCMoversRetained_{it\,(t+5)}\ (AmongMSAStayers) = \alpha + \beta_1 \Delta\ CityCrime_{i(t-3)\,t} + \gamma X_{it,} + \lambda_r + \varepsilon_i \quad (10)$$

$$CCMoversRetained_{it\,(t+5)}\ (AmongMSAStayers) = \alpha + \beta_1 CityCrime_{it} + \gamma X_{it,} + \lambda_r + \varepsilon_i \quad (11)$$

Table 8 presents results for these modified retention models. The top panel summarizes retention model results for our first subsample, central city residents who remained in the metropolitan area five years later. The bottom panel presents results for the second subsample, central city residents who remained in the metropolitan area and changed housing over the five year time period. All models contain the same set of control variables employed previously, but for brevity those coefficients are not reported in the table.

[Table 8 here]

Here we do find evidence that crime rates matter. While the coefficients on lagged changes in crime continue to be insignificant, the coefficients on crime rates are significantly negative in three out of four of the models. Central cities with lower crime rates are more likely to retain residents among those who stay in the metropolitan area, and among this group, there is some evidence they differentially retain those who change housing during that time period. These last models and results are our most direct assessment of crime and 'flight'. They suggest that the reduced crime rates achieved in recent years in some cities may have helped to abate flight to the suburbs.

To get a sense of the magnitude of these effects, cities with crime levels a standard deviation below the mean in 1995 retained between 1.4 and 2.5 percent more of those residents remaining in the MSA over the next five year, for an overall increase of population between 1.3 and 2.2 percent. As a point of comparison, the coefficients on crime levels in our instrumented model for the 1990s (Appendix Table B2), suggest that crime levels a standard deviation below the mean would result in population increases between 1.7 and 3.1 percent.

Attraction of New Residents to the City

Finally, we consider a second channel through which central city population levels may be affected by lower crime rates, via new entrants. We estimate a basic model of new entrant attraction to cities, where the dependent variable is the ratio of the number of people who moved to the central city between 1995 and 2000 to total city population in 1995. Specifically,

$$CCAttraction_{it\,(t+5)} = \alpha + \beta_1 \Delta\, CityCrime_{i(t-3)\,t} + \gamma X_{it,} + \lambda_r + \varepsilon_i \quad (12)$$

$$CCAttraction_{it\,(t+5)} = \alpha + \beta_1 CityCrime_{it} + \gamma X_{it,} + \lambda_r + \varepsilon_i \quad (13)$$

As noted previously, new entrants coming from farther distances may differ in the ways in which local attributes attract them, either due to differential moving costs or differences in information. Thus, we also attempt to explain intra-metropolitan relocation from the suburbs to the city, the ‘reverse’ of flight, as a function of city crime rates, estimating our central city attraction model on the subsample of households who were already in the MSA prior to 2000.

In none of these models is any coefficient on crime or crime change significant. (For brevity, the table is not included).[31] While our retention models suggest some ability of lower crime rates to retain households, we find no evidence that low or declining crime rates have helped to attract new residents, either from within the MSA or outside.

V. Conclusion

We began this paper with an interest in assessing whether the decline in crime rates in the 1990s had a positive impact on city growth. Our preliminary results suggested that in aggregate, they did not (and indeed may have depressed growth). However, those results suffered from multiple estimation issues, most notably measurement error bias and the endogeneity of crime rates. When we focused on the number of homicides (controlling for population separately) or estimated decade models of population growth, we found no evidence of positive association. Further, when we instrumented for total crime rates, previous findings of a positive association were eliminated, and we found some weak evidence of a significantly negative relationship between changes in crime and city population changes. The only time period for which we found evidence of a relationship between crime levels and population changes was for the 1990s, when crime levels were at their lowest. Our interpretation of these results is that crime, whether measured in levels or changes, plays only a small role in aggregate population changes for cities.

Results from our city migration models suggest that to the extent that crime does affect location decisions, it is through influencing intra-metropolitan decisions. Specifically, while

[31] This is consistent with Cullen and Levitt (1996) who find the crime matters for whether people leave cities but has little effect on whether they move in from the surrounding suburbs.

lower crime rates do not appear to enable cities to attract new residents as a result (either from the surrounding suburbs or from beyond the metro area) they do help cities retain a larger share of those residents who remain within the MSA. Lower crime rates, that is, appear to abate flight, although not actually to reverse it.

