The Effect of Police on Crime: New Evidence from U.S. Cities, 1960-2010

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Abstract

This paper shows that errors in the measurement of police are a primary impediment to the accurate estimation of the effect of police on crime. We collect multiple measures of the number of police for a large sample of cities over a long period of time. Correcting for measurement error, we estimate elasticities of crime with respect to police of roughly -0.4 for violent crime and -0.2 for property crime. Elasticities are largest for murder, robbery, and motor vehicle theft.

JEL Classification: C14, C21, C52.

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I. Introduction

One of the most intuitive predictions of deterrence theory is that, all else equal, an increase in the probability of apprehension decreases participation in crime. This prediction is a core part of Becker's (1968) account of deterrence theory and is also present in the historical articulations of the theory given in Beccaria (1764) and Bentham (1789). The prediction is no less important in more recent treatments, such as the models discussed in Lochner (2004), Burdett, Lagos and Wright (2004), Lee and McCrary (2009), and McCrary (2010), among others.[1]

On the empirical side, the literature has focused on the specific question of the relationship between police prevalence and crime, where police are viewed as a primary factor influencing the probability of apprehension facing a potential offender. The empirical literature addressing the effect of police on crime encompasses hundreds of articles, and indeed, the literature is sufficiently large that there are many prominent review articles, including Nagin (1978), Cameron (1988), Nagin (1998), Eck and Maguire (2000), Skogan and Frydl (2004), and Levitt and Miles (2006), among others.[2]

Early empirical papers such as Ehrlich (1972) and Wilson and Boland (1978) focused on the cross-sectional association between police and crime. Concern over the potential endogeneity of policing levels, however, led to a predominance of papers using panel data techniques (Cornwell and Trumbull 1994, Marvell and Moody 1996, Witt, Clarke and Fielding 1999, Fajnzylber, Lederman and Loayza 2002, Baltagi 2006) and, more recently, quasi-experimental techniques such as instrumental variables and differences-in-differences (Levitt 1997, Di Tella and Schargrodsky 2004, Klick and Tabarrok 2005, Evans and Owens 2007, Machin and Marie 2011).

In the U.S. context, the typical panel data approach uses information on cities over time and regresses log crime on the log of the number of sworn police as well as additional control variables.[3] Common control variables include city effects, year effects, and measures of the age structure in the population. Frequently, city effects are not estimated using fixed effects, but rather are eliminated by taking first differences. Generally speaking, elasticity estimates based on these panel data approaches tend to be persistently negative, but small relative to the estimated standard error, at least for large U.S. cities in recent decades.

These findings have convinced many researchers that cities hire police officers during, or perhaps even in anticipation of, crime waves, leading even growth rate regressions to be subject to simultaneity bias

[1] Polinsky and Shavell (2000) provide a review of the theoretical deterrence literature that emerged since Becker (1968), with a particular focus on the normative implications of the theory for the organization of law enforcement strategies.

[2] The most recent survey, Lim, Lee and Cuvelier (2010), surveys 258 papers.

[3] Sworn police officers carry a badge and a gun and have the power of arrest. Civilian employees do not have the same authority.

(Marvell and Moody 1996, Levitt 1997, Di Tella and Schargrodsky 2004, Klick and Tabarrok 2005). These papers have estimated the police elasticity using a variety of quasi-experimental approaches. In the main, the results from this literature are larger in magnitude than those from the panel data regression papers.

Another explanation for the small magnitude of the police elasticity estimates based on least squares, relative to those from the quasi-experimental literature, is that the number of police is measured with error. As emphasized in the literature on the return to education, even under the classical measurement error model, measurement error in a regressor leads to two basic difficulties (Griliches 1977, Ashenfelter and Krueger 1994, Angrist and Krueger 1999). First, the estimated effect of the mismeasured regressor is attenuated; that is, it is of the same sign as but smaller in magnitude than its population counterpart. Second, this attenuation bias is exacerbated by the inclusion of control variables, or by taking transformations of the data, such as first differences. Since most of the panel data literature on the effect of police on crime includes control variables in first differenced specifications, measurement error in the number of police has the potential to be an economically important source of bias.

In this paper, we present estimates of the elasticity of crime with respect to police that correct for measurement error. Our results are based on a large new panel data set on crime and policing pertaining to 135 large U.S. cities over the period 1960-2010. For each city and each year, we utilize two measures of the number of police, one based on the standard data set on police staffing collected by the Federal Bureau of Investigation (FBI) as part of its Uniform Crime Reports (UCR) program and the other based on a rarely used data set on police staffing collected by the Census Bureau as part of its Annual Survey of Government (ASG) program. The crux of our approach is to use one noisy measure of police staffing as an instrument for another noisy measure. Under the classical measurement error model, such instrumental variables estimates have the same probability limit as least squares, were the true measure of police available. If, as has been emphasized in the previous literature, simultaneity bias is an important additional source of bias, then the true elasticity of crime with respect to police is likely at least as large as that probability limit. Hence, our analysis may be viewed as conservative.

We begin the paper with a discussion of some evidence on the extent of measurement error. We then turn to a discussion of the data. We next report our estimated elasticities, which are based on conventional econometric methods to adjust for measurement error.

II. Evidence on the Extent of Measurement Error

A. Direct Evidence

In the 2003 version of *Crime in the United States*, the Federal Bureau of Investigation reports that the New York Police Department employed 28,614 sworn police officers on October 31, 2003. Relative to the 37,240 sworn officers employed in 2002 and the 35,513 officers employed in 2004, this is a remarkably low number. If these numbers are to be believed, then the ranks of sworn officers in New York City fell by one-quarter in 2003, only to return to near full strength in 2004.

An alternative interpretion is that the 2003 number is a mistake. Panel A of Figure 1 compares the time series of sworn officers of the New York Police Department based on the UCR reports with that based on administrative data from 1990-2009 discussed in Zimring (2011).[4] These data confirm that the 2003 measure is in error and additionally suggest that the 1999 measure may be in error. These discrepancies may also support a more speculative inference that the numbers for 1963 and 1974 are in error.

Administrative data on the number of officers is difficult to obtain. More readily available are departmental annual reports. However, even these are not easy to obtain; annual reports are largely internal municipal documents and historically did not circulate widely.[5] Moreover, the annual report may or may not report the number of officers employed by the police department.

Nonetheless, we have been able to obtain scattered observations on the number of sworn officers from annual reports for selected other cities in selected years: Los Angeles, Chicago, Boston, and Lincoln, Nebraska. The numbers for Chicago have been further augmented by the strength report data reported in Siskin and Griffin (2007).[6] The time series of sworn officers for these cities is given in Figure 1 in panels B through E. The figure shows that the UCR data for Los Angeles are in close correspondence with the annual report data and that the UCR data for Chicago, Boston, and Lincoln are more accurate than those for New York, but less accurate than those for Los Angeles.

Table 1 summarizes these findings. Columns correspond to the five cities and rows correspond to whether the number of officers are measured in logs or in log differences. The table highlights that, treating the administrative and annual report data as the true measure, (1) there is a broad range of fidelity in reporting to the UCR program, with Los Angeles being the most faithful, New York the least, and the others somewhere between those two bookends, and (2) after taking first differences, the correlation between the UCR data

[4]See Data Appendix for details on these data.

[5]In recent years, many departments have begun a practice of posting annual reports online, but only a few cities have endeavored to post historical annual reports.

[6]See Data Appendix for details on the annual report and strength report data.

and the truth falls by anywhere from an estimated 8 percent (Los Angeles) to 51 percent (New York). This is important, because much of the literature uses first-differenced data. Consequently, the much smaller correlations in the second row of the table are the relevant ones for gauging the magnitude of measurement error.

It may be surprising that there is ambiguity regarding the number of sworn officers. However, counting the number of sworn officers is more subtle than it would appear. First, and perhaps most basically, there may be confusion between the number of total employees—sworn and civilian—and the number of sworn officers. Second, newly hired officers typically attend Police Academy at reduced pay for roughly 6 months prior to swearing in, and there may be ambiguity regarding whether those students count as sworn officers prior to graduation. Third, there is often a discrepancy between authorized and deployed strength. Authorized strength refers to the number of officers the department has authority from the city government to employ, whereas deployed strength refers to the actual number of employees. These numbers can differ. For our main sample of cities, we have measures of the number of authorized and deployed sworn officers for selected recent years from the Law Enforcement Management and Administrative Statistics (LEMAS). These data show that the number of deployed sworn officers ranges from 62 to 128 percent of authorized strength.[7] Fourth, some officers work part time. This creates ambiguity, as well—should "sworn officers" be interpreted to mean all sworn officers, full-time sworn officers, or full-time-equivalent sworn officers? The LEMAS data indicate that roughly 1 to 2 percent of officers work part-time. Fifth, the number of sworn officers fluctuates within the year. The New York Police Department uses average daily strength in some internal police documents. The UCR reports a point-in-time measure of the number of sworn officers as of October 31. Based on the internal reports we have reviewed, the most common pattern is to use a point-in-time measure as of the end of the fiscal year, typically June 30. To give a sense of how the timing of measurement may matter, Figure 2 displays the monthly count of the number of sworn officers for Chicago for 1979-1997, with the count for October superimposed.[8] The figure makes it clear that there is a great deal of within-year volatility in the number of sworn officers. This discussion makes it clear that errors in measures of the number of sworn officers take a variety of sources, including (1) typographical or data entry errors, (2) errors arising from genuine uncertainty regarding the number of police by municipal governments, and (3) transitory movements in the number of police throughout the calendar year, among others.

[7]Numbers refer to a pooled analysis of data from 1987, 1990, 1993, 1997, 1999, 2000, and 2003. Population weighted mean and standard deviation are 97 percent and 5 percent, respectively.

[8]During this period, a unique micro dataset on sworn officers is available. These data are discussed in Siskin and Griffin (2007). See Data Appendix for details.

B. Comparison of Two Noisy Measures

Police department internal documents are presumably more accurate than the information police departments report to the UCR program. However, as discussed, these are only available in selected cities and selected years. Trading off accuracy for coverage, we now present a comparison of the UCR series on the number of sworn officers with a series based on the ASG. We use the ASG data to construct an annual series on full-time sworn officers for all 135 cities in our main analysis sample. We define this sample and give background on the ASG data in Section III, below.

Figure 3 provides visual evidence of the statistical association between the UCR and ASG series for sworn officers, measured in logs (panel A) and first differences of logs, or growth rates (panel B). In panel A, we observe a nearly perfect linear relationship between the two measures, with the majority of the data points massed around the 45° line. The regression line relating the log UCR measure to the log ASG measure is nearly on top of the 45° line, with a slope of 0.98. Panel B makes it clear that differencing the data substantially reduces the statistical association between the UCR and ASG series; the slope coefficient for the log differenced data is just 0.25.

These patterns are consistent with the classical measurement error model, which posits that two observed series are related to a single latent measure as

$$S_{ct} = S^*_{ct} + u_{ct} \quad (1)$$

$$Z_{ct} = S^*_{ct} + v_{ct} \quad (2)$$

Here, S_{ct} is the UCR measure in city c and year t, Z_{ct} is the ASG measure, S^*_{ct} is the latent variable or *signal*, and u_{ct} and v_{ct} are mean zero measurement errors which are mutually independent at all leads and lags and independent of the signal at all leads and lags. This simple statistical model implies that the covariance between the UCR and ASG is given by the variance of the signal and that the population regression of the UCR series on the ASG series yields a coefficient of $V[S^*_{ct}]/\left(V[S^*_{ct}] + V[v_{ct}]\right)$, a quantity which is known as the reliability ratio. Consequently, under this model, we would interpret the slope coefficient of 0.98 in panel A to mean that the variance of the noise v_{ct} is approximately 2 percent as large as the variance of the signal S^*_{ct}.[9]

[9]That is, if the population regression coefficient is π, then $V[u_{ct}] = V[S^*_{ct}](1-\pi)/\pi$.

To contextualize the results in panel B, we take first differences of equations (1) and (2), obtaining

$$\Delta S_{ct} = \Delta S^*_{ct} + \Delta u_{ct} \quad (3)$$

$$\Delta Z_{ct} = \Delta S^*_{ct} + \Delta v_{ct} \quad (4)$$

Parallel with the calculations above, under the classical measurement error model, we interpret the slope coefficient in panel B of 0.25 to mean that the variance of Δv_{ct} is 3 times as large as the variance of ΔS^*_{ct}.[10]

This is a dramatic difference, but the result is intuitive. The dominant source of variation in log sworn officers is time-invariant differences between cities. This is of course one of the key motivations in the literature for taking first differences. Nonetheless, the upshot of this fact is that $V[\Delta S_{ct}]$ is quite small relative to $V[S_{ct}]$. Differencing has the opposite effect on the noise term: since v_{ct} is white noise, $V[\Delta v_{ct}]$ is twice as big as $V[v_{ct}]$.

A standard result in econometrics, noted in Wooldridge (2002, p. 75) for example, is that the probability limit of the slope coefficient in a bivariate regression of one variable on another, where the other variable has a reliability ratio of r, is the target parameter times r. Consequently, since $r = 0.25$ for the data in growth rates, this simple analysis suggests that in order to furnish an estimate of the elasticity of crime with respect to police, the slope coefficient in a regression of growth rates in crime on growth rates in police should be inflated by a factor of roughly 4. This is a simple way to understand the motivation for using IV in the presence of measurement errors: IV amounts to taking the ordinary least squares (OLS) coefficient and inflating it by the inverse of the reliability ratio.

When further control variables are added, all of which are measured without error, then the relevant reliability ratio becomes $r = V[\xi_{ct}] \big/ (V[\xi_{ct}] + V[\Delta v_{ct}])$, where ξ_{ct} is the error term in the population regression of ΔS^*_{ct} on all of the control variables in the model. Since $V[\xi_{ct}] \leq V[\Delta S^*_{ct}]$, we conclude that once control variables such as fixed effects are added to the model, it may be appropriate to inflate regression estimates by a factor larger than 4. These conclusions underscore the practical relevance of the problem of measurement error in the context of panel data regressions of crime on police.

