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

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Intro

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 *increases* in crime rates 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 consider the effect of crime on overall changes in city population, as well as 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. We attempt 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.

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.

Several authors have considered the impact of city crime levels on rates of suburbanization, using density gradients as a proxy for suburbanization. Do we see flatter density gradients in a metropolitan area when the central city's crime rate is higher? Mills and Price (1984), for example, examine density gradients for between thirty-five and fifty-eight metropolitan areas between 1960 and 1970. They find no evidence that relative levels of central city crime has contributed to suburbanization in the U.S., after controlling for a variety of other city characteristics. They also estimate a variety of alternative models, with similarly unsupportive results.

Jordan et al (1998) also use density gradients to measure suburbanization. They examine the 1980s and find higher crime rates are associated with less suburbanization, counter to their expectations. They attribute this finding to the possible effects of heightened zoning and policies that might constrain suburbanization in higher crime areas, or to the tendency for the share of crimes actually reported in a city to go up as expenditures on safety increase.

Other authors use aggregate migration data from the census to explore impacts of crime levels on particular subsets of the population. Frey (1979), for instance, uses aggregate central city migration data on 39 large MSAs from 1967 to 1970, to study the mobility and migration decisions of white households.. While central city crime rates do not appear to affect the likelihood of moving, they do appear to have influenced the likelihood that white households would move to the suburbs, conditional on their changing homes. However, once other city characteristics were taken into account, he reports that the independent influence of crime is quite small.

Consistent with Frey, Grubb (1982) finds evidence of sensitivity to central city crime rates for white households only during the 1960s, particularly upper-income white households. However, he does not control for other measures of perceived urban decline.

Greenwood and Stock (1990) compare patterns of high- and low-income households. They estimate a structural model of intra-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. The authors find some limited support that crime levels affect population flows. High crime rates appear to have increased the movement of high-income households to the suburbs in the 1950s while decreasing the immigration of lower income households through to the 1970s.

South and Crowder (1997) is one of the few studies to use household panel data to study intra-metropolitan moves. They rely on the geo-coded files of the Panel Study of Income Dynamics (PSID) between 1979 and 1985, to examine annual intra-metropolitan mobility patterns of black and white household heads. They focus on moves from a census tract, either within a city, or across the city/suburb divide, with household data linked to both census tract characteristics, and city/suburb characteristics. They model mobility as a two-stage process, first considering the move out of a neighborhood (census tract) and then the choice of where to arrive.

They find that the ratio of central city violent crime rates to suburban rates matters, but only for black households. It increases both the likelihood that black households will move from their neighborhood, and the likelihood that they will move to the suburbs. The ratio of crime rates does not emerge as significant in any of the other regressions.

Taken together, the results of these various studies of the impact of city crime levels are quite mixed. One reason for the mixed evidence is that in addition to crime affecting residential decisions, there may be several channels through which these residential decisions -- and suburbanization in particular -- actually affect crime rates If models fail to correct for this reverse causality, their estimates are likely to be biased. Farley (1987) suggests several avenues of reverse causality – reasons, that is, why suburbanization may lead to higher crime rates in the central city. First, assuming 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 city crime rates will appear inflated. In addition, because migration to the suburbs is selective, the differences between the populations living in central cities and suburbs will be larger in areas with more suburbanization. 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.

Cullen and Levitt (1999) offer some of the most recent (and widely cited) work in this area. 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 population losses, and that white households, families with children, and those with greater education are more sensitive to changes in crime. In addition, the authors find that changes in crime rates have their greatest influence on relocation decisions within the metropolitan area. Perhaps most importantly, this work improves on existing 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 when crime rates declined in most (though not all) 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. It is quite possible that households react more quickly to increases in crime than to decreases. Risk-averse households may worry that crime may rise again soon. The media may cover increases in crime more heavily than decreases, and even with media coverage, Cities may also have a difficult time shedding their high-crime reputations.

Building on the work of Cullen and Levitt (1999), we structure our empirical work around two sets of questions. First, we examine whether reductions in central city crimes rates lead to overall population gains for cities. Second, we examine the effect of crime on city-to-suburb and suburb-to-city moves. Is there evidence that reductions in crime appear to reverse 'flight'—that is, do crime reductions increase the likelihood that central city households will remain in cities? Is there any evidence that reductions in crime help to attract nearby suburban residents?

II. Data

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. The appendix 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, these data only reveal net changes in city population, from all sources, including births, deaths and migration from outside the country, which are unlikely to be affected by city crime rates. Moreover, 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.