Figure 1: Crime rate per 1000 residents, 1980-2000, 145 cities

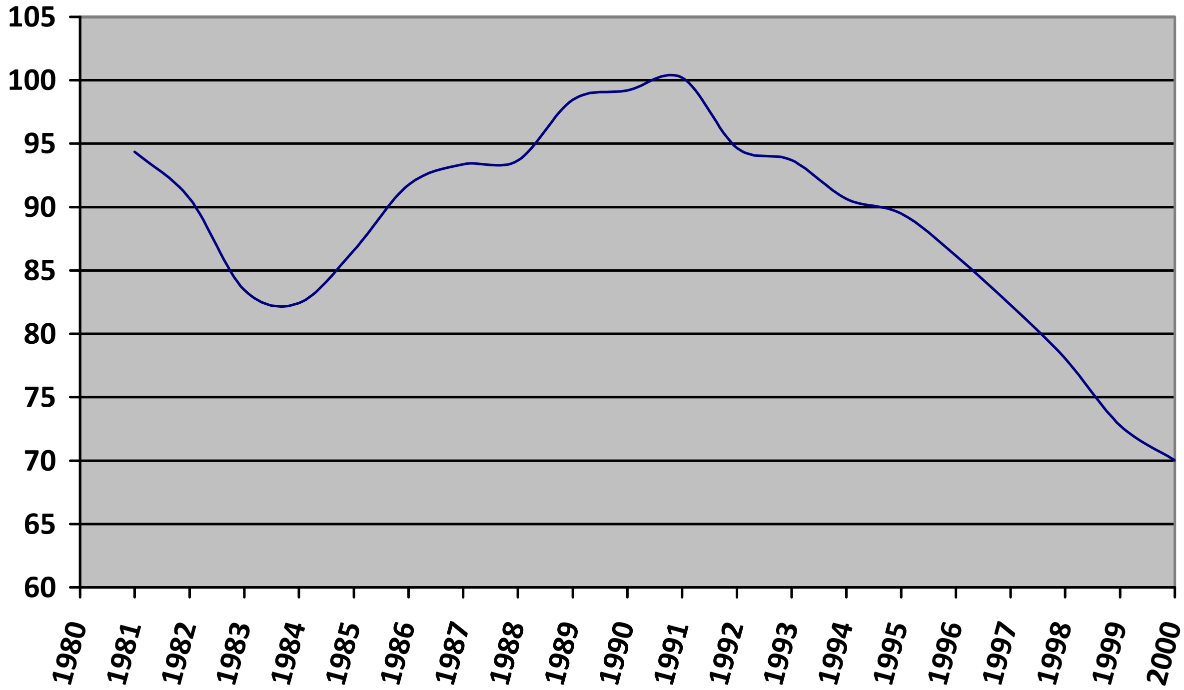

Table 1: Modeling City Growth
Dependent Variable: Change in Log City Population, t, t+1

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
ΔCrime, t-1, t	0.064	0.060	0.033			
	(0.061)	(0.060)	(0.060)			
Crime rate, t				0.043*	0.147***	0.174***
				(0.023)	(0.040)	(0.042)
MSA Unemp, t	0.000	-0.001***	-0.001	0.000	-0.002***	-0.001
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Median Income, t	0.005***	0.003	0.000	0.005***	0.004*	0.002
	(0.001)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)
Pct Black, t	-0.019***	-0.103**	-0.086**	-0.022***	-0.117***	-0.101***
	(0.004)	(0.042)	(0.042)	(0.004)	(0.039)	(0.038)
Pct 0to17, t	-0.047	-0.784***	-0.682**	-0.028	-0.795***	-0.755***
	(0.060)	(0.208)	(0.268)	(0.060)	(0.180)	(0.237)
Pct 18to24, t	0.176**	-0.446**	-0.215	0.215**	-0.549***	-0.223
	(0.086)	(0.204)	(0.270)	(0.087)	(0.184)	(0.244)
Pct 25to44, t	-0.070*	-0.554**	-1.017***	-0.040	-0.651***	-0.972***
	(0.041)	(0.241)	(0.302)	(0.043)	(0.212)	(0.261)
Pct 45to64, t	0.007	-0.607**	-0.310	0.036	-0.654***	-0.279
	(0.104)	(0.243)	(0.294)	(0.104)	(0.209)	(0.262)
Pct Foreign, t	0.005	0.038	0.048	0.007	0.047	0.080*
	(0.006)	(0.044)	(0.045)	(0.006)	(0.040)	(0.041)
Pct Chg State Pop, t	0.392***			0.385***		
	(0.0604)			(0.0607)		
Year Fixed Effects	Yes	Yes		Yes	Yes	
Region Fixed Effects	Yes			Yes		
City Fixed Effects		Yes	Yes		Yes	Yes
Region-Year Fixed Effects			Yes			Yes
Observations	2453	2453	2453	2483	2623	2623
Adjusted R-squared	0.213	0.253	0.325	0.213	0.269	0.338

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

All models include intercept and are estimated using weighted least squares, weighted by 1990 city population.