III. Data

Virtually all empirical studies of the effect of police on crime use data from the UCR, collected annually by the FBI. Crime measures represent the total number of offenses known to police to have occurred during

[10]Following the calculations from above, and treating the estimated slope as the population regression coefficient, we have $V[\Delta u_{ct}] = V[\Delta S^*_{ct}](1 - 0.25)/0.25 = 3V[\Delta S^*_{ct}]$.

the calendar year and are part of the "Return A" collection. As noted above, sworn police represent a snapshot as of October 31st of the given year and are part of both the Law Enforcement Officers Killed or Assaulted (LEOKA) collection and the Police Employees (PE) collection. Because of the late date of the measurement of the number of police, it is typical to measure police in year t using the LEOKA file from year $t-1$, and we follow that convention here. Consequently, although we have data from 1960-2010, our regression analyses of growth rates pertain to 1962-2010.

As noted above, we augment data from the UCR with data from the Annual Survey of Government (ASG) Employment, an annual survey of municipal payrolls that has been administered by the Bureau of Labor Statistics and reported to the U.S. Census annually since 1952. The ASG data provide payroll data for a large number of municipal functions including elementary and secondary education, judicial functions, public health and hospitals, streets and highways, sewerage and police and fire protection among others. The survey generally provides information on the number of full-time, part-time and full-time equivalent sworn and civilian employees for each function and for each municipal government.[11]

Our sample of 135 cities consists of cities with a 2010 population exceeding 100,000 individuals and a population of at least 50,000 individuals in each year during the 1960-2010 study period. Information on police staffing is available in both the UCR data and ASG data for each of these cities for the entire study period.[12] The LEOKA data provide the number of full-time sworn police officers in each year. The ASG data provide the same information beginning in 1977. Prior to 1977, the ASG series reports only the number of full-time equivalent police personnel, without differentiating between sworn officers and civilian employees. In order to extend the series, we generate a city- and year-specific estimate of the proportion of officers who are sworn using the LEOKA data.[13] This was accomplished by regressing the proportion sworn on a year and city effects and generating a predicted value for each city-year. The predicted values were then multiplied by full-time equivalent officers from the ASG series prior to 1977 to generate a predicted number of sworn FTE officers. Next, in order to generate an estimate of the number of full-time sworn officers, a city-specific estimate of the average ratio of full-time equivalent officers to full-time officers was generated using the ASG data from 1977-2008.[14] Multiplying this ratio by the number of sworn FTEs yields an estimated number of sworn full-time officers for each city prior to 1977.

[11]Full-time equivalent employees represent the number of full-time employees who could have been employed if the hours worked by part-time employees had instead been dedicated exclusively to full-time employees. The statistic is calculated by dividing the number of part-time hours by the standard number of full-time hours and then adding this number to the number of full-time employees.

[12]We fill in missing observations using linear interpolation. We note that the ASG was not administered in 1996.

[13]The LEOKA data series is chosen because it is complete for all city-years.

[14]This ratio ranges from a low of 83% to a high of 100%, with a mean of 99.8%.

In addition to these data we have collected historical information on several important covariates. One such control variable is city log revenues. We were particularly concerned with collecting this series because of a particular causal channel which might lead regression-based estimates of the effect of police on crime to be negatively biased. According to this story, cities lay off police officers when the budget is tight, which coincides with a period of a weak local economy and a possible labor market link to crime. Appendix Figure 1 shows the time series of city government revenues and expenditures less police department expenditures for each city in our sample. These data are from the Annual Survey of Government Finance.[15]

Another obvious control variable is the overall population. The population measure utilized in this research is drawn from the FBI's LEOKA file. While this series contains valid observations for nearly all city-years, it is potentially contaminated by measurement error, particularly in the years immediately prior to the decennial Census.[16] In particular, for a substantial number of cities, population is not smooth across the Census year thresholds. We generated an adjusted population measure using the predictions from local linear regression with a bandwidth of 5 and the triangle kernel (Fan and Gijbels 1996).[17] These population imputations, as well as the raw data underneath them, are shown for each city in the sample in Appendix Figure 2. That figure also shows population counts based on the decennial census for reference.

We additionally consider population disaggregated by age, sex, and race/ethnicity. These data, collected by the Census Bureau as part of its Population Estimates program, are only available starting in 1970. However, we extend the series using data from the 1960 Census and linearly interpolating between the Census years.

We turn now to Table 2, which provides summary statistics for each of our two primary police measures as well as each of the seven so-called index offenses—murder, rape, robbery, aggravated assault, burglary, larceny exclusive of motor vehicle theft ("larceny"), and motor vehicle theft. We additionally report summary statistics for the aggregated crime categories of violent and property crime. The left-hand panel of Table 2 gives statistics for the levels of crime and police in per capita terms, specifically as a measure of the value per 100,000 population. The right-hand panel gives statistics for log differences of crime and police.

Several features of the data are worth noting. First, a typical city employs approximately 260 police officers per 100,000 population, one officer for every 4 violent crimes, and one officer for every 24 property crimes. There is considerable heterogeneity in this measure over time, with the vast majority of cities hiring additional police personnel over the study period. However, there is even greater heterogeneity accross

[15] See Data Appendix for details on these data.
[16] See the Data Appendix for a visual presentation of these data for each city.
[17] We describe the procedure we employ in greater detail in the Data Appendix to this paper.

cities, with between city variation accounting for nearly 90% of the overall variation in the measure. The pattern is somewhat different for the crime data, with a roughly equal proportion of the variation arising between and within cities.

Second, it is worth pointing out that the vast majority (86%) of crimes are property crimes with the most violent crimes (murder and rape) comprising less than 1% of all crimes reported to police. It is likewise important to note that each of the crime aggregates is dominated by a particular crime type with assault comprising nearly half of all violent crimes and larceny comprising 57% of all property crimes.

Third, and turning to the growth rates, perhaps the most relevant feature of the data is that the taking first differences of the series essentially eliminates time invariant cross-sectional heterogeneity in log crime and log police. For each measure of crime and police, the within standard deviation in growth rates is essentially equal to the overall standard deviation.

Figure 4 highlights long-run trends in crime and police. Panels A, B, and C present the time series for total violent crime, total property crime, and total sworn officers for our sample of 135 cities, 1960-2010. The series show a remarkable 30 year rise in criminality from 1960 to 1990, followed by an equally remarkable 20 year decline in criminality from 1990 to 2010. These swings are spectacular in magnitude. Violent crimes are below 200,000 in 1960, rise to well over 800,000 by 1990, and then decline to just above 500,000 by 2008. Property crimes are below 1.5 million in 1960, rise to 4.5 million by 1990, and then decline to below 3 million by 2008.

The series for sworn police shows quite different secular trends. The 1960s is a decade of strong gains, from 110,000 officers to 150,000 officers, with acceleration evident after the wave of riots 1965-1968, followed by a slower rate of increase during the first half of the 1970s. During the second half of the 1970s, we see an era of retrenchment, perhaps related to urban fiscal problems. From 1980 to 2000, sworn police generally increase, with particularly strong increases in the 1990s. Since 2000 the numbers are roughly flat, with the exception of 2003, which is driven entirely by the erroneous estimate provided by the New York City Police Department to the UCR program (cf., Figure 1).

These secular trends are fascinating, but it is hard to know what to make of them. Throughout our analysis, we focus on year-over-year growth rates in crime and police and further absorb the secular trends by including year effects as covariates. Interestingly, this is also the performance metric used by many police departments in their annual reports. That is, they discuss year-over-year growth rates and compare their numbers to year-over-year growth rates in national averages.

Our focus on this transformation implies that the source of identifying information in our estimation strategies is related to the temporal changes in the standard deviation of year-over-year growth rates. The bottom part of panels A, B, and C show these standard deviations and how they have evolved over time. The figure shows that there has been some slight decline in the standard deviation of the growth rate of sworn police over time. An interesting pattern is the strong spike in the standard deviation of the crime growth rates around 1990. This pattern is attributable to differences across cities in the date of the peak of crime. Around 1990, some cities are still experiencing the wave of violence related to the crack epidemic, while other cities are already seeing the beginnings of the crime decline. Generally speaking, however, all time periods seem equally likely on an a priori basis to be informative regarding the effect of police on crime and so we focus on estimates that are based on all available years.

IV. Identification

Our key equation of interest is

$$Y_{cst} = \theta_0 S^*_{cst} + \pi_0' X_{cst} + \mu_c + \varepsilon_{cst} \tag{5}$$

where Y_{cst} is log crime in city c in state s and year t, S^*_{cst} is the log of the true number of police, μ_c are city effects, and X_{cst} is a vector of control variables such as log revenues per capita, log population, the demographic structure of the population, and year effects or state-by-year effects. Were the true number of police to be observed, we would be interested in obtaining weighted least squares estimates of θ_0, with weights proportional to 2010 city population to arrive at a police elasticity estimate that is representative of a typical resident of a large U.S. city.

Following the literature, we difference this specification to eliminate the city effects μ_c. Combining this first-differenced model with the measurement error model articulated in equations (3) and (4), we have the statistical model

$$\Delta Y_{cst} = \theta_0 \Delta S^*_{cst} \beta + \pi_0' \Delta X_{cst} + \Delta \varepsilon_{cst} \tag{6}$$

$$\Delta S_{cst} = \Delta S^*_{cst} + \Delta u_{cst} \tag{7}$$

$$\Delta Z_{cst} = \Delta S^*_{cst} + \Delta v_{cst} \tag{8}$$

where as before the measurement errors Δu_{cst} and Δv_{cst} are mutually independent, are independent of the signal ΔS^*_{cst}, and newly are further independent of $\Delta \varepsilon_{cst}$. Substituting equation (7) into equation (6) and then linearly projecting ΔS_{cst} onto ΔZ_{cst}, and ΔX_{cst}, we have a variant on the simultaneous equations model, whereby ΔZ_{cst} is an excluded instrument for the endogenous regressor ΔS_{cst}.

This reasoning suggests a second consistent estimator as well. The IV estimator described above views ΔS_{cst} as the endogenous regressor and ΔZ_{cst} as the instrument, but we could symmetrically view instead ΔZ_{cst} as the endogenous regressor and ΔS_{cst} as the instrument (to see this, simply exchange the substitution sequence described). We can also then combine these two approaches to yield a third estimate, which solves the overidentified method of moments problem associated with the moment function

$$g_{cst}(\theta, \nu) = \begin{pmatrix} \begin{pmatrix} \Delta Z_{cst} \\ \Delta X_{cst} \end{pmatrix} (\Delta Y_{cst} - \theta \Delta S_{cst} - \nu_1' \Delta X_{cst}) W_{cst} \\ \begin{pmatrix} \Delta S_{cst} \\ \Delta X_{cst} \end{pmatrix} (\Delta Y_{cst} - \theta \Delta Z_{cst} - \nu_2' \Delta X_{cst}) W_{cst} \end{pmatrix} \tag{9}$$

where $\nu = (\nu_1, \nu_2)$ is a vector of nuisance parameters and W_{cst} is 2010 city population.

We will additionally be concerned about the role of population in the outcome equation and the extent to which it is measured with error. A secondary equation of interest is thus

$$Y_{cst} = \theta_0 S^*_{cst} + \beta_0 P^*_{cst} + \pi_0' X_{cst} + \mu_c + \varepsilon_{cst} \tag{10}$$

where now log population is no longer included in X_{cst}. We will show that annual city population is measured with substantial error in both the LEOKA and ASG data. We adopt the same strategy to avoid the bias associated with mismeasurement of population: use the one noisy measure as an instrument for the other. This approach will yield consistent estimates under the classical measurement error model, which in addition to the articulation given above now implies that the log population proxies can be thought of as

$$P_{cst} = P^*_{cst} + \widetilde{u}_{cst} \tag{11}$$

$$Q_{cst} = P^*_{cst} + \widetilde{v}_{cst} \tag{12}$$

where we assume that $\widetilde{u}_{cst}$ is conditionally (on X_{cst}) independent of u_{cst}, v_{cst}, $\widetilde{v}_{cst}$, S^*_{cst}, and ε_{cst} and that $\widetilde{v}_{cst}$ is conditionally independent of u_{cst}, v_{cst}, $\widetilde{u}_{cst}$, S^*_{cst}, and ε_{cst}, and similarly for u_{cst} and v_{cst}.

This leads to four consistent IV estimators, rather than just two. We can use both ASG measures as instruments for both LEOKA measures, both LEOKA measures as instruments for both ASG measures, or the ASG and LEOKA measures of police and population as instruments for the LEOKA and ASG measures of the same, or the LEOKA and ASG measures of police and population as instruments for the ASG and LEOKA measures. Estimates of θ_0 and β_0 can be obtained by solving the overidentified method of moments problem associated with the moment function

$$g_{cst}(\theta,\beta,\nu) = \begin{pmatrix} \begin{pmatrix} \Delta Z_{cst} \\ \Delta Q_{cst} \\ \Delta X_{cst} \end{pmatrix}(\Delta Y_{cst} - \theta\Delta S_{cst} - \beta\Delta P_{cst} - \nu_1'\Delta X_{cst})W_{cst} \\ \begin{pmatrix} \Delta S_{cst} \\ \Delta Q_{cst} \\ \Delta X_{cst} \end{pmatrix}(\Delta Y_{cst} - \theta\Delta Z_{cst} - \beta\Delta P_{cst} - \nu_2'\Delta X_{cst})W_{cst} \\ \begin{pmatrix} \Delta Z_{cst} \\ \Delta P_{cst} \\ \Delta X_{cst} \end{pmatrix}(\Delta Y_{cst} - \theta\Delta S_{cst} - \beta\Delta P_{cst} - \nu_3'\Delta X_{cst})W_{cst} \\ \begin{pmatrix} \Delta S_{cst} \\ \Delta P_{cst} \\ \Delta X_{cst} \end{pmatrix}(\Delta Y_{cst} - \theta\Delta Z_{cst} - \beta\Delta Q_{cst} - \nu_4'\Delta X_{cst})W_{cst} \end{pmatrix} \quad (13)$$

where $\nu = (\nu_1, \nu_2, \nu_3, \nu_4)$ and W_{cst} is a population weight.