We use a different data set to examine the effect of crime on city to suburb and suburb to city migration decisions. Specifically, we rely on city-level migration data from the 2000 census for a subset of these cities – the 111 central cities that are the only central cities in their metropolitan areas. For these cities, the Census reports the number of residents in 2000 who were also city residents 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 the 'suburbs'). These data, together with estimates of city population in 1995, permit us to separately examine the role of crime in 'flight' and attraction. Unfortunately, there is not enough cross-state variation in these data to employ an instrument strategy..

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. After 1992, however, mean crime rates declined almost continuously. The most recent work examining urban flight (Cullen and Levitt, 1999) relies on data through 1993, and thus misses this period of significant decline.[1]

[Figure 1 here]

[1] 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.

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 note that 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 typically prompted by changes in a household's housing needs, such as family formation, child bearing, aging, the choice of which jurisdiction to settle in (and in particular, whether to choose the city or choose a jurisdiction outside) may well depend on the current assessment of 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. Cullen and Levitt primarily rely on contemporaneous changes in crime and population, as have some other researchers. 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. 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.

We begin with an annual model of city population growth.[2] The basic models are as follows:

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

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

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, which includes: the percentage of the city's population that is black, the percentage that is foreign born, the city median family income,[3] 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).[4] All baseline variables are measured at the start of the time period, to minimize endogeneity.

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

[2] This model is very similar in structure to Cullen and Levitt (1999).
[3] Also in keeping with Cullen and Levitt, we base percent black on the city level measures from the decennial census, and use a simple linear interpolation. We use this same method for intercensal estimates of median family income and percent foreign born.
[4] We drop the percentage change in the state population in the models that include city fixed effects.

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 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, 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, we have also estimated these models including similar measures of suburban crime, with essentially the same results.[5] We have also re-estimated the models in Table 1 with all the independent variables measured in difference form and again obtain the same results. When we stratify our sample by city size, we find essentially the same results in both large and small cities. 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. Where the coefficients on crime rates are significant, the coefficients are positive. Finally, we considered the possibility that crime matters differently in cities that face other challenges by interacting our crime variable

[5] 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, we obtain essentially the same results.

with unemployment rates (one set of models), with share of the population that is black (another set of models). Again, the key results remain the same.[6]

Despite the robustness of our findings, it seems implausible that higher crime rates are actually attracting residents. What is also puzzling is the large divergence in our results from the findings of Cullen and Levitt (1999), who use a very similar approach but find that larger increases in crime are significantly associated with lower rates of city growth. By contrast, we find that changes in crime have no discernible effect and that higher levels of crime are associated with higher rates of city growth. 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, their work ends in 1993, right as the dramatic decline in crime rates began. As noted previously, there are theoretical reasons to believe that increases in crime have larger impacts than decreases. With this in mind, we estimated 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.

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

[6] All results available from authors upon request.

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 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. The puzzle is why we should obtain such different results when changing the lag in 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. Thus, it seems likely that some estimation problems are driving this reversal in the sign of the coefficient on crime. One potential source of bias is 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 ratio bias. 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.[7] Thus, this measurement error might explain the reversal of 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.[8] Specifically, when ratio 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.[9] If the number of crimes were independent of population size, then indeed, crime *rates* will be negatively correlated with population growth by construction.

One possible response to these estimation issues is to estimate the relationship between population changes and the number of crimes itself, which would remove concerns about ratio bias. While convenient, we believe that 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 incidents rather than overall crime rates. Awareness of homicides may come through media

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[7] 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.

[8] 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)

[9] 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

coverage of specific events, and news reports typically highlight the total number of homicides in a given year (rather than rates). Thus, number of homicides might be a better measure of worries about crime than homicide rates.

With this in mind, and an eye towards limiting ratio bias, 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:

$$\Delta \ln \text{population}_{it\,(t+1)} = \alpha + \beta_1 \text{NumberHomicides}_{it} + \beta_2 \text{Population}_{i,t} + \gamma X_{it} + \delta_t + \lambda_r + \varepsilon_{it} \quad (2)$$

Given this is a panel model with city fixed effects, we are estimating the effect of deviations in the number of homicides within a city, controlling for current population. We also estimate models in which we control for the lagged change in the number of homicides. Table 3 summarizes our results.

[Table 3]

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 with respect to crime rates, we find positive and significant coefficients on homicide rates.