Table 2: Modeling City Growth as Function of Contemporaneous Crime Changes
Dependent Variable: Change in Log City Population, t, t+1

	Model 1	Model 2	Model 3
ΔCrime, t, t+1	-0.657***	-0.654***	-0.601***
	(0.059)	(0.055)	(0.056)
MSA Unemp, t	0.000	-0.001***	-0.001
	(0.000)	(0.000)	(0.001)
Median Income, t	0.005***	0.005*	0.002
	(0.001)	(0.002)	(0.003)
Pct Black, t	-0.019***	-0.103***	-0.086**
	(0.004)	(0.037)	(0.037)
Pct 0to17, t	-0.023	-0.702***	-0.676***
	(0.058)	(0.171)	(0.226)
Pct 18to24, t	0.256***	-0.388**	-0.073
	(0.083)	(0.175)	(0.233)
Pct 25to44, t	-0.048	-0.497**	-0.907***
	(0.039)	(0.201)	(0.250)
Pct 45to64, t	0.053	-0.502**	-0.222
	(0.100)	(0.199)	(0.251)
Pct Foreign, t	0.005	0.0201	0.054
	(0.006)	(0.038)	(0.039)
Pct Chg State Pop, t	0.379***		
	(0.058)		
Year Fixed Effects	Yes	Yes	
Region Fixed Effects	Yes		
City Fixed Effects		Yes	Yes
Region-Year Effects			Yes
Observations	2457	2597	2597
Adjusted R-squared	0.252	0.309	0.376

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

All models include intercept and are estimated using weighted least squares, weighted by 1990 city population.

Table 3: Modeling City Growth as Function of Homicides
Dependent Variable: Change in Log City Population, t, t+1

	Model 1	Model 2	Model 3	Model 4
Homicide rate, t	0.023***	0.026***		
	(0.008)	(0.008)		
# Homicide, t			-0.081	-0.064
			(0.276)	(0.269)
City population, t			-0.020***	-0.018***
			(0.003)	(0.003)
Year Fixed Effects	Yes		Yes	
Region Fixed Effects				
City Fixed Effects	Yes	Yes	Yes	Yes
Region-Year Effects		Yes		Yes
Observations	2675	2675	2675	2675
Adjusted R-squared	0.268	0.335	0.281	0.344

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Not displayed are the same covariates listed in Tables 1 and 2: MSA unemployment, city median income (interpolated annually using state change rates and city levels at the start and beginning of the decade), percent black (linear interpolation), percent foreign (linear interpolation), and state level age categories – all measured as levels in year t. In models without city fixed effects, state level changes in state population from t to t-1 are also included. All models include intercept and are estimated using weighted least squares, weighted by 1990 city population.

Table 4: Modeling Urban Growth, 2SLS
Dependent Variable: Change in Log City Population, t, t+1

	Model 1	Model 2	Model 3	Model 4
Crime rate, t	-0.261	-0.258		
	(0.169)	(0.215)		
ΔCrime, t-1, t			-1.797*	-1.228
			(0.924)	(0.810)
Year Fixed Effects	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes		Yes	
City Fixed Effects		Yes		Yes
Observations	2343	2483	2175	2175
R-squared	0.150	0.222	0.000	0.103

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Not displayed are the same covariates listed in Tables 1 and 2: MSA unemployment, city median income (interpolated annually using state change rates and city levels at the start and beginning of the decade), percent black (linear interpolation), percent foreign (linear interpolation), and state level age categories – all measured as levels in year t in both the first and second stages. In models without city fixed effects, state level changes in state population from t to t-1 are also included. All models include intercept and are estimated using weighted least squares, weighted by 1990 city population.