For both of these overidentified models, the test of overidentifying restrictions can be viewed as an omnibus test of the classical measurement error model. Because of the well-known statistical problems with overidentified method of moments estimators and, in particular, with the test of overidentifying restrictions, we focus on results using empirical likelihood (Owen 2001).

The police elasticity estimates that result from these types of approaches correct for measurement error bias, but do not adjust for simultaneity bias. These estimates will therefore accurately approximate the elasticity of crime with respect to police if fluctuations in police staffing within a city over time are exogenous with respect to crime.

Exogeneity of police fluctuations is not a completely implausible assumption. Cities may have other objectives in regards to police staffing than the intertemporal smoothing of the marginal disutility of crime. Consider the example of Detroit's police numbers over the period 1975-1984. Mayor Coleman Young sought to aggressively hire officers under an affirmative action plan (Deslippe 2004). In 1977, 1245 officers were hired under the plan, increasing the size of the police force by some 20 percent. The next year, a further 227 officers were hired under the plan. After Detroit hired those officers, the city confronted a serious budget crisis. The city was compelled to lay off 400 and 690 officers in 1979 and 1980, respectively. In 1981 and 1982, the city was able to recall 100 and 171 of the laid off officers, respectively. However, a new round of cuts in 1983 undid this effort, as 224 officers were again laid off. In 1984, 135 of those officers were recalled.[18]

These boom and bust patterns in police hiring are somewhat common and seem to reflect some combination of city constraints and lack of foresight (Koper, Maguire and Moore 2001). For example, municipalities operate under many borrowing constraints, including tax and expenditure limitations (Joyce

[18] *NAACP v. Detroit Police Officers Association*, 591 F. Supp. 1194 (1984).

and Mullins 1991, Advisory Commission on Intergovernmental Relations 1995, Poterba and Rueben 1995, Shadbegian 1999), and balanced budget requirements (Cope 1992, Rubin 1997, City of Boston 2007).[19] Lewis (1994) reports that 99 of the 100 largest U.S. cities are required to balance the budget by state constitution, state statute, or city charter.

Perhaps in part because of these constraints, fiscal crises emerge with some regularity in cities, and this leads to police layoffs. Responding to the recent financial crisis, Camden laid off 45 percent of its sworn officers in early 2011 (Katz and Simon 2011). More historically, in 1981, Boston confronted a sluggish to recessionary economy, Proposition 2½, and a major Massachusetts Supreme Court decision that led to large reductions in Boston's property tax revenue.[20] Seeking to balance the budget, the city reduced the police department budget by over 27 percent. The department eliminated all capital expenditures, closed many police stations, and reduced the number of sworn officers by 24 percent (Boston Police Department 1982).

Cities also frequently fail to anticipate the ripple effects of past booms in hiring. Pension rules lead to spikes in retirement after 20 and 25 years, so a hiring boom two or more decades ago may result in a police officer shortage. Describing the situation in Chicago in 1986, Recktenwald (1986) notes that "[i]n 1983, an average of 32 officers a month left the force. Today the monthly average stands at 71, the records show. This comes at a time when the department's largest branch... is more than 1,000 officers short of the 7,940 level authorized by the Chicago City Council".

On the other hand, cities facing a difficult crime problem may be able to obtain extra funding from the state or federal government, and this may lead to simultaneity bias. Describing the situation in Washington, D.C., Harriston and Flaherty (1994) note that "[t]he [1994] hiring spree was a result of congressional alarm over the rising crime rate and the fact that 2,300 officers—about 60 percent of the department—were about to become eligible to retire. Congress voted to withhold the $430 million federal payment to the District for 1989 and again for 1990 until about 1,800 more officers were hired." Boston, in response to the 1981 crisis in police staffing, ultimately obtained a lump sum disbursement from the state government that helped Boston avoid deeper cuts to police department staffing.[21]

Overall, we suspect that our estimates are likely compromised somewhat by simultaneity bias. As noted in McCrary (2002), criminologists and economists have argued for several decades now that the sign of the

[19] Of course, balanced budget requirements have more bite in some jurisdictions than in others. New York City famously required a last minute loan in 1975 from the federal government to avoid insolvency, yet the city charter requires a balanced budget (Gramlich 1976). At the other end of the spectrum, Atlanta's charter holds members of the city budget commission personally liable for any deficit (Chang 1979).

[20] *Tregor v. Assessors of Boston*, 377 Mass. 602, *cert. denied* 44 U.S. 841 (1979). For background on Proposition 2½, see Massachusetts Department of Revenue (2007).

[21] A succinct discussion of the local public finance implications of Proposition 2½ and the Tregor decision is given in *Boston Firefighters Union Local 718 v. Boston Chapter NAACP, Inc.*, 468 U.S. 1206 (1984).

bias is positive, leading to an underestimate of the magnitude of the policing elasticity. Thus, the correct magnitude is likely at least as large as what our results indicate.

Before discussing results, we would like to pause to point out that the classical measurement error model imposes strong assumptions. One particularly interesting hypothesis is discussed in Levitt (1998). There, the focus is on a particular form of reporting bias, whereby crimes known to police increase when there are more police, due to an increase in citizen willingness to report crime. Levitt (1998) does not find strong evidence in support of this view.

In the next section, we discuss in detail a series of tests of implications of the classical measurement error model. Despite the many restrictions imposed by the classical measurement error model, we find surprisingly little evidence against them.

V. Results

We begin with a discussion of the first stage relationship between the two measures of police, presented in Table 3A. The first four columns of Table 3A present results in which the growth rate in the LEOKA measure is regressed on the growth rate in the ASG measure. These models correspond to what we term our "forward" regressions, models in which the LEOKA measure is employed as the endogenous measure of police that is measured with error and the ASG measure is employed as the instrumental variable. The final four columns present results arising from a regression of the ASG measure on the LEOKA measure. We refer to results arising from this formulation as our "reflected" regressions. We begin, in specification (1), by presenting a regression of the growth rate in the LEOKA measure on the growth rate in the ASG measure, controlling for the growth rate in the city's population and a vector of year dummies. In column (2), we add add a control variable for the city's expenditures exclusive of police expenditures, to capture time-varying shocks to a city's budget cycle. In column (3), we add a vector of demographic controls which capture the proportion of a city's population that is comprised of twelve age-race-gender subgroups. Finally, in column (4), we include an unrestricted set of polynomials and interactions between each of the demographic variables in order to flexibly model the effect of a city's demographic composition on its growth rate in crime. Columns (5)-(8) are equivalent to columns (1)-(4) but pertain to the reflected first stage regressions. Throughout Table 3A, and in subsequent tables, we report two sets of standard errors: Huber-Eicker-White "robust" standard errors, reported in parentheses below the coefficient estimates and robust standard errors clustered at the city level, contained in square brackets.[22] The F-statistic on the

[22]As the two sets of standard errors are very similar, statistical inferences are not dependent on the choice of clustering.

excluded instrument is reported below the coefficient estimates as a standard test of instrument relevance.[23]

The resulting coefficients provide a measure of the relatedness of the growth rates in each of the two sworn officer series. Consistent with the scatterplots presented in Figure 3, the coefficients reported in Table 3A are relatively small in magntiude, indicating that each measure contains an appreciable amount of noise. Referring for example, to column (1) of Table 3A, we observe that, conditional on the growth rate in population, a one percent increase in the ASG measure is associated with only a 0.17 percent increase in the LEOKA measure. Put differently, the growth rate in the ASG measure explains just 13 percent of the variation in the growth rate of the LEOKA measure. In each panel of column (3), the magnitude of the coefficients is relatively insensitive to the inclusion of controls for budget cycles and demographics. [24] Referring to columns (5)-(8) which report results for the reflected first stage regressions, we observe coefficients that are substantially larger in magnitude than the coefficients in columns (1)-(4). This result arises from the smaller variance in the growth rate of the LEOKA measure. [25]. Despite the larger magnitude of the coefficients, the results follow a similar pattern as the degree to which the two police measures are related depends only minimally on the inclusion of additional controls.

In Table 3B, we present least squares models of the effect of police on crime, maintaining the same table structure introduced in Table 3A. Consistent with least squares results reported by prior researchers, we report modest elasticities of crime with respect to police. We begin our discussson referring to column (1) of Table 3B, which conditions only on year fixed effects. Using the LEOKA measure of police officers, these elasticities are largest for murder (-0.26), motor vehicle theft (-0.20) and robbery (-0.15), with all three elasticities meeting the standard threshold for statistical significance. Overall, the elasticity is greater for violent crime (-0.10) than for property crime (-0.08). As with the first stage results, the estimated elasticites are extremely insensitive to the inclusion of control variables for either budget cycles or demographic composition. Referring to column (4) which adds controls for the budget cycle and changes in a city's demographic composition, the results are nearly identical to those in column (1). Columns (5)-(8) report results for models in which the growth rate in crimes is regressed on the growth rate in the ASG measure of police. [26] While the coefficients in columns (5)-(8) are smaller in magnitude, reflecting a weaker association between this measure of police and crime, they are also more precisely estimated with significant coefficients for murder (-0.17), robbery (-0.15), motor vehicle theft (-0.13) and burglary (-0.07). Taken as a

[23] The smallest F-statistic we report exceeds 109.

[24] We note that the estimated coefficient is approximately 10 percent larger with the inclusion of polynomials and interactions in demographics.

[25] This is true as the covariance between X and y is scaled by $(X'X)^{-1}$. Thus, with less variation in the "instrument," the value of the coefficient will be larger.

[26] To our knowledge, these results are, as of yet, unreported in the literature.

whole, least squares estimates of the elasticity of crime with respect to police point to an observable albeit modest relationship between changes in police manpower and criminal activity. To underscore this point, we note that a 10 percent increase in the size of a city's police force (which would correpond to an unusually large and costly change in the policy regime) is predicted to lead to only a 1 percent reduction in the rate of violent and property crimes. Accordingly, the magnitude of these elasticities has lead researchers to conclude that least squares estimates are inconsistent due to the presence of simultaneity bias.

In Tables 3C we report IV estimates of each crime elasticity that are robust to the mismeasurement of each police series. Consistent with the small first stage coefficients reported in Table 3A, comparing the estimated elasticities in Table 3C to those estimated via least squares in Table 3B yields substantial evidence of attenuation bias. In particular, referring to Table 3C, the estimated coefficients are typically four to five times larger in magnitude than those estimated via least squares. Referring to column (4) which includes the full set of control variables for the forward IV regressions, the largest elasticites are those for murder (-0.98), robbery (-0.85), motor vehicle theft (-0.77) and burglary (-0.38). In addition, we report precisely estimated elasticities for each of the two crime aggregates of -0.56 for violent crimes and -0.30 for property crimes. The elasticities arising from the reflected IV regressions reported in columns (5)-(8) exhibit a similar pattern though the estimated coefficients are substantially smaller in magnitude with elasticities for murder, robbery and motor vehicle theft of -0.68, -0.44 and -0.55, respectively. Elasticities for the crime aggregates are -0.28 for violent crimes and -0.21 for property crimes, each of which is between a third and a half smaller than those reported in column (4).

The elasticities reported in Table 3C reveal considerable attenuation in least squares coefficients resulting from the presence of measurement errors in the police series. Given the degree of the attenuation, it should be clear that measurement error is a prominent factor underlying discrepancies between least squares and IV coefficients that have been estimated in prior research. However, because the potential for simultaneity bias remains, the models estimated in Table 3C do not convincingly identify a "state-of-the-art" causal estimate of the effect of police on crime. That is, while these models remove between-city variation via first differencing and control for national crime trends, budget cycles and changes in a city's demographic composition, we are unable to rule out the existence of unit and time-varying confounders which are correlated with both changes in the size of a city's police force and its crime rate. In particular, it is possible that changes in regional macroeconomic conditions, idiosyncratic shocks to regional crime markets or changes in state-level criminal justice policies, each of which is unaccounted for in the models presented in Table 3, will lead to inconsistent parameter estimates. The omission of time-varying state-level policy variables is especially concerning as the adoption of a "get tough on crime" attitude among a state's lawmakers (or its citizens) might plausibly lead to both increases in police and more punitive sentencing policies. The result would be a positively biased police elasticity as we would mistakenly attribute some portion of increased punitiveness to the effect of increases in police manpower. Fortunately, since sentencing policy is determined almost entirely at the state level, we can address this potential source of bias with the inclusion of a set of unrestricted state-by-year fixed effects. These state-by-year effects which add an additional 1,500 parameters to the estimating equations in the paper, control for unobserved heterogeneity in the crime rate within state-years. As a testament to the explanatory power of the state-by-year effects, we note that models that include state-by-year effects explain nearly 60 percent of the annual growth rate in crime.

Tables 4A, 4B and 4C report first stage, least squares and IV results for models that include the full set of unrestricted state-by-year effects. In Table 4A, we observe that the relatedness between the LEOKA and ASG measures of police is very similar to results reported in Table 3A, with the estimated coefficient declining from approximately 0.17 to 0.16. Consistent with the extraordinary explanatory power of the state-by-year effects, we note that in both the forward and reflected first stage regressions, the effect of the control variables for budget cycles and demographic composition is greatly diminished conditioning on the state-by-year effects. However, as the relationship between the police measures is not very related to the state-by-year effects, the F-statistic on the excluded instrument remains quite high, exceeding a value of 90 in all cases.