As for the coefficients on the number of homicides, neither is statistically different from zero. 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. (Specifically, when the reported population in year t is larger than the actual population, it will also be true that our calculated population change between year t+1 and year t will be smaller than the actual change.) So these results suggest that correlation of errors helps to explain the difference between our results and those of Cullen and Levitt (1999), leading to an upward bias in our results and a downward bias in theirs.

Instrumental Variables Estimation

A second approach to addressing the issue of ratio bias 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.[10] 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

[10] For a detailed discussion of the instrument, see Cullen and Levitt 1996.

variable.[11] 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 B 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. In all models, these proxies are jointly significant at the .001 level or higher. Table 4 presents results for the second stage models. We ran three versions of instrument tests, and two told us that the instruments were exogenous and good. The third test (Hansen's J test for overidentification) suggested that in some models, instruments were weak. [Table 4]

Unlike our previous results (in which the coefficients on initial levels of crime rates were significantly positive), the coefficients on the instrumented initial levels of crime (Models 3 and 4) are no longer significant. As for the models including instrumented crime changes, the first two models shows that the negative and significant association between contemporaneous changes in crime and changes in population persists, even when using 2SLS. This is consistent with Cullen and Levitt (1999). And when using 2SLS, our coefficients on instrumented, lagged changes in crime (Models 5 and 6) change dramatically. While the coefficients on these lagged changes were positive and insignificant in OLS, we now obtain negative coefficient estimates, and one is statistically significant while the other is nearly significant.

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

In short, 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 evidence

that elevated crime rates depress population growth. But our results do not suggest that crime is a major driver of aggregate population changes.

We also experimented with many robustness tests of these instrumented results. We again divided the sample into large and small cities and saw little change in the coefficients on city crime. We also experimented with different specifications of crime. We used longer lags, separately tested for effects of property and violent crime, and included measures of suburban crime.

Finally, we returned to our core question of whether increases or decreases in crime had greater effects. First, we ran separate models for the 1980s (a decade when crime generally rose) and the 1990s (a decade when crime generally declined), and obtained highly similar results. Second, we estimated spline models, in which we separately estimated the effects of crime increases and crime decreases. In both cases, we found weak evidence that changes in crime were negatively correlated with subsequent growth. It made no difference whether the changes were positive or negative.

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 not likely to be related to crime: births, deaths, and migration from abroad. 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. We turn next to these distinct channels of central city population change – retention and attraction.

Note in both these cases, it seems likely that the residential choices of households who remain within the same metropolitan area when they move would be more sensitive to crime. For example, while out migration from a central city to an entirely different metropolitan area may primarily be driven by larger economic and labor market factors, movements from the central city to the suburbs may be more significantly affected by local public services and amenities.[12]

The decennial census 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. For 111 of our central cities (those that are the only central city within their metropolitan area), we can consider several sources of central city population changes. Specifically, 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.[13] 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.

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[12] 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.
[13] 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.

a. Retention of City Residents

Our first 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.[14]

$$CCRetained_i = \alpha + \beta_1 CityCrime_i + \gamma X_{i,} + \lambda_r + \varepsilon_i \qquad (3)$$

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 industry mix in the city, homeownership rates, educational attainment of the population, and measures of city climate. 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). As for crime, we test two alternative measures of crime: (1) changes in crime from 1992 to 1995[15]; and (2) the 1995 crime rate. 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. This last control is not without problems. Table 5 presents the results.

[Table 5 here]

The first two columns of Table 5 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

[14] 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.
[15] We have tried measures of cumulative change and different lagged time periods, with similar results.

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 include measures of suburban crime. [16]

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 improve a city's ability to retain residents who might otherwise be drawn to nearby suburbs, more so than those considering leaving the metropolitan area altogether. Indeed, prior literature on flight was focused on the mirror image of this phenomena – flight of residents from cities to their suburbs. To consider this more directly, our second model of retention focuses on within metropolitan area moves, retention of central city residents who choose to remain somewhere in the MSA over this five year period.

$$CCRetained_i \text{ (AmongMSAStayers)} = \alpha + \beta_1 Crime_i + \gamma X_{i,} + \lambda_r + \varepsilon_i \quad (4)$$

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).[17] In contrast with our first model of retention, the denominator no longer includes people who left the metropolitan area between 1995 and 2000. Estimates from the 2000 CPS on moves during the

[16] 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 our 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.