Table 5: Modeling Urban Growth as a function of positive and negative crime changes[32], 2SLS
Dependent Variable: Change in Log City Population, t, t+1

	Model 1	Model 2
Positive Crime Chg t,t-1	-10.660	-8.347
	(6.779)	(7.784)
Negative Crime Chg t,t-1	3.171	2.186
	(5.658)	(6.402)
Year Fixed Effects	Yes	Yes
Region Fixed Effects	Yes	
City Fixed Effects		Yes
Observations	2265	2265
Adjusted R-squared	0.091	0.127

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Not displayed are the same covariates listed in Tables 1 and 2: MSA unemployment, city median income (interpolated annually using state change rates and city levels at the start and beginning of the decade), percent black (linear interpolation), percent foreign (linear interpolation), and state level age categories – all measured as levels in year t in both the first and second stages. In models without city fixed effects, state level changes in state population from t to t-1 are also included. All models include intercept and are estimated using weighted least squares, weighted by 1990 city population.

[32] This model has two first stage equations – one with positive crime changes(t) as the dependent variable, the other with negative crime changes(t) as the dependent. Each IV is then entered into the second stage.

Table 6: Modeling Decennial City Growth as Function of Early Decade Crime Changes
Dependent Variable: Change in Log City Population, t, t+10

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
ΔCity crime, t-1, t+4	0.204	-0.124	-0.360			
	(0.494)	(0.586)	(0.605)			
City crime Rate, t				0.305	0.513	0.573
				(0.347)	(0.802)	(0.842)
Unemployment, t	-1.401***	-0.296	-0.321	-1.464***	-0.381	-0.310
	(0.483)	(0.854)	(1.182)	(0.485)	(0.880)	(1.162)
Med Income (10000s), t	-0.001	-0.017	0.010	0.002	-0.014	0.014
	(0.026)	(0.032)	(0.036)	(0.026)	(0.031)	(0.036)
Pct Black, t	-0.167**	-0.105	-0.185	-0.179**	-0.212	-0.301
	(0.082)	(0.575)	(0.617)	(0.083)	(0.559)	(0.601)
Pct Age 0 to 17, t	1.111***	-1.567	-0.163	1.212***	-1.498	-0.196
	(0.402)	(1.304)	(1.539)	(0.413)	(1.293)	(1.527)
Pct Age 18 to 24, t	0.726	-2.769*	-1.787	0.805*	-2.871*	-1.811
	(0.446)	(1.518)	(1.835)	(0.454)	(1.517)	(1.826)
Pct Age 25 to 44, t	1.349***	0.540	0.578	1.383***	0.362	0.471
	(0.499)	(1.269)	(1.377)	(0.500)	(1.252)	(1.355)
Pct Age 45 to 64, t	0.814	0.254	0.291	1.013	0.373	0.337
	(0.776)	(1.600)	(1.630)	(0.795)	(1.587)	(1.615)
Pct manufacturing, t	-0.097	-0.170	-0.385	-0.083	-0.258	-0.455
	(0.136)	(0.369)	(0.551)	(0.135)	(0.361)	(0.538)
Pct College or more, t	-0.179	-0.518	-0.229	-0.163	-0.394	-0.180
	(0.176)	(0.828)	(0.884)	(0.174)	(0.796)	(0.850)
Pct homeowners, t	-0.055	0.802**	1.011**	-0.027	0.787**	0.991**
	(0.149)	(0.391)	(0.432)	(0.149)	(0.382)	(0.424)
Pct foreign, t	0.075	-0.578	-0.544	0.039	-0.598	-0.508
	(0.120)	(0.483)	(0.567)	(0.119)	(0.468)	(0.542)
Region effects	Yes			Yes		
Year effects	Yes	Yes		Yes	Yes	
City effects		Yes	Yes		Yes	Yes
Region-year interactions			Yes			Yes
Observations	287	287	287	289	289	289
Adjusted R-squared	0.390	0.538	0.553	0.391	0.541	0.557

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Modeling Central City Retention
Dependent Variable: Share of 1995 city residents remaining in the city in 2000