Table 4B reports least squares estimates of the effect of police on crime, inclusive of the state-by-year effects. Referring to column (1), the elasticities for the violent and property crime aggregates are -0.13 and -0.06 respectively, with both elasticities meeting the standard threshold for statistical significance. Elasticities are largest for robbery (-0.23), murder (-0.18) and motor vehicle theft (-0.12). The reflected least squares estimates are likewise similar to those reported in Table 3B with a violent crime elasticity of -0.09 and a property crime elasticity of -0.04. Finally, in Table 4C we present IV results that correct for attentuation bias in least squares. Conditional upon state-by-year effects, we report a violent crime elasticity that is between -0.35 and -0.53 and a property crime elasticity that is between -0.15 and -0.22. The violent crime elasticities are largely similar in magnitude to those reported in Table 3C though the property crime elasticities are approximately one third smaller. With regard to the individual crimes, elasticities are largest for robbery (between -0.59 and -0.91), murder (between -0.47 and -0.87), motor vehicle theft (between -0.31 and -0.49) and burglary (between -0.13 and -0.32).

We have, thus far, privileged estimates in columns (1)-(4) of Tables 3 and 4 to estimates reported in columns (5)-(8). We do so because the primary measure of police that is employed in prior research is the LEOKA measure drawn from the FBI's Uniform Crime Reports and, as such, mismeasurement in this series is of greater relevance in drawing inferences from results reported in the extent literature. However, it is important to note that we have no *a priori* reason to prefer the estimated elasticities in columns (1)-(4) to those in columns (5)-(8). In principle, both the forward and reflected IV regressions contain valuable information in estimating the extent to which measurement error attenuates least squares coefficients. Accordingly, a state-of-the-art estimate of the effect of police on crime should draw upon information contained in both sets of estimates. In Table 5, we present pooled GMM estimates of the elasticity of crime with respect to police that efficiently combines information from both the forward and reflected IV regressions presented in Table 4C. For each crime type, Table 5 provides an estimated elasticity with robust standard errors in parentheses below the reported coefficients. The elasticities are smaller in magnitude than those reported in column (1) of Table 4C, but larger in magnitude than those in column (5), and are estimated with enhanced precision as standard errors are approximately 20 percent smaller than those estimated from the reflected IV regressions. Pooling the estimates, we report precisely estimated elasticities of -0.69 for robbery, -0.58 for murder, -0.38 for motor vehicle theft and -0.19 for burglary. With regard to the crime aggregates, we report an elasticity of -0.42 for violent crimes and -0.17 for property crimes. These estimates represent our best guess regarding the police elasticity and are our preferred estimates.

As we have noted, under classical measurement error, the forward and reflected IV regressions provide two estimates of the same underlying parameter. This observation gives rise to an overidentification test in which we can test the equality of the forward and reflected IV coefficients. As we demonstrate in the preceeding section of the paper, this overidentification test provides an omnibus test for the presence of classical measurement errors. In Table 5 we report a likelihood ratio test statistic which provides a measure of the degree to which the two parameter estimates differ. Under the null hypotheis of classical measurement error, the statistic has a χ^2 distribution with one degree of freedom. Given a critical value for the test of 3.84, an examination of Table 5 reveals that we fail to reject the null hypothesis of classical measurement error in each of nine tests. We interpret the equivalence of the IV coefficients reported in Table 4C as evidence of the existence of classical measurement error and consequently, as evidence in favor of the consistency of the estimated elasticities in Table 4C and Table 5.

We supplement the results of the omnibus tests for the presence of classical measurement error presented in Table 5 with several additional analyses that are designed to either directly or indirectly test each of the assumptions of the classicial measurement error model individually. Recall that under the three-equation classicial measurement error model presented in Section IV of the paper, the measurement errors must satisfy three conditions:

(A1) Δu_{cst} and Δv_{cst} are both independent of $\Delta \varepsilon_{cst}$,

(A2) Δu_{cst} and Δv_{cst} are both independent of ΔS^*_{cst}, and

(A3) Δu_{cst} and Δv_{cst} are independent of each other.

Condition (A1) states that the measurement errors must be independent of the residual in the outcome equation. A testable implication of (A1) is that the mesurement errors must be independent of the growth rates in each of the seven crimes we test and the two crime aggregates. Condition (A2) requires that the measurement errors be independent of the signal while condition (A3) requires that the measurement errors be independent of each other. To test these two conditions, we we introduce a third measure of police manpower and modify conditions (A1)-(A3) in the obvious ways to reflect a third measurement error. With three measures of manpower, and under the classical measurement error hypothesis, the difference between any two measures is the difference in measurement errors. Under (A2) and (A3), the difference in two measurement errors cannot be related to the third manpower measure, because the third measure is comprised of the signal, which the measurement error difference should not predict, and a third measurement error, which the measurement error difference should not predict.

Table 6 presents additional evidence on the existence of classical measurement error for three incarnations of the measurement errors, each of which corresponds to the difference between two different measures of police manpower. Our third measure of police manpower is drawn from the Law Enforcement Management and Administrative Statistics (LEMAS) series. These data, which have been collected at regular intervals from 1987-2007 provide an additional measure of police in our sample of 135 cities.[27] In Table 6, Column (1) expresses the measurement error as the difference between the growth rate in the LEOKA measure and the growth rate in the LEMAS measure while columns (2) and (3) use the difference between the LEOKA measure and the ASG measure and the difference between the LEMAS measure and the ASG measure, respectively. We begin in Panel A of Table 6, by regressing the growth rate in each of the nine crime types on each incarnation of the measurement error, conditional on the growth rate in population. To the extent that the measurement error is not correlated with the growth rate in each type of crime, it should not be correlated with ϵ, the error term in the structural equation. Table 6 provides twenty-seven tests of this hypothesis, three for each crime type. Notably, we fail to reject the null hypothesis of a relationship between the measurement errors and growth rate in crime in all cases.[28]

[27] For additional details regarding the LEMAS series, please see the data appendix to the paper.

[28] We also test the joint significance of the seven crime categories in explaining the measurement error. In each case, we fail

Next, in panel B of Table 6, we provide a joint test of conditions (A2) and (A3), as discussed above. An examination of the coefficients for each of the three incarnations of the measurement errors reveals that we fail to reject that the measurement errors are related to the signal.

VI. Discussion

The IV estimates reported in the previous section of this paper can be thought of as police elasticities that are robust to errors in the measurement of police. Conditional upon only year fixed effects, we find elasticities of violent and property crimes with respect to police of between -0.25 and -0.5 and -0.2 and -0.26, respectively. Conditioning on a set of fully interacted state-by-year effects, and pooling estimates from our forward and reflected IV regressions, we report precisely estmated elasticities of -0.4 for violent crimes and -0.2 for property crimes, with especially large elasticities for robbery (-0.69), murder (-0.58), motor vehicle theft (-0.38) and burglary (-0.19).

In this section, we contextualize these findings by comparing our reported elasticities to those in the prior literature. Table 7 presents police elasticities from seven recent papers, each of which aims to correct for simultaneity bias, which our estimates do not adjust for. Under the classical measurement error hypothesis, these estimates jointly address bias arising from simultaneity and measurement errors. We compare each of these estimated elasticities to those reported in this paper.

Prior research typically finds that police have a larger protective effect on violent crimes than on property crimes. Violent crime elasticities that meet the standard threshold for statistical significance range from -0.44 to -0.99. An additional set of estimates using mayoral and gubernatorial elections as instruments, reported by Levitt (1997) and McCrary (2002) report an elasticity that is similar in magnitude though is not precisely estimated. With regard to the individual crimes, elasticities that meet the threshold of significance are typically largest for murder (-0.84 and -0.91) and robbery (-1.34). However, despite consistently large point estimates, results often remain insignificant due to the presence of correspondingly large standard errors. For example, McCrary (2002) and Levitt (2002) report robbery elasticities of -0.98 and -0.45, respectively, though both estimates are small relative to their standard error. With regard to property crimes, overall elasticities are insignificant in two of three aggregate data analyses, with point estimates ranging from 0 to -0.5. Elasticities for motor vehicle theft and burglary are typically largest with reported elasticities for motor vehicle theft of between -0.3 and -0.8 and for burglary of between -0.3 and -0.6.

Though each of the studies spans different numbers of cities and time periods, it is apparent that the

to reject the null hypothesis that the seven coefficients are jointly different than zero.

elasticities reported in this paper are quite similar to those reported in prior research. Since our estimated elasticities are robust to measurement error and state and time-varying omitted variables but not the presence of simultaneity between police and crime, our research implies a smaller role for simultaneity than has been suggested by prior studies. In particular, the evidence appears to support the proposition that changes in police hiring are often idiosyncratic and that it is difficult for cities to hire police during, or in anticipation of, a crime wave - at least in the short run. While we continue to view our results as representing a lower bound on the police elasticity, we note that an advantage of this approach is that we report elasticities that are more precisely estimated than a majority of the results in the prior literature.

VII. Conclusion

In this paper, we have presented estimates of the elasticity of crime with respect to police for index offenses: murder, rape, robbery, assault, burglary, larceny, and motor vehicle theft. These estimates are based on annual data on crime and police in a panel data set of 135 cities observed from 1960-2008. Our specifications model year-over-year growth rates in crime as a function of the year-over-year growth rate in the number of sworn officers from the year preceding, as well as a large number of control variables including year effects, state-by-year effects, budget cycles, and demographic controls.

Our main focus is on IV estimates where one noisy measure of the growth rate in police per capita is instrumented using another noisy measure of the growth rate in police. Under the classical measurement error model, the errors in measurement in one proxy are independent of the errors in measurement of the other proxy and of unobserved factors influencing the growth rate in crime. These assumptions imply that IV is consistent for the elasticity of crime with respect to police.

One implication of the classical measurement error hypothesis is that there are two consistent IV estimators. The first instruments one noisy measure of the growth rate in police per capita with another noisy measure of the growth rate in police. The second instruments the other noisy measure of the growth rate in police per capita with the one. That two consistent estimators are available for the police elasticity suggests pooling the two estimates to arrive at an efficient minimum chi-square estimate of the police elasticity. This approach also yields an immediate test of the classical measurement error hypothesis in the form of the minimized value of the test statistic. Generally speaking, there is little evidence in these tests against the null hypothesis of classical measurement error.

Our focus on measurement error stands in contrast to the previous literature, which has instead emphasized the potential for simultaneity bias, whereby cities or perhaps police departments take heed of ongoing and possibly upcoming trends in crime and hire police officers accordingly. We have instead emphasized the variety of institutional considerations that make such lifecycle optimization challenging for cities and departments, including tax and expenditure limitations, the *de facto* requirement to balance the city budget under state constitution, state statute, or city charter, and the predominance of policing costs as a fraction of the city budget. An additional consideration is that cities and departments may simply fail to optimize appropriately. For example, many cities report staffing difficulties in the wake of retirement booms, but these are of course largely predictable based on simple actuarial projections using years of service and year hired. As an empirical matter, cities seem to engage in boom and bust hiring with relatively little attention paid to the level of crime and relatively more attention paid to shortfalls of police staffing from recent norms.

Consistent with this reasoning, our estimates are robust to the inclusion of a variety of control variables that have direct bearing on crime, in particular budget cycles and demographic variables. Indeed, our estimates are robust to the inclusion of unrestricted state-by-year effects. This implies that our estimates represent a pure effect of policing that cannot be attributed to, for example, changes to punishment policy since those are made at the state level. Our best guess regarding the elasticity of crime with respect to police is -0.5 for violent crime and -0.25 for property crime. Crime categories where police seem to be most effective are murder, robbery, and motor vehicle theft. Our estimates are similar to those found in the previous literature, but are somewhat more precisely estimated.