[17] This is very similar to Frey (1979), at least in terms of the denominator.

last year of our time period suggest that nearly half of moves that originate from a central city end within the same metropolitan area.[18]

Our final model of retention considers a subset of 1995 residents who chose to remain in the MSA and changed 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.[19] There is an extensive literature suggesting that while these decision stages are inter-related, the factors affecting the decision to change housing units may differ from those factors affecting where to move (Kim et al, 2005; Quigley and Weinberg 1977, for summaries). In essence, these models suggest a two (or three) stage 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.

$$CCMoversRetained_i\ (AmongMSAStayers) = \alpha + \beta_1 CityCrime_i + \gamma X_{i,} + \lambda_r + \varepsilon_i \qquad (5)$$

Table 6 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 retention results for the second

18 Cullen and Levitt (1999) find about 40 % of their central city leavers go to the suburbs.
[19] For example, Knapp et al (2001) considers the MSA retention decision with a two-stage choice process.

subsample, central city residents who remained in the metropolitan area and changed housing over the five year time period.

[Table 6 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, suggest that the recent declines in crime rates have helped to abate suburban flight.

b. 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 two basic models of new entrant attraction to cities. In essence, the first considers the rate of in-movement between 1995-2000, measured as the ratio of the number of people who moved to the central city between 1995 and 2000 to total city population in 1995. As noted previously, new entrants coming from farther distances may differ in the ways in which local attributes attract them. Specifically, we would expect that crime rates may matter most for those coming from within the metropolitan area, relative to those coming from outside the metropolitan area. 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. We estimate both models using each of the measures of crime employed previously. We find no significant relationship between attraction to the city and our measures of city crime (for brevity, table not included). [20]

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), 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 weak evidence of a significantly negative relationship between changes in crime and population changes.

Results from our city migration models suggest that to the extent that crime affects location decisions, it is through influencing intra-metropolitan decisions. Specifically, while lower crime rates do not enable cities to attract new residents as a result (either from their surrounding suburbs or from beyond the metro area) they do help them to retain a larger share of those residents who remain within the MSA. While not supporting an actual reversal of flight, this latter finding is completely consistent with abating flight.

[20] 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.


Crime Rates 1980-2000:
Average for 151 US Cities
Crimes Per 100,000 People
1.2
1.15
1.1
1.05
1
0.95
0.9
0.85
0.8
0.75
0.7
0.65
0.6
0.55
0.5
1980
1985
1990
1995
2000
Total

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

VARIABLES	Model 1 chglnpop	Model 2 chglnpop	Model 3 chglnpop	Model 4 chglnpop	Model 5 chglnpop	Model 6 chglnpop
Change in crime, t-1, t	0.0638	0.0604	0.0329			
	(0.0605)	(0.0597)	(0.0601)			
Crime rate, t				0.0426*	0.147***	0.174***
				(0.0231)	(0.0400)	(0.0415)
lag_unemployment	-0.000311	-0.00128***	-0.000532	-0.000289	-0.00157***	-0.000515
	(0.000241)	(0.000408)	(0.000514)	(0.000242)	(0.000386)	(0.000481)
lag_income_cnl	0.00473***	0.00245	0.000268	0.00504***	0.00425*	0.00200
	(0.000914)	(0.00280)	(0.00290)	(0.000932)	(0.00251)	(0.00258)
lag_pctblack	-0.0189***	-0.103**	-0.0864**	-0.0222***	-0.117***	-0.101***
	(0.00386)	(0.0421)	(0.0419)	(0.00419)	(0.0387)	(0.0384)
lag_p0to17	-0.0474	-0.784***	-0.682**	-0.0284	-0.795***	-0.755***
	(0.0599)	(0.208)	(0.268)	(0.0602)	(0.180)	(0.237)
lag_p18to24	0.176**	-0.446**	-0.215	0.215**	-0.549***	-0.223
	(0.0862)	(0.204)	(0.270)	(0.0870)	(0.184)	(0.244)
lag_p25to44	-0.0698*	-0.554**	-1.017***	-0.0400	-0.651***	-0.972***
	(0.0410)	(0.241)	(0.302)	(0.0428)	(0.212)	(0.261)
lag_p45to64	0.00662	-0.607**	-0.310	0.0356	-0.654***	-0.279
	(0.104)	(0.243)	(0.294)	(0.104)	(0.209)	(0.262)
lag_foreign	0.00446	0.0378	0.0475	0.00648	0.0474	0.0800*
	(0.00599)	(0.0444)	(0.0449)	(0.00593)	(0.0404)	(0.0411)
lag_pchgstpop	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
Constant	0.0128	0.586***	0.597***	-0.0168	0.635***	0.583***
	(0.0416)	(0.179)	(0.217)	(0.0429)	(0.154)	(0.187)
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