	Model 1	Model 2	Model 3	Model 4
ΔCrime, 1992-1995	0.002	-0.039		
	(0.336)	(0.282)		
Crime rate, 1995			-0.305	-0.122
			(0.201)	(0.174)
Unemployment, 1990	0.000	0.002	0.001	0.002
	(0.003)	(0.003)	(0.003)	(0.003)
Median family income, 1990	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Percent black, 1990	0.048	0.100***	0.059*	0.103***
	(0.031)	(0.027)	(0.031)	(0.027)
Percent 0 to 17, 1990	0.275	0.002	0.260	0.003
	(0.200)	(0.174)	(0.197)	(0.173)
Percent 18 to 24, 1990	-1.203***	-0.974***	-1.211***	-0.988***
	(0.205)	(0.177)	(0.195)	(0.170)
Percent 25 to 44, 1990	-0.292	0.115	-0.163	0.152
	(0.233)	(0.207)	(0.239)	(0.210)
Percent 45 to 64, 1990	0.161	-0.128	0.081	-0.154
	(0.243)	(0.210)	(0.246)	(0.212)
Percent manufacturing, 1990	-0.127	-0.015	-0.140	-0.021
	(0.105)	(0.090)	(0.103)	(0.090)
Percent homeowner, 1990	0.169**	0.200***	0.160**	0.194***
	(0.077)	(0.065)	(0.074)	(0.063)
Percent BA or higher, 1990	0.059	-0.004	0.047	-0.004
	(0.117)	(0.099)	(0.112)	(0.095)
Avg Jan temp, 1994	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Avg July temp, 1994	0.002	0.001	0.002	0.001
	(0.002)	(0.001)	(0.002)	(0.001)
Annual precipitation, 2000	0.000	0.000	0.001	0.000
	(0.001)	(0.000)	(0.001)	(0.000)
Percent foreign, 1990	0.369***	0.218**	0.340***	0.210**
	(0.101)	(0.088)	(0.101)	(0.089)
%of MSApopinCC, 1990		0.116***		0.113***
		(0.019)		(0.019)
Observations	111	111	111	111
R-squared	0.799	0.846	0.785	0.846

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
All models include intercept, controls for region of the country, and are estimated using weighted least squares, weighted by 1990 city population.

Table 8: Modeling Central City Retention
Dependent Variable: Share of 1995 city residents remaining in the city in 2000
Restricted Sample: 1995 city residents who remained in MSA in 2000

	Model 1	Model 2	Model 3	Model 4
ΔCity crime, 1992-95	-0.054	-0.188		
	(0.831)	(0.562)		
City crime, 1995			-1.190**	-0.585*
			(0.489)	(0.341)
City share of MSA pop		0.388***		0.376***
		(0.038)		(0.038)
Observations	111	111	111	111
R-squared	0.530	0.771	0.565	0.779

Dependent Variable: Share of 1995 city residents remaining in the city in 2000
Restricted sample: 1995 city residents who remained in MSA in 2000 and who changed housing units between 1995 and 2000

	Model 1	Model 2	Model 3	Model 4
ΔCity crime, 1992-95	-0.095	-0.320		
	(1.280)	(0.761)		
City crime, 1995			-1.660**	-0.636
			(0.757)	(0.465)
City share of MSA pop		0.650***		0.637***
		0.051		0.052
Observations	111	111	111	111
R-squared	0.601	0.848	0.625	0.851

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

All models include intercept and are estimated using weighted least squares, weighted by 1990 city population. As with the models in table 7, covariates include MSA unemployment, city median income, percent black, percent foreign, age categories, percent manufacturing, percent homeowners, and the percent with a college degree – all measured using 1990 Census. Additionally, the average daily temperatures in January and July are included, as well as annual precipitation, all measured in 1994. Region controls are also included, with the south as the reference category.

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Appendix A: Data sources and Variables

Uniform Crime Reports of the United States (Federal Bureau of Investigation)
This is the source of annual city and county crime rates, from 1980 through 2001. Our crime rates are based on Index I crimes: violent crimes (murder, rape, robbery and aggravated assault) and property crimes (burglary, larceny and auto theft). We use three measures of crime, total (simple total across all seven categories), violent and property. Our suburban crime rates are calculated as the metropolitan area crime minus the central city crime. We use constant MSA boundaries based on the 2000 definitions, and create these rates based on county data. The UCR includes city population estimates, provided by the census, which are used in the annual model. 1995 city population estimates are also used in the city and individual-level migration models.

Decennial Census
1980, 1990 and 2000 city level data was collected on city population, percent manufacturing, median family income, density, percent black, age distribution of the population, educational attainment of population, and homeownership rates. 1990 data were used as controls in city and individual-level migration models. Annual measures of the percent of city population that is black are a simple linear interpolation between each decennial census.