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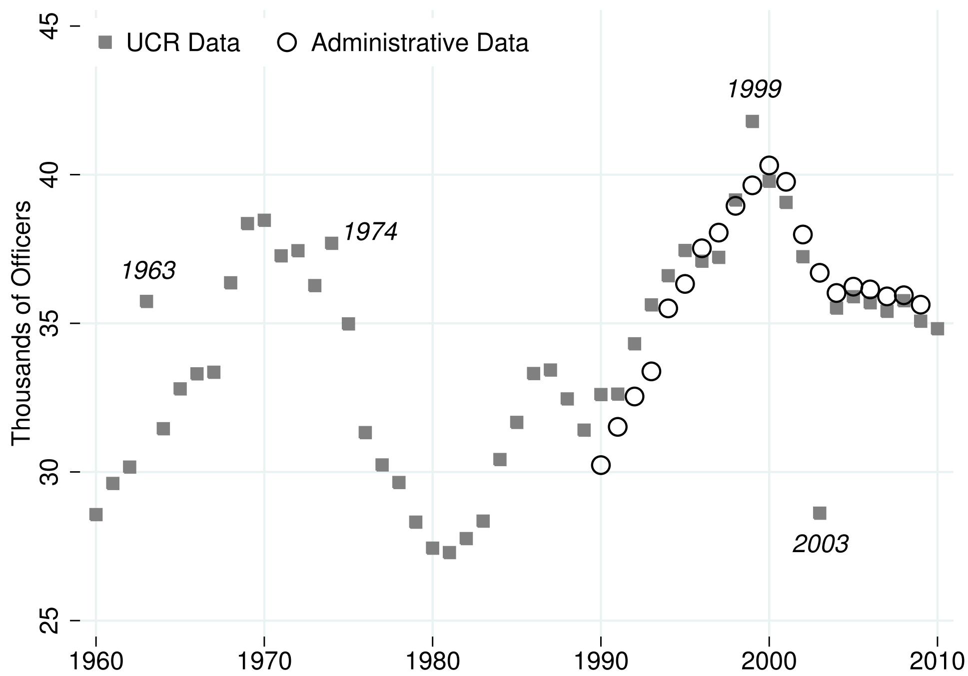
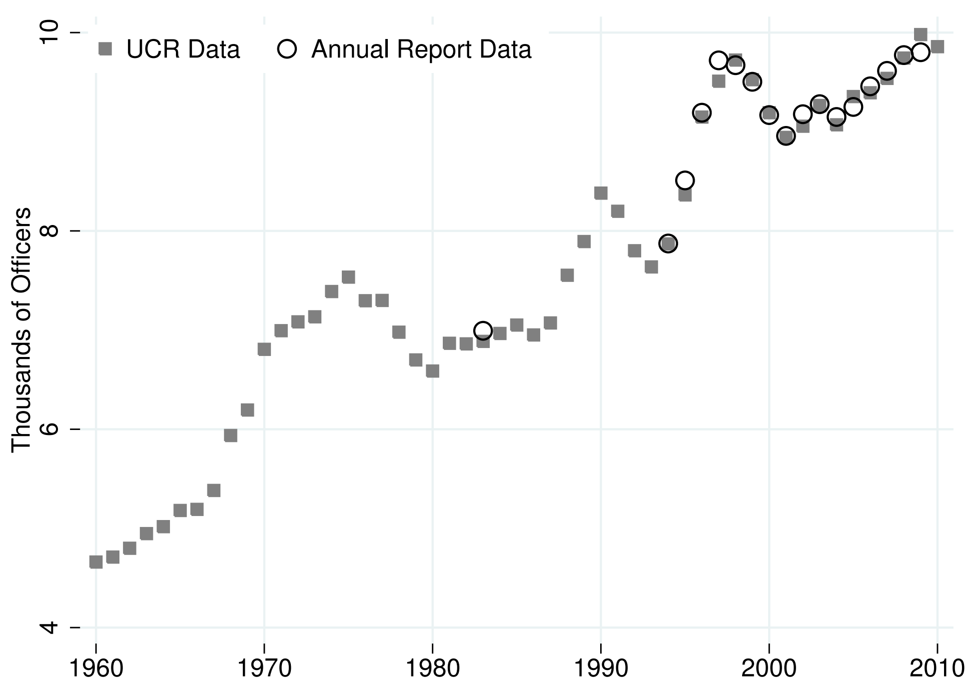
Figure 1. Sworn Officers in Five Cities:
the Uniform Crime Reports and Direct Measures from Departments

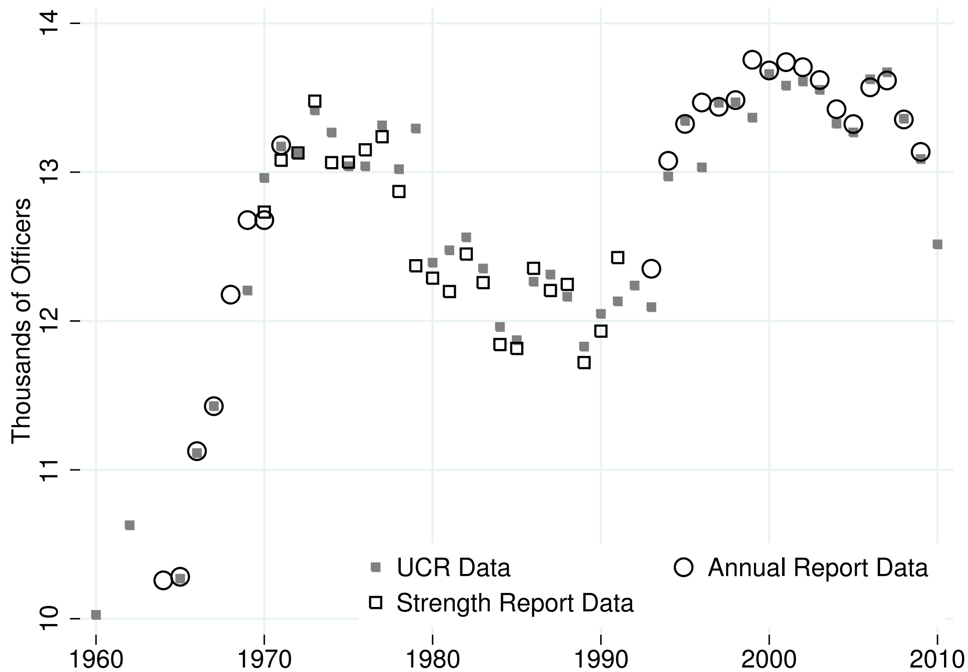
A. New York



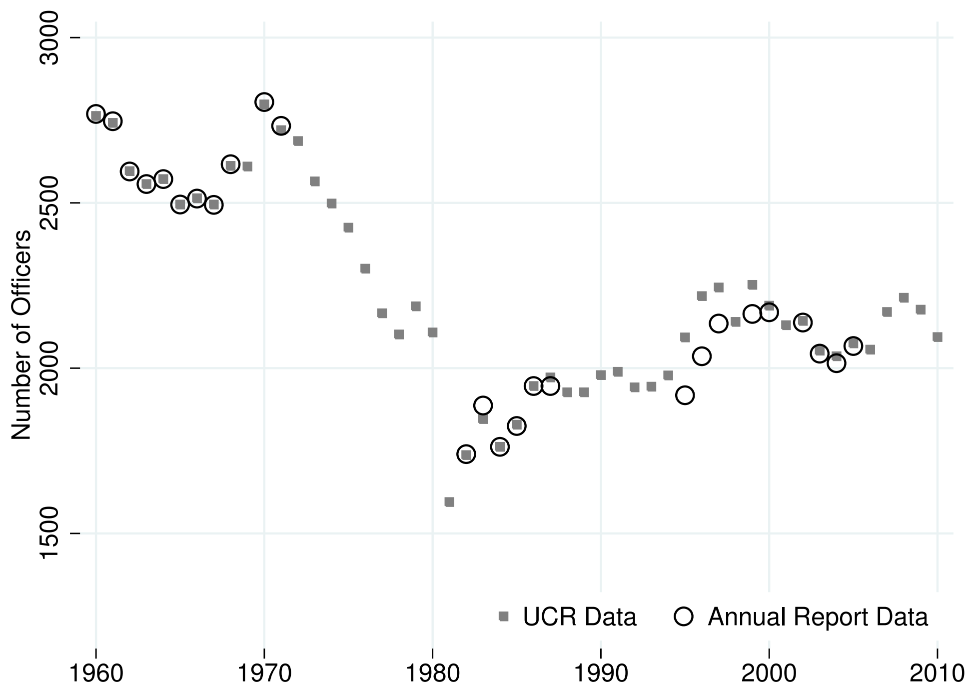
B. Los Angeles



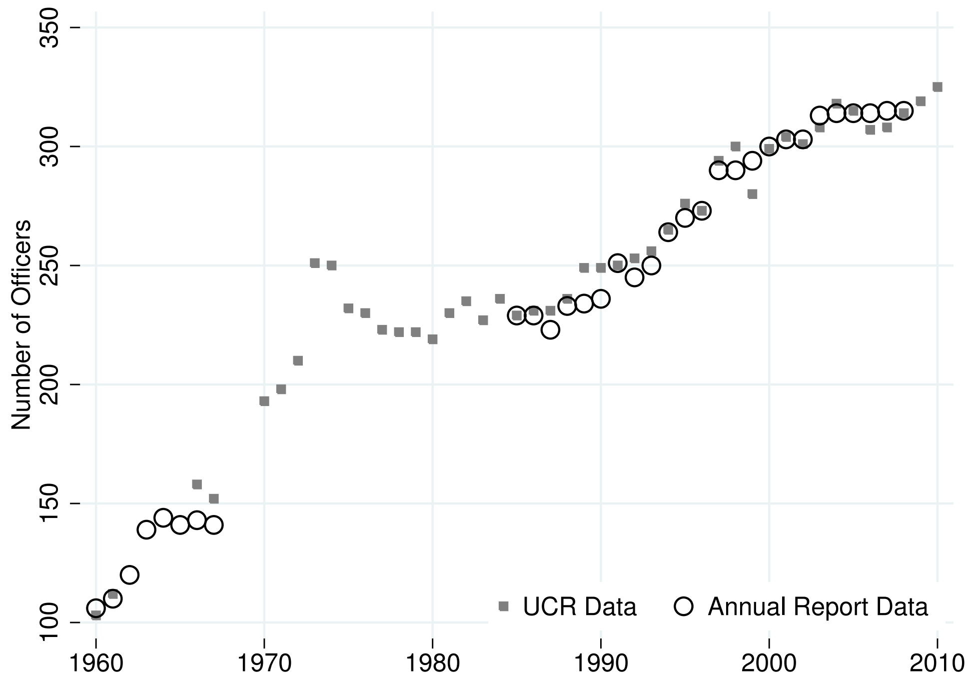
C. Chicago



D. Boston

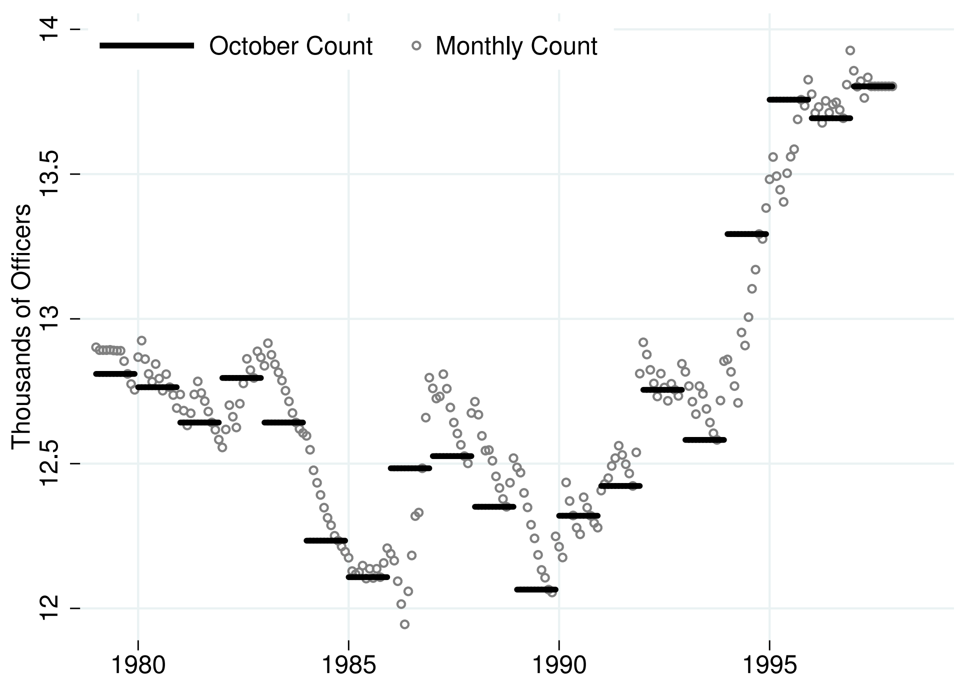


E. Lincoln, Nebraska



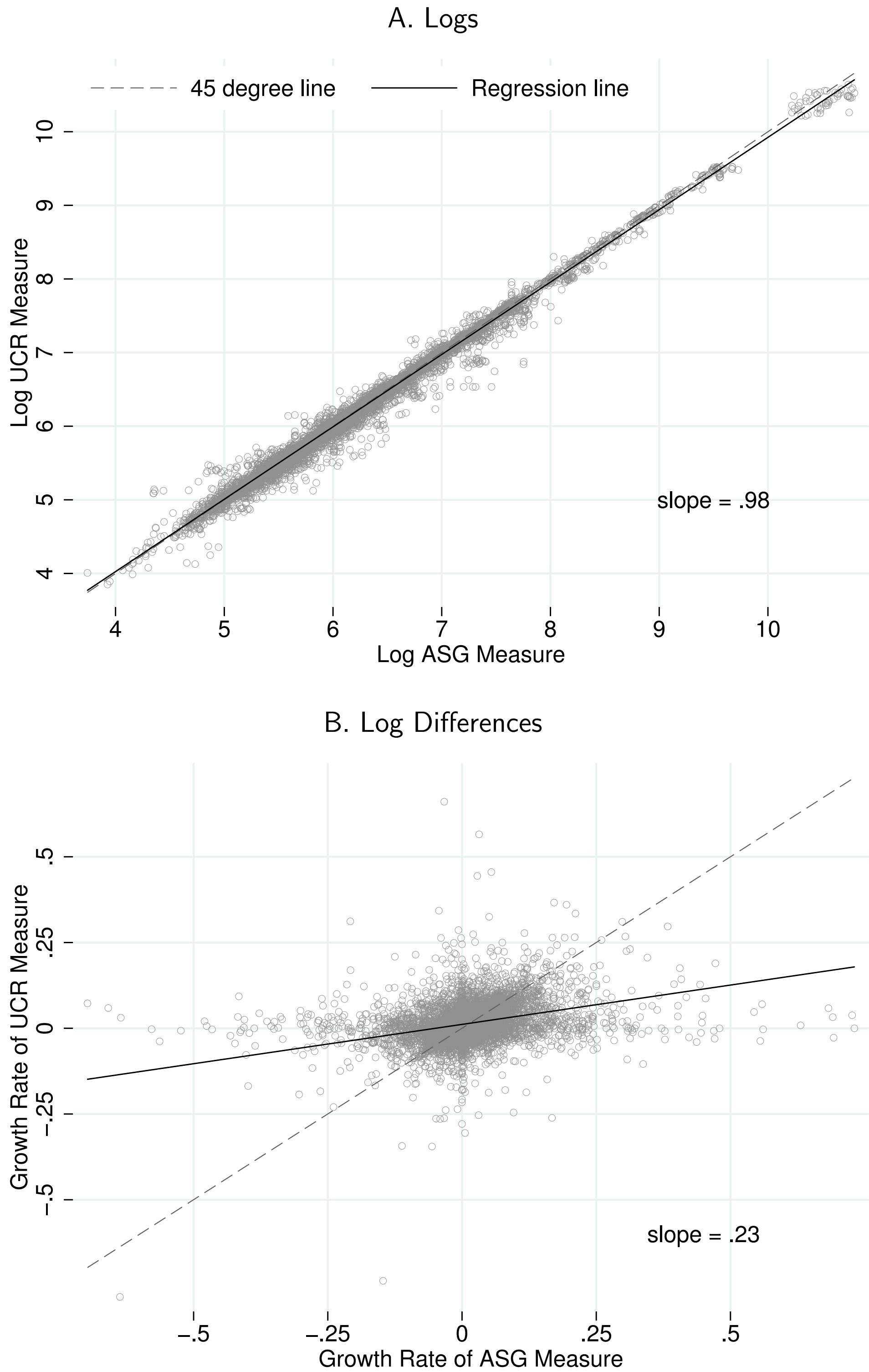
Note: In panel A, numbers for 1960-1994 are adjusted to account for the 1995 merger of NYPD with housing and transit police. See Data Appendix for details.