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

VARIABLES	(1) chglnpop	(2) chglnpop	(3) Chglnpop
Change in crime, t, t+1	-0.657***	-0.654***	-0.601***
	(0.0585)	(0.0552)	(0.0555)
lag_unemployment	-0.000325	-0.00144***	-0.000611
	(0.000232)	(0.000370)	(0.000460)
lag_income_cnl	0.00460***	0.00454*	0.00216
	(0.000877)	(0.00240)	(0.00247)
lag_pctblack	-0.0192***	-0.103***	-0.0861**
	(0.00372)	(0.0368)	(0.0365)
lag_p0to17	-0.0228	-0.702***	-0.676***
	(0.0577)	(0.171)	(0.226)
lag_p18to24	0.256***	-0.388**	-0.0726
	(0.0830)	(0.175)	(0.233)
lag_p25to44	-0.0482	-0.497**	-0.907***
	(0.0394)	(0.201)	(0.250)
lag_p45to64	0.0528	-0.502**	-0.222
	(0.0998)	(0.199)	(0.251)
lag_foreign	0.00479	0.0201	0.0540
	(0.00569)	(0.0383)	(0.0389)
lag_pchgstpop	0.379***		
	(0.0582)		
Year Fixed Effects	Yes	Yes	
Region Fixed Effects	Yes		
City Fixed Effects		Yes	Yes
Region-Year Effects			Yes
Constant	-0.0192	0.525***	0.524***
	(0.0400)	(0.147)	(0.179)
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

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

VARIABLES	Model 1 chglnpop	Model 2 chglnpop	Model 3 chglnpop	Model 4 chglnpop
Homicide rate, t	0.0232***	0.0252***		
	(0.00826)	(0.00817)		
# Homicide, t			-0.0813	-0.0644
			(0.276)	(0.269)
City population, t			-0.0203***	-0.0177***
			(0.00271)	(0.00271)
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

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

VARIABLES	(1) chglnpop	(2) chglnpop	(3) chglnpop	(4) chglnpop	(5) chglnpop	(6) chglnpop
Change in crime, t, t+1	-1.168*	-1.341**				
	(0.705)	(0.660)				
Crime rate, t			-0.0925	0.130		
			(0.247)	(0.377)		
Change in crime, t-1, t					-1.791**	-1.268
					(0.877)	(0.848)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes		Yes		Yes	
City Fixed Effects		Yes		Yes		Yes
Region-Year Effects						
Observations	2324	2324	2485	2485	2182	2182
R-squared	0.278	0.307	0.121	0.182		0.158

Table 5
Retention: Share of 1995 city residents remaining in the city in 2000

	One		Two		Three		Four	
Variable	**B**	**SE**	**B**	**SE**	**B**	**SE**	**B**	**SE**
ΔCitycrime9295	0.002	0.336	-0.039	0.282	---	---	---	---
Citycrimerate 1995	---	---	---	---	-0.305	0.201	-0.122	0.174
MSAunemployment	0.000	0.003	0.002	0.003	0.001	0.003	0.002	0.003
CityMedianIncome	0.000	0.000	0.000	0.000	0.000	0.000	***0.000***	0.000
%black	0.048	0.031	**0.100**	0.027	***0.059***	0.031	**0.103**	0.027
%pop<17	0.275	0.200	0.002	0.174	0.260	0.197	0.003	0.173
% 18_24	**-1.203**	0.205	**-0.974**	0.177	**-1.211**	0.195	**-0.988**	0.170
%25_44	-0.292	0.233	0.115	0.207	-0.163	0.239	0.152	0.210
%45_64	0.161	0.243	-0.128	0.210	0.081	0.246	-0.154	0.212
%MANUF	-0.127	0.105	-0.015	0.090	-0.140	0.103	-0.021	0.090
%Homewners	**0.169**	0.077	**0.200**	0.065	**0.160**	0.074	**0.194**	0.063
%CollegeDegree+	0.059	0.117	-0.004	0.099	0.047	0.112	-0.004	0.095
JANAVGDL	0.000	0.001	0.000	0.001	0.000	0.001	0.000	0.001
JULYAVGD	0.002	0.002	0.001	0.001	0.002	0.002	0.001	0.001
AVGANLPR	0.000	0.001	0.000	0.000	0.001	0.001	0.000	0.000
MIDWEST	***0.028***	0.015	**0.032**	0.013	**0.028**	0.015	**0.031**	0.013
NORTHEAS	**0.044**	0.018	**0.067**	0.016	**0.041**	0.018	**0.065**	0.016
WEST	-0.016	0.023	0.013	0.020	-0.007	0.021	0.015	0.018
%Foriegn Born	**0.369**	0.101	**0.218**	0.088	**0.340**	0.101	**0.210**	0.089
%of MSApopinCC	---	---	**0.116**	0.019	---	---	**0.113**	0.019
R-square	0.799		0.846		0.785		0.846	