2000 Public Use Micro Sample (via IPUMS)
As available through the Minnesota Population center, http://usa.ipums.org/usa/.
The 2000 PUMS is a five percent sample of the U.S. population. Our data is drawn from the half of that sample that provides information on residential location five years ago. For a limited number of metropolitan areas, it is possible to identify everyone in the sample who resided in that metropolitan area in 1995, and in 2000. These data form the basis of our individual models of intra-metropolitan location. In addition to MSA of residence, for people in this sample we also know whether they reside in the central city in 2000, and whether they have changed housing units over the past five years. We also have standard data on the demographics, education, etc. of the head of household, and data on household structure and composition.

Bureau of Justice Statistics
Provides annual data on state-level prison population, including prison commitments and releases.

Bureau of Labor Statistics
1980 through 2000 metropolitan area unemployment rates were based on BLS estimates.

Statistical Abstract of the U.S.
The source of 1990 city level measures for temperature and precipitation, used in the city and individual migration models. This is also the primary source of state level data on per-capita income, state population and state age distributions, which was generously provided by Steven Raphael and Rucker Johnson.

Appendix Table A1: Descriptive Statistics for Annual Sample

Variable	N	Mean	Std. Dev
Total population, 2000	145	414027.3	779667.0
Total population, 1990	145	383093.8	716722.6
Total population, 1980	145	362484.0	691007.5
Change in ln population	2731	0.006	0.035
City total crime per capita	2821	0.090	0.026
City total crime change	2645	-0.001	0.008
State prison releases per crime	2900	1.649	2.527
State prison admissions per crime	2900	1.835	2.678
Change in prison releases per crime	2755	0.170	0.452
Change in prison admissions per crime	2755	0.173	0.420
MSA Unemployment rate	2852	6.131	2.574
Percent black	2900	0.229	0.178
City Median Income / 10000	2900	3.022	0.886
State age 0 to 17	2900	0.263	0.021
State age 18 to 24	2900	0.111	0.015
State age 25 to 44	2900	0.311	0.021
State age 45 to 64	2900	0.193	0.014
State age 65+	2900	0.122	0.021
Foreign	2900	0.099	0.108
Percent change state population	2755	0.011	0.011
Suburb total crime per capita	2811	0.043	0.014
Change in suburb total crime	2657	-0.001	0.005
# Murders/10000	2879	0.008	0.018
Murder rate*1000 people	2877	0.158	0.125
Change in # Murders/10000	2719	-0.0002	0.003
Change in murder rate*1000 people	2715	-0.002	0.049

Appendix Table A2: Descriptive Statistics for Migration Sample

Variable	N	Mean	Std. Dev.
CC Retention Model 1 (# Stayed in CC 1995-2000 / 1995 pop (interpolated from Census)	111	0.724	0.055
CC Retention Model 2 (# Stayed in CC 1995-2000 / (# Stayed in CC + Moved to Suburbs 1995-2000))	111	0.806	0.108
CC Retention Model 3 (# Changed Homes w/in CC 1995-2000 / (# Changed Homes w/in CC 1995-2000 + # Moved to the Suburbs 1995-2000))	111	0.608	0.158
Crime change, 1995-2000	109	-0.032	0.041
Crime change, 1992-1995	106	-0.004	0.013
Crime change, 1990-1995	107	-0.013	0.028
Crime rate, 1995	106	0.089	0.024
Suburban crime change, 1995-2000	106	-0.006	0.009
Suburban crime change, 1992-1995	106	-0.006	0.011
Suburban crime change, 1990-1995	106	-0.007	0.011
Unemployment, 1990	111	0.078	0.028
Median family income (1000s), 1990	111	31.5	5.8
Percent black, 1990	111	0.232	0.183
Percent 0 to 17, 1990	111	0.237	0.042
Percent 18 to 24, 1990	111	0.118	0.032
Percent 25 to 44, 1990	111	0.313	0.032
Percent 45 to 64, 1990	111	0.161	0.025
Percent manufacturing, 1990	111	0.149	0.057
Percent BA or higher, 1990	111	0.216	0.078
Percent homeowner, 1990	111	0.518	0.086
Avg July temp, 1994	111	76.4	5.0
Avg Jan temp, 1994	111	34.5	11.4
Annual precipitation, 2000	111	35.8	14.0
Percent Foreign, 1990	111	0.071	0.072
Central city share of MSA, 1990	111	0.430	0.208