Figure 2. Sworn Officers in Chicago 1979-1997, by Month



Note: See text for details.

Figure 3. Two Leading Measures of Sworn Officers: the Uniform Crime Reports and the Annual Survey of Government



Note: See text and Data Appendix for details.

FIGURE 4. LOCATION OF CITIES IN SAMPLE

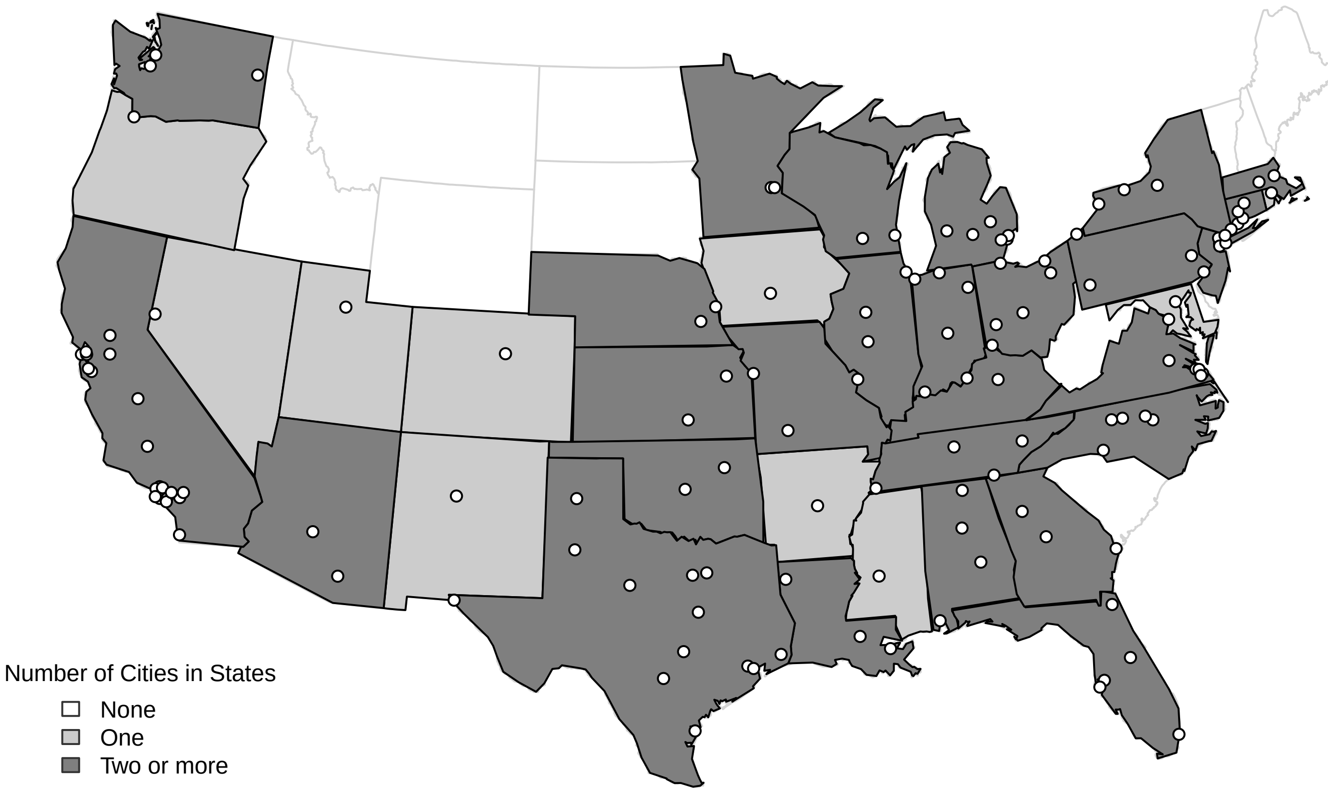
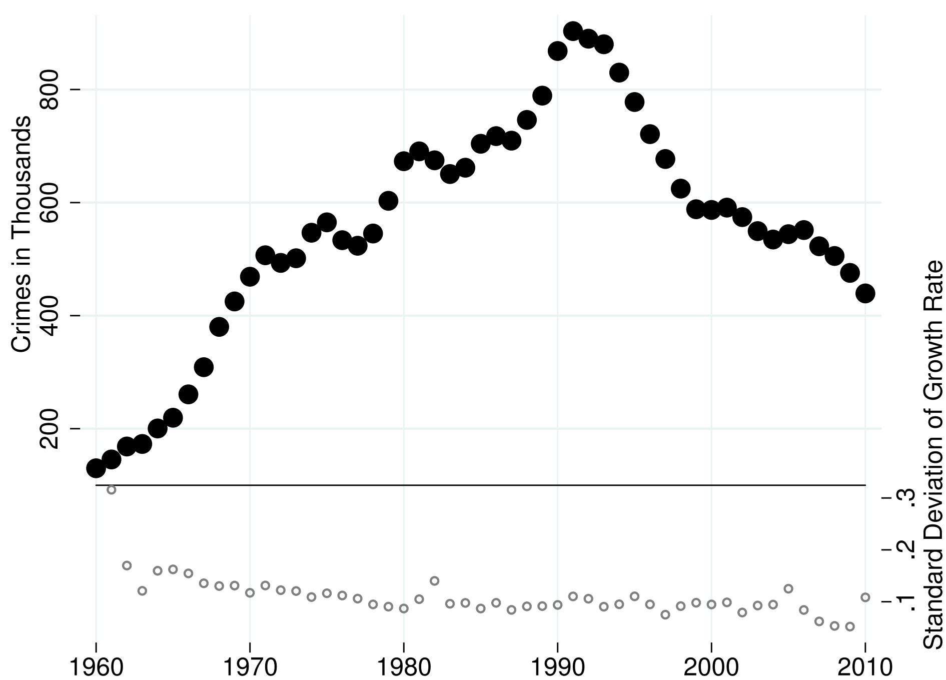
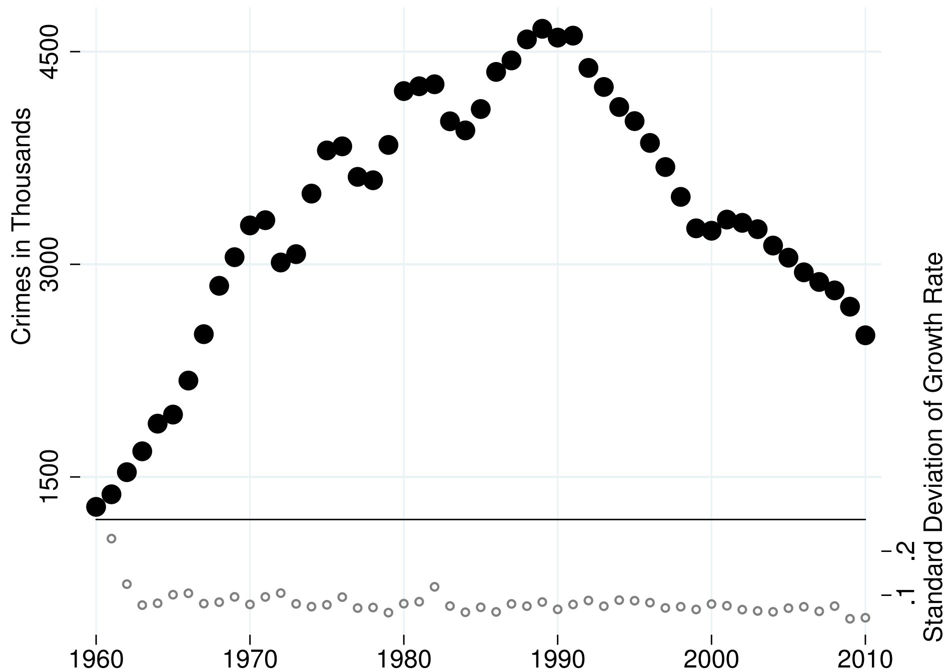


FIGURE 5. AGGREGATE TRENDS IN VIOLENT AND PROPERTY CRIME AND POLICE: EVIDENCE FROM THE UNIFORM CRIME REPORTS

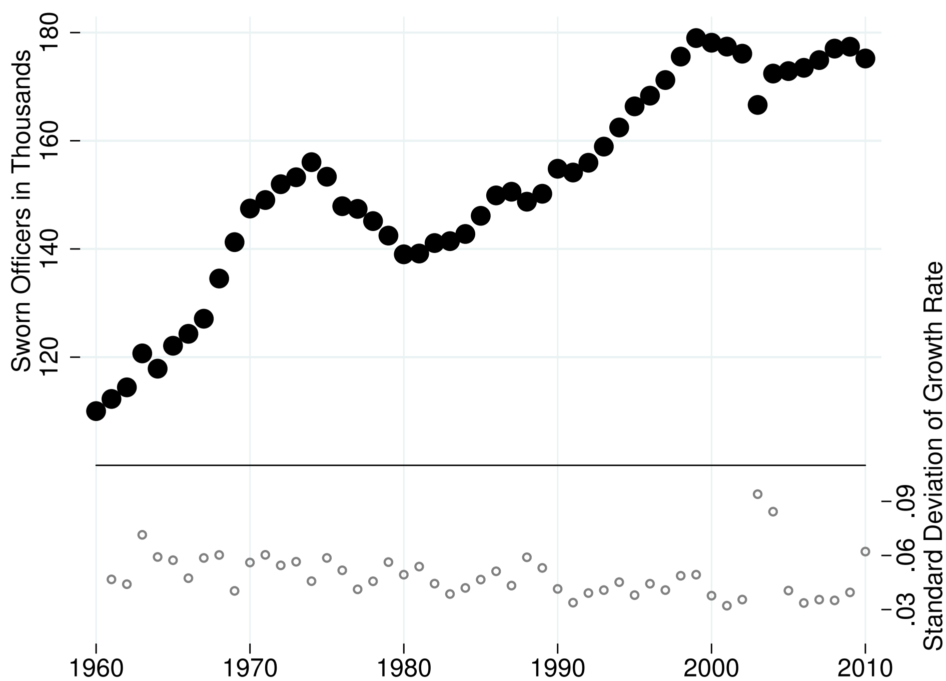
A. Violent Crime: Murder, Rape, Robbery, Aggravated Assault



B. Property Crime: Burglary, Larceny, Motor Vehicle Theft



C. Sworn Police



Note: In the UCR data, larceny is defined to exclude motor vehicle theft. Solid circles give totals and open circles give standard deviations of year-over-year growth rates. See text and Data Appendix for details.

Table 1. Correlation of UCR and Police Department Measures of Number of Sworn Personnel

Measure	New York	Los Angeles	Chicago	Boston	Lincoln
Log Sworn Police	0.65	0.99	0.96	0.98	0.99
Growth Rate	0.32	0.92	0.65	0.94	0.45

Note: Table entries are correlation coefficients between the UCR measure of the number of sworn police and a measure of the number of sworn police taken from police department reports. Annual report data for Boston in 1982 are omitted from the calculations.

Table 2. Summary Statistics on Police and Crime

Variable	N		Levels (per 100,000 population) Mean	S.D.	Min.	Max.	Log Differences Mean	S.D.	Min.	Max.
Sworn police (LEOKA)	6,615	O	257.4	116.8	76.0	786.6	0.016	0.055	-0.783	0.661
		B		109.4				0.012		
		W		40.8				0.053		
Sworn police (ASG)	6,615	O	269.8	132.8	74.1	787.7	0.016	0.074	-0.637	0.731
		B		123.0				0.012		
		W		50.2				0.073		
Violent crimes	6,544	O	992.7	634.8	16.6	4,189.0	0.038	0.138	-0.610	1.493
		B		406.0				0.020		
		W		484.2				0.137		
Property crimes	6,606	O	6,154.2	2,386.4	897.2	16,933.4	0.018	0.106	-0.585	0.973
		B		1,271.5				0.014		
		W		2,019.6				0.105		
Murder	6,609	O	15.4	10.5	0.5	110.9	0.188	0.225	-1.386	1.705
		B		7.9				0.012		
		W		6.9				0.225		
Rape	6,544	O	48.5	29.5	0.5	216.3	0.034	0.209	-2.485	2.922
		B		16.1				0.027		
		W		24.7				0.207		
Robbery	6,609	O	465.2	351.7	3.1	2,358.0	0.038	0.173	-0.693	1.139
		B		243.0				0.018		
		W		254.2				0.172		
Assault	6,609	O	486.4	337.7	1.4	2,617.7	0.038	0.169	-0.933	2.015
		B		200.2				0.022		
		W		271.9				0.1667		
Burglary	6,609	O	1,653.5	809.4	143.7	4,849.0	0.012	0.134	-0.919	0.792
		B		400.8				0.017		
		W		703.1				0.133		
Larceny	6,606	O	3,582.5	1,490.7	148.0	10,118.9	0.019	0.115	-1.031	1.138
		B		913.5				0.014		
		W		1,178.6				0.114		
Motor vehicle theft	6,609	O	918.1	572.8	65.1	5,217.4	0.018	0.157	-0.667	1.120
		B		356.9				0.017		
		B		447.9				0.156		

Note: This table reports descriptive statistics for the two measures of sworn police officers used throughout the article as well as for each of the seven crime categories and two crime aggregates. For each variable, we report the overall mean, the standard deviation decomposed into overall ("O"), between ("B"), and within ("W") variation, as well as the minimum and maximum values, in levels and growth rates. Results are weighted by 2008 city population.