All models include intercepts. Bold indicates significant at the 5% of higher level, italics 10% or higher.

Table 6

CCRetention: among CC residents staying in MSA

	One		**Two**		**Three**		**Four**	
Variable	**B**	**SE**	**B**	**SE**	**B**	**SE**	**B**	**SE**
∆Citycrime9295	-0.054	0.831	-0.188	0.562	---	---	---	---
Citycrimerate 1995	---	---	---	---	**-1.190**	0.489	***-0.585***	0.341
%of MSApopinCC	---	---	**0.388**	0.038	---	---	**0.376**	0.038
R-square								

CCRetention: CC residents movers, but staying in MSA

Variable	**B**	**SE**	**B**	**SE**	**B**	**SE**	**B**	**SE**
∆Citycrime9295	-0.095	1.280	-0.320	0.761	---	---	---	---
Citycrimerate 1995	---	---	---	---	**-1.660**	0.757	-0.636	0.465
%of MSApopinCC	---	---	**0.650**	0.051	---	---	**0.637**	0.052
R-square								

All models include intercepts. Bold indicates significant at the 5% of higher level, italics 10% or higher.
Estimated using weighted least squares.
All models include full list of controls in Table 5, in paper.

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b

Appendix: 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 Stephen Raphael and Rucker Johnson.

Appendix Table A1

Descriptive Statistics for Annual Data Sample

	N	Mean	Std. Deviation
2000 census pop	3926	416910.1	763755.3
change in ln population	3746	0.008	0.039
city total crime per capita level	3825	0.085	0.026
Suburb total crime per capita	3058	0.043	0.014
City total crime change	3640	-0.001	0.008
Change in suburb total crime	2899	0.000	0.006
Suburb violent crime per capita	3060	0.004	0.002
MSA unemployment	3123	6.010	2.574
Prison Releases per Crime	3622	1.977	3.129
Prison Admits per Crime	3622	2.136	3.228
Change Prison Releases per Crime	3471	0.142	0.504
Change Prison Admits per Crime	3471	0.147	0.462
State Per Capita Income	3775	21.123	7.440
%Black	3171	0.230	0.177
State age 0 to 17	3775	0.262	0.020
State age 18 to 24	3775	0.108	0.015
State age 25 to 44	3775	0.307	0.022
State age 45 to 64	3775	0.201	0.022
Percent Change State Population	3624	0.012	0.012
Valid N (listwise)	2767		

Append Table A2: Migration Sample Statistics

Variable	**Mean**	S.D.
ΔPer Capita City Crime (1995-2000)	-0.0316	(0.0405)
ΔPer Capita City Crime (1992-1995)	-0.0069	(0.0213)
ΔPer Capita Suburban Crime (1995-2000)	-0.0060	(0.0093)
ΔPer Capita Suburban Crime (1992-1995)	-0.0057	(0.0106)
MSA Unemployment Rate, 1995	0.0518	(0.0220)
City Median Family Income, 1990	31667.4	(6070.4)
%Black, 1990	0.2310	(0.1831)
%Aged 0-17, 1990)	0.2369	(0.0421)
%Aged 18-24, 1990	0.1173	(0.032)
%Aged 25-44, 1990	0.3134	('0.0317)
%Aged 45-64, 1990	0.1611	(0.0251)
%Manufacturing, 1990	0.1478	(0.058)
%Home Owner, 1990	0.5183	(0.0853)
%College Degree, 1990	0.2168	(0.0780)
Average July Temperature, 1994	76.2	(5.28)
Average January Temperature, 1994	34.4	(11.54)
Average Yearly Precipitation, 1994	35.6	(14.03)
N	112	