Appendix Table A3: Descriptive Statistics for Decennial Panel Sample

Variable	N	Mean	Std. Dev.
Ln population change, t,t+1	290	0.066	0.160
Crime change, 80-84 and 89-94	287	-0.010	0.017
Crime rate, t	289	0.097	0.026
ln(population), t	290	12.374	0.772
Unemployment, t	290	0.074	0.028
Median family income, t	290	23636.9	9631.8
Percent black, t	290	0.222	0.173
Percent 0 to 17, t	290	0.254	0.036
Percent 18 to 24, t	290	0.140	0.036
Percent 25 to 44, t	290	0.306	0.041
Percent 45 to 64, t	290	0.179	0.023
Percent manufacturing, t	290	0.176	0.077
Percent BA or higher, t	290	0.212	0.090
Percent homeowner, t	290	0.546	0.104
Percent foreign, t	290	0.086	0.100

Appendix B: Instrumental Variables Results

Appendix Table B1: First Stage Results for Base 2SLS Specification

Dependent Variables:	ΔCrime t,t-1	ΔCrime t,t-1	Crime t	Crime t
ΔPrisAdm/Crime t-1,t-2	-0.003***	-0.003***		
	(0.001)	(0.001)		
ΔPrisRel/Crime t-1,t-2	0.004***	0.005***		
	(0.001)	(0.001)		
ΔPrisAdm/Crime t-2,t-3	-0.002	-0.002		
	(0.001)	(0.002)		
ΔPrisRel/Crime t-2,t-3	0.000	0.001		
	(0.001)	(0.001)		
PrisAdm/Crime t,t-1			-0.008***	-0.006***
			(0.003)	(0.002)
PrisAdm/Crime t-1,t-2			-0.005*	-0.005***
			(0.003)	(0.002)
PrisRel/Crime t,t-1			0.007**	0.007***
			(0.003)	(0.002)
PrisRel/Crime t-1,t-2			0.006**	0.003**
			(0.003)	(0.002)
Year Fixed Effects	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes		Yes	
City Fixed Effects		Yes		Yes
Region-Year Effects				
Observations	2190	2190	2493	2493
R-squared	0.177	0.203	0.405	0.829

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Not displayed are the same covariates listed in Tables 1 and 2: MSA unemployment, city median income (interpolated annually using state change rates and city levels at the start and beginning of the decade), percent black (linear interpolation), percent foreign (linear interpolation), and state level age categories – all measured as levels in year t in both the first and second stages. In models without city fixed effects, state level changes in state population from t to t-1 are also included. All models include intercept and are estimated using weighted least squares, weighted by 1990 city population.

Appendix Table B2: Modeling City Growth: Robustness Checks, 2SLS
Dependent Variable: Change in Log City Population, t, t+1

	Base specification		Small city sample		Large city sample		1980 sample		1990 sample		Property crime only		Violent crime only		With Suburban crime controls	
Δ Crime t,t-1	-1.797*	-1.228	-1.881	-1.456	0.170	0.725	-0.794	0.483	-1.259	-1.173	-2.070**	-1.462	-15.94	-14.76*	-1.364	-0.918
	(0.924)	(0.810)	(1.185)	(1.074)	(1.210)	(0.925)	(0.903)	(0.947)	(0.896)	(0.742)	(1.050)	(0.910)	(9.859)	(8.589)	(0.870)	(0.765)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FEs	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
City FEs		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes
Crime, t	-0.261	-0.258	-0.318	-0.310	-0.060	-0.342	-0.143	-0.068	-0.63**	-1.16**	-0.060	-0.143	1.216	-9.840	-0.257	-0.260
	(0.169)	(0.215)	(0.295)	(0.322)	(0.154)	(0.252)	(0.258)	(0.239)	(0.281)	(0.510)	(0.278)	(0.381)	(4.421)	(8.720)	(0.162)	(0.252)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FEs	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
City Fes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes

Each model includes one measure of crime, with models in the top panel using change in crime, and those in the bottom, crime levels. For brevity we only report coefficients for crime variables. Not displayed are the same covariates listed in Tables 4: MSA unemployment, city median income (interpolated annually using state change rates and city levels at the start and beginning of the decade), percent black (linear interpolation), percent foreign (linear interpolation), and state level age categories – all measured as levels in year t in both the first and second stages. In models without city fixed effects, state level changes in state population from t to t-1 are also included. All models include intercept and are estimated using weighted least squares, weighted by 1990 city population.