Table 3A. First Stage Models

	EC = LEOKA Measure INS = ASG Measure				EC = ASG Measure INS = LEOKA Measure			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ASG measure	0.174 (0.016) [0.020]	0.174 (0.016) [0.020]	0.171 (0.016) [0.020]	0.193 (0.018) [0.022]				
LEOKA measure					0.369 (0.034) [0.040]	0.367 (0.034) [0.042]	0.364 (0.034) [0.044]	0.393 (0.038) [0.052]
F-statistic	116.0	112.2	117.7	114.0	117.5	114.4	113.6	109.7
N	6,750	6,615	6,615	6,615	6,750	6,615	6,615	6,615
budget cycles	no	yes	yes	yes	no	yes	yes	yes
demographics	no	no	yes	yes	no	no	yes	yes
polynomials and interactions	no	no	no	yes	no	no	no	yes

Note: Each column reports results of an OLS regression of the growth rate in a given measurement of the number of per capita police officers on the the growth rate in the other measurement. Columns (1)-(4) report results for the models in which the LEOKA measure is employed as the endogenous covariate and the ASG measure is employed as the instrumental variable while columns (5)-(8) report results for models in which the ASG measure is employed as the endogenous covariate and the LEOKA measure is employed as the instrumental variable. For each set of models, the first column reports regression results, conditional on the growth rate in the city's population and a vector of year effects. The second column adds a control variable for the city's per capita expenditures exclusive of police expenditures. In the third column we add demographic controls which capture the proportion of a city's population that is comprised of each of twelve age-gender-race groups. Finally, in th fourth column, we add polynomial terms and a full set of interactions of the demographic variables. All models are estimated using 2010 city population weights. Two sets of standard errors are reported below the coefficient estimates. The top row reports Huber-Eicker-White standard errors that are robust to heteroskedasticity. The standard errors reported in the second row are clustered at the city level.

Table 3B. Least Squares Models of the Effect of Police on Crime

	LEOKA Measure				ASG Measure			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Violent crimes	-0.097	-0.098	-0.108	-0.103	-0.091	-0.092	-0.100	-0.100
	(0.055)	(0.054)	(0.048)	(0.047)	(0.023)	(0.023)	(0.023)	(0.023)
	[0.043]	[0.043]	[0.037]	[0.037]	[0.021]	[0.021]	[0.023]	[0.023]
Murder	-0.263	-0.262	-0.260	-0.255	-0.163	-0.162	-0.165	-0.170
	(0.062)	(0.062)	(0.062)	(0.062)	(0.040)	(0.040)	(0.040)	(0.040)
	[0.065]	[0.066]	[0.065]	[0.066]	[0.039]	[0.039]	[0.039]	[0.039]
Rape	0.018	0.016	0.008	0.019	-0.035	-0.037	-0.037	-0.032
	(0.060)	(0.060)	(0.059)	(0.060)	(0.041)	(0.041)	(0.039)	(0.039)
	[0.063]	[0.062]	[0.059]	[0.064]	[0.042]	[0.042]	[0.039]	[0.037]
Robbery	-0.147	-0.148	-0.164	-0.164	-0.135	-0.136	-0.145	-0.148
	(0.099)	(0.099)	(0.087)	(0.084)	(0.030)	(0.030)	(0.030)	(0.030)
	[0.076]	[0.075]	[0.064]	[0.064]	[0.024]	[0.024]	[0.025]	[0.026]
Assault	-0.029	-0.030	-0.030	-0.022	-0.053	-0.054	-0.060	-0.058
	(0.046)	(0.046)	(0.046)	(0.045)	(0.028)	(0.028)	(0.028)	(0.028)
	[0.039]	[0.039]	[0.039]	[0.039]	[0.029]	[0.029]	[0.030]	[0.030]
Property crimes	-0.075	-0.075	-0.078	-0.076	-0.046	-0.046	-0.051	-0.051
	(0.036)	(0.036)	(0.032)	(0.031)	(0.022)	(0.022)	(0.021)	(0.021)
	[0.024]	[0.024]	[0.023]	[0.024]	[0.012]	[0.012]	[0.013]	[0.013]
Burglary	-0.033	-0.034	-0.039	-0.039	-0.056	-0.057	-0.065	-0.066
	(0.074)	(0.074)	(0.064)	(0.062)	(0.025)	(0.026)	(0.025)	(0.025)
	[0.043]	[0.043]	[0.038]	[0.039]	[0.016]	[0.016]	[0.018]	[0.018]
Larceny	-0.046	-0.046	-0.047	-0.043	-0.018	-0.017	-0.020	-0.020
	(0.035)	(0.035)	(0.033)	(0.032)	(0.024)	(0.024)	(0.023)	(0.022)
	[0.025]	[0.024]	[0.025]	[0.025]	[0.016]	[0.016]	[0.015]	[0.015]
Motor vehicle theft	-0.196	-0.195	-0.203	-0.201	-0.131	-0.130	-0.133	-0.133
	(0.058)	(0.058)	(0.053)	(0.052)	(0.036)	(0.036)	(0.035)	(0.035)
	[0.046]	[0.046]	[0.044]	[0.045]	[0.038]	[0.038]	[0.041]	[0.040]
budget cycles	no	yes	yes	yes	no	yes	yes	yes
demographics	no	no	yes	yes	no	no	yes	yes
polynomials and interactions	no	no	no	yes	no	no	no	yes

Note: Each column reports results of a least squares regression of thegrowth rate in each of nine crime rates on the first lag of the growth rate in the number of per capita sworn police officers. Columns (1)-(4) report results for the models in which the LEOKA measure is employed as the measure of police while columns (5)-(8) report results for models in which the ASG measure is employed as the measure of police. For each set of models, the first column reports regression results, conditional on the growth rate in the city's population and a vector of year effects. The second column adds a control variable for the city's per capita expenditures exclusive of police expenditures. In the third column we add demographic controls which capture the proportion of a city's population that is comprised of each of twleve age-gender-race groups. Finally, in the fourth column, we add polynomial terms and selected interactions of the demographic variables. All models are estimated using 2010 city population weights. Two sets of standard errors are reported below the coefficient estimates. The top row reports Huber-Eicker-White standard errors that are robust to heteroskedasticity . The standard errors reported in the second row are clustered at the city level.

Table 3C. 2SLS Models of the Effect of Police on Crime

	EC = LEOKA Measure INS = ASG Measure				EC = ASG Measure INS = LEOKA Measure			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Violent crimes	-0.497	-0.503	-0.557	-0.564	-0.257	-0.260	-0.286	-0.276
	(0.126)	(0.127)	(0.126)	(0.127)	(0.141)	(0.140)	(0.123)	(0.120)
	[0.114]	[0.115]	[0.125]	[0.128]	[0.098]	[0.097]	[0.084]	[0.087]
Murder	-0.914	-0.913	-0.942	-0.978	-0.696	-0.696	-0.694	-0.684
	(0.234)	(0.235)	(0.237)	(0.239)	(0.170)	(0.170)	(0.169)	(0.170)
	[0.222]	[0.222]	[0.223]	[0.226]	[0.164]	[0.165]	[0.170]	[0.171]
Rape	-0.192	-0.201	-0.207	-0.178	0.047	0.043	0.021	0.050
	(0.222)	(0.222)	(0.217)	(0.215)	(0.159)	(0.159)	(0.157)	(0.159)
	[0.232]	[0.231]	[0.221]	[0.211]	[0.168]	[0.167]	[0.158]	[0.172]
Robbery	-0.754	-0.762	-0.825	-0.852	-0.388	-0.393	-0.438	-0.439
	(0.171)	(0.173)	(0.170)	(0.170)	(0.255)	(0.254)	(0.222)	(0.213)
	[0.130]	[0.132]	[0.142]	[0.148]	[0.168]	[0.168]	[0.140]	[0.140]
Assault	-0.300	-0.304	-0.343	-0.332	-0.077	-0.079	-0.079	-0.059
	(0.159)	(0.160)	(0.162)	(0.164)	(0.122)	(0.122)	(0.122)	(0.119)
	[0.167]	[0.168]	[0.176]	[0.178]	[0.102]	[0.102]	[0.103]	[0.103]
Property crimes	-0.260	-0.260	-0.289	-0.296	-0.198	-0.198	-0.209	-0.205
	(0.123)	(0.124)	(0.120)	(0.119)	(0.094)	(0.094)	(0.085)	(0.081)
	[0.063]	[0.063]	[0.069]	[0.071]	[0.061]	[0.061]	[0.063]	[0.063]
Burglary	-0.315	-0.318	-0.371	-0.383	-0.088	-0.090	-0.105	-0.104
	(0.143)	(0.145)	(0.145)	(0.144)	(0.196)	(0.196)	(0.169)	(0.165)
	[0.082]	[0.082]	[0.096]	[0.100]	[0.106]	[0.106]	[0.095]	[0.097]
Larceny	-0.099	-0.097	-0.113	-0.115	-0.122	-0.121	-0.124	-0.115
	(0.132)	(0.133)	(0.129)	(0.128)	(0.092)	(0.092)	(0.087)	(0.084)
	[0.090]	[0.092]	[0.088]	[0.087]	[0.067]	[0.066]	[0.070]	[0.070]
Motor vehicle	-0.733	-0.733	-0.759	-0.766	-0.518	-0.519	-0.542	-0.539
	(0.205)	(0.206)	(0.204)	(0.205)	(0.151)	(0.152)	(0.141)	(0.137)
	[0.189]	[0.189]	[0.215]	[0.216]	[0.107]	[0.108]	[0.117]	[0.120]
budget cycles	no	yes	yes	yes	no	yes	yes	yes
demographics	no	no	yes	yes	no	no	yes	yes
polynomials and interactions	no	no	no	yes	no	no	no	yes

Note: Each column reports results of a 2SLS regression of the growth rate in each of nine crime rates on the first lag of the growth rate in the number of per capita sworn police officers. Columns (1)-(4) report results for the models in which the LEOKA measure is employed as the endogenous covariate and the ASG measure is employed as the instrumental variable while columns (5)-(8) report results for models in which the ASG measure is employed as the endogenous covariate and the LEOKA measure is employed as the instrumental variable. For each set of models, the first column reports regression results, conditional on the growth rate in the city's population and a vector of year effects. The second column adds a control variable for the city's per capita expenditures exclusive of police expenditures. In the third column we add demographic controls which capture the proportion of a city's population that is comprised of each of twleve age-gender-race groups. Finally, in the fourth column, we add polynomial terms and selected interactions of the demographic variables. All models are estimated using 2010 city population weights. Two sets of standard errors are reported below the coefficient estimates. The top row reports Huber-Eicker-White standard errors that are robust to heteroskedasticity . The standard errors reported in the second row are clustered at the city level.

Table 4A. First Stage Models
Within-State Differences

	EC = LEOKA Measure INS = ASG Measure				EC = ASG Measure INS = LEOKA Measure			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ASG measure	0.162 (0.017) [0.018]	0.162 (0.017) [0.018]	0.162 (0.017) [0.018]	0.162 (0.017) [0.018]				
LEOKA measure					0.383 (0.032) [0.033]	0.383 (0.032) [0.034]	0.384 (0.032) [0.034]	0.384 (0.032) [0.034]
F-statistic	94.2	91.9	91.2	90.0	145.9	143.1	141.8	140.3
N	6,750	6,615	6,615	6,615	6,750	6,615	6,615	6,615
budget cycles	no	yes	yes	yes	no	yes	yes	yes
demographics	no	no	yes	yes	no	no	yes	yes
polynomials and interactions	no	no	no	yes	no	no	no	yes

Note: Each column reports results of an OLS regression of the growth rate in a given measurement of the number of per capita police officers on the the growth rate in the other measurement. Columns (1)-(4) report results for the models in which the LEOKA measure is employed as the endogenous covariate and the ASG measure is employed as the instrumental variable while columns (5)-(8) report results for models in which the ASG measure is employed as the endogenous covariate and the LEOKA measure is employed as the instrumental variable. For each set of models, the first column reports regression results, conditional on the growth rate in the city's population and an unrestricted set of state-by-year effects. The second column adds a control variable for the city's per capita expenditures exclusive of police expenditures. In the third column we add demographic controls which capture the proportion of a city's population that is comprised of each of twelve age-gender-race groups. Finally, in th fourth column, we add polynomial terms and a full set of interactions of the demographic variables. All models are estimated using 2010 city population weights. Two sets of standard errors are reported below the coefficient estimates. The top row reports Huber-Eicker-White standard errors that are robust to heteroskedasticity. The standard errors reported in the second row are clustered at the city level.

Table 4B. Least Squares Models of the Effect of Police on Crime
Within-State Differences

	LEOKA Measure				ASG Measure			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Violent crimes	-0.136	-0.136	-0.133	-0.132	-0.087	-0.087	-0.086	-0.087
	(0.044)	(0.044)	(0.044)	(0.044)	(0.023)	(0.023)	(0.023)	(0.023)
Murder	-0.187	-0.186	-0.183	-0.184	-0.138	-0.136	-0.137	-0.141
	(0.086)	(0.086)	(0.086)	(0.088)	(0.050)	(0.050)	(0.050)	(0.050)
Rape	-0.002	-0.002	0.007	0.007	-0.002	-0.002	0.005	0.005
	(0.076)	(0.076)	(0.076)	(0.076)	(0.045)	(0.045)	(0.045)	(0.044)
Robbery	-0.234	-0.234	-0.233	-0.231	-0.146	-0.146	-0.148	-0.148
	(0.054)	(0.054)	(0.054)	(0.055)	(0.031)	(0.031)	(0.031)	(0.031)
Assault	-0.034	-0.033	-0.028	-0.027	-0.044	-0.044	-0.042	-0.043
	(0.057)	(0.057)	(0.057)	(0.057)	(0.032)	(0.032)	(0.032)	(0.032)
Property crimes	-0.061	-0.061	-0.057	-0.057	-0.036	-0.036	-0.034	-0.036
	(0.030)	(0.030)	(0.030)	(0.030)	(0.016)	(0.016)	(0.016)	(0.016)
Burglary	-0.055	-0.055	-0.052	-0.052	-0.054	-0.054	-0.052	-0.052
	(0.044)	(0.044)	(0.044)	(0.044)	(0.024)	(0.024)	(0.024)	(0.024)
Larceny	-0.028	-0.028	-0.022	-0.022	-0.015	-0.014	-0.012	-0.015
	(0.032)	(0.032)	(0.032)	(0.032)	(0.019)	(0.018)	(0.018)	(0.018)
Motor vehicle theft	-0.126	-0.126	-0.124	-0.121	-0.081	-0.080	-0.080	-0.080
	(0.055)	(0.055)	(0.055)	(0.055)	(0.028)	(0.028)	(0.028)	(0.028)
budget cycles	no	yes	yes	yes	no	yes	yes	yes
demographics	no	no	yes	yes	no	no	yes	yes
polynomials and interactions	no	no	no	yes	no	no	no	yes

Note: Each column reports results of a least squares regression of thegrowth rate in each of nine crime rates on the first lag of the growth rate in the number of per capita sworn police officers. Columns (1)-(4) report results for the models in which the LEOKA measure is employed as the measure of police while columns (5)-(8) report results for models in which the ASG measure is employed as the measure of police. For each set of models, the first column reports regression results, conditional on the growth rate in the city's population and an unrestricted set of state-by-year effects. The second column adds a control variable for the city's per capita expenditures exclusive of police expenditures. In the third column we add demographic controls which capture the proportion of a city's population that is comprised of each of twelve age-gender-race groups. Finally, in the fourth column, we add polynomial terms and selected interactions of the demographic variables. All models are estimated using 2010 city population weights. Two sets of standard errors are reported below the coefficient estimates. The top row reports Huber-Eicker-White standard errors that are robust to heteroskedasticity . The standard errors reported in the second row are clustered at the city level.

Table 4C. 2SLS Models of the Effect of Police on Crime
Within-State Differences

	EC = LEOKA Measure INS = ASG Measure				EC = ASG Measure INS = LEOKA Measure			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Violent crimes	-0.526	-0.524	-0.524	-0.531	-0.345	-0.344	-0.339	-0.337
	(0.136)	(0.136)	(0.137)	(0.139)	(0.108)	(0.108)	(0.109)	(0.110)
Murder	-0.834	-0.824	-0.838	-0.866	-0.472	-0.471	-0.468	-0.473
	(0.304)	(0.302)	(0.305)	(0.308)	(0.214)	(0.214)	(0.216)	(0.221)
Rape	-0.012	-0.015	0.031	0.030	-0.004	-0.004	0.018	0.019
	(0.259)	(0.259)	(0.258)	(0.256)	(0.182)	(0.182)	(0.183)	(0.185)
Robbery	-0.886	-0.885	-0.903	-0.912	-0.592	-0.592	-0.595	-0.593
	(0.187)	(0.186)	(0.189)	(0.190)	(0.132)	(0.132)	(0.133)	(0.135)
Assault	-0.269	-0.266	-0.256	-0.263	-0.085	-0.085	-0.072	-0.068
	(0.185)	(0.185)	(0.187)	(0.189)	(0.139)	(0.139)	(0.140)	(0.140)
Property crimes	-0.219	-0.216	-0.207	-0.219	-0.154	-0.154	-0.144	-0.145
	(0.094)	(0.094)	(0.093)	(0.093)	(0.070)	(0.071)	(0.070)	(0.071)
Burglary	-0.328	-0.326	-0.317	-0.322	-0.139	-0.139	-0.134	-0.133
	(0.143)	(0.143)	(0.145)	(0.146)	(0.104)	(0.104)	(0.105)	(0.105)
Larceny	-0.089	-0.086	-0.076	-0.091	-0.070	-0.070	-0.056	-0.057
	(0.107)	(0.107)	(0.107)	(0.106)	(0.077)	(0.077)	(0.077)	(0.077)
Motor vehicle theft	-0.492	-0.486	-0.486	-0.492	-0.319	-0.319	-0.317	-0.311
	(0.167)	(0.167)	(0.167)	(0.168)	(0.130)	(0.130)	(0.131)	(0.132)
budget cycles	no	yes	yes	yes	no	yes	yes	yes
demographics	no	no	yes	yes	no	no	yes	yes
polynomials and interactions	no	no	no	yes	no	no	no	yes

Note: Each column reports results of a 2SLS regression of the growth rate in each of nine crime rates on the first lag of the growth rate in the number of per capita sworn police officers. Columns (1)-(4) report results for the models in which the LEOKA measure is employed as the endogenous covariate and the ASG measure is employed as the instrumental variable while columns (5)-(8) report results for models in which the ASG measure is employed as the endogenous covariate and the LEOKA measure is employed as the instrumental variable. For each set of models, the first column reports regression results, conditional on the growth rate in the city's population and a set of unrestricted state-by-year effects. The second column adds a control variable for the city's per capita expenditures exclusive of police expenditures. In the third column we add demographic controls which capture the proportion of a city's population that is comprised of each of twelve age-gender-race groups. Finally, in the fourth column, we add polynomial terms and selected interactions of the demographic variables. All models are estimated using 2010 city population weights. Two sets of standard errors are reported below the coefficient estimates. The top row reports Huber-Eicker-White standard errors that are robust to heteroskedasticity . The standard errors reported in the second row are clustered at the city level.

Table 5. GMM Models of the Effect of Police on Crime
Pooled Estimates

	Violent Crime	Murder	Rape	Robbery	Assault	Property Crime	Burglary	Larceny	Motor Vehicle Theft
Pooled Estimate	-0.416 (0.087)	-0.583 (0.178)	n/a n/a	-0.690 (0.110)	-0.148 (0.112)	-0.174 (0.057)	-0.188 (0.083)	-0.076 (0.063)	-0.376 (0.099)
LR test statistic	2.45	1.10	0.003	3.57	1.50	1.10	2.81	0.54	1.94
N	5,997	6,021	5,997	6,023	6,023	6,012	6,023	6,012	6,023

Note: Each column reports results of a pooled IV regression of the growth rate in each of nine crime rates on the first lag of the growth rate in the number of sworn police officers as measured by the FBI's Uniform Crime Reporting Program. The growth rate in the number of sworn police officers as measured by the U.S. Census' Annual Survey of Government Employment is employed as an instrumental variable. Estimates are computed via GMM estimation. All models are estimated using 2010 city population weights. Huber-Eicker-White standard errors are reported in parentheses below the coefficient estimates. Below the standard errors, we report the value of the likelihood ratio test statistic, which is distributed χ_1 under the null hypothesis of classical measurement error. The criticial value of the test is 3.84.

Table 6. Further Tests of Classical Measurement Errors

	Measurement Error Type		
	LEOKA-LEMAS (1)	LEOKA-ASG (2)	LEMAS-ASG (3)
Panel A: Test of Assumption A1			
Violent crimes	0.006	-0.016	0.020
	(0.013)	(0.012)	(0.018)
Property crimes	0.015	-0.018	0.008
	(0.017)	(0.016)	(0.023)
Murder	-0.002	-0.009	0.006
	(0.005)	(0.005)	(0.008)
Rape	0.001	-0.005	0.017
	(0.009)	(0.006)	(0.012)
Robbery	0.001	-0.015	0.008
	(0.010)	(0.009)	(0.013)
Assault	0.001	-0.002	0.008
	(0.009)	(0.009)	(0.013)
Burglary	0.012	-0.025	0.026
	(0.012)	(0.013)	(0.018)
Larceny	0.011	-0.001	-0.004
	(0.014)	(0.013)	(0.020)
Motor vehicle theft	0.007	-0.014	0.001
	(0.009)	(0.010)	(0.014)
Panel B: Test of Assumptions A2 and A3			
ASG Measure	-0.015		
	(0.028)		
LEMAS Measure		-0.039	
		(0.035)	
LEOKA Measure			0.021
			(0.036)

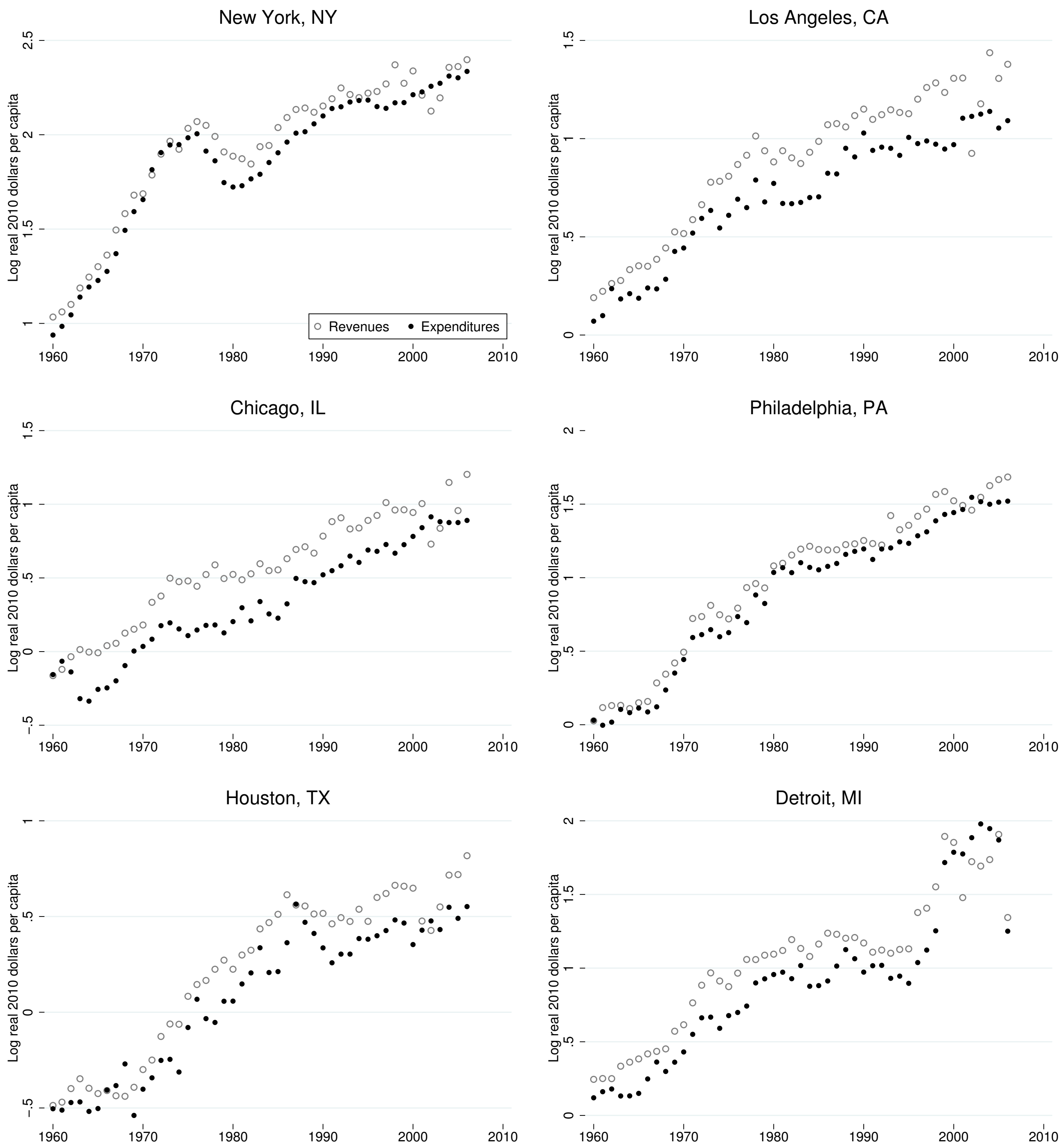
Note: Each column corresponds to a particular incarnation of measurement error. In column (1), the measurement error is calculated as the difference between the LEOKA series and the LEMAS series. In column (2) the measurement error is calculated as the difference between the LEOKA series and the ASG series. Finally, in column (3), the measurement error is calculated as the difference between the LEMAS series and the ASG series. Due to the limitied availability of LEMAS data, estimates in columns (1) and (3) are calculated using the following years of data: 1987, 1990, 1992, 1993, 1996, 1997, 1999, 2000, 2003, 2004, 2007 and 2008. Estimates in column (2) use the full 1960-2008 sample period. Panel A of the table reports the results of a series of regressions of growth rate in the number of crimes on the measurement error, conditional on the growth rate in population. Panel B reports the results of a series of regressions of a given proxy for the number of police on the measurement error, calculated as the difference between the two remaining measures. Each of the models contains a full set of state by year fixed effects. All models are estimated using 2010 city population weights. Huber-Eicker-White standard errors that are robust to heteroskedasticity are reported in parentheses below the coefficient estimates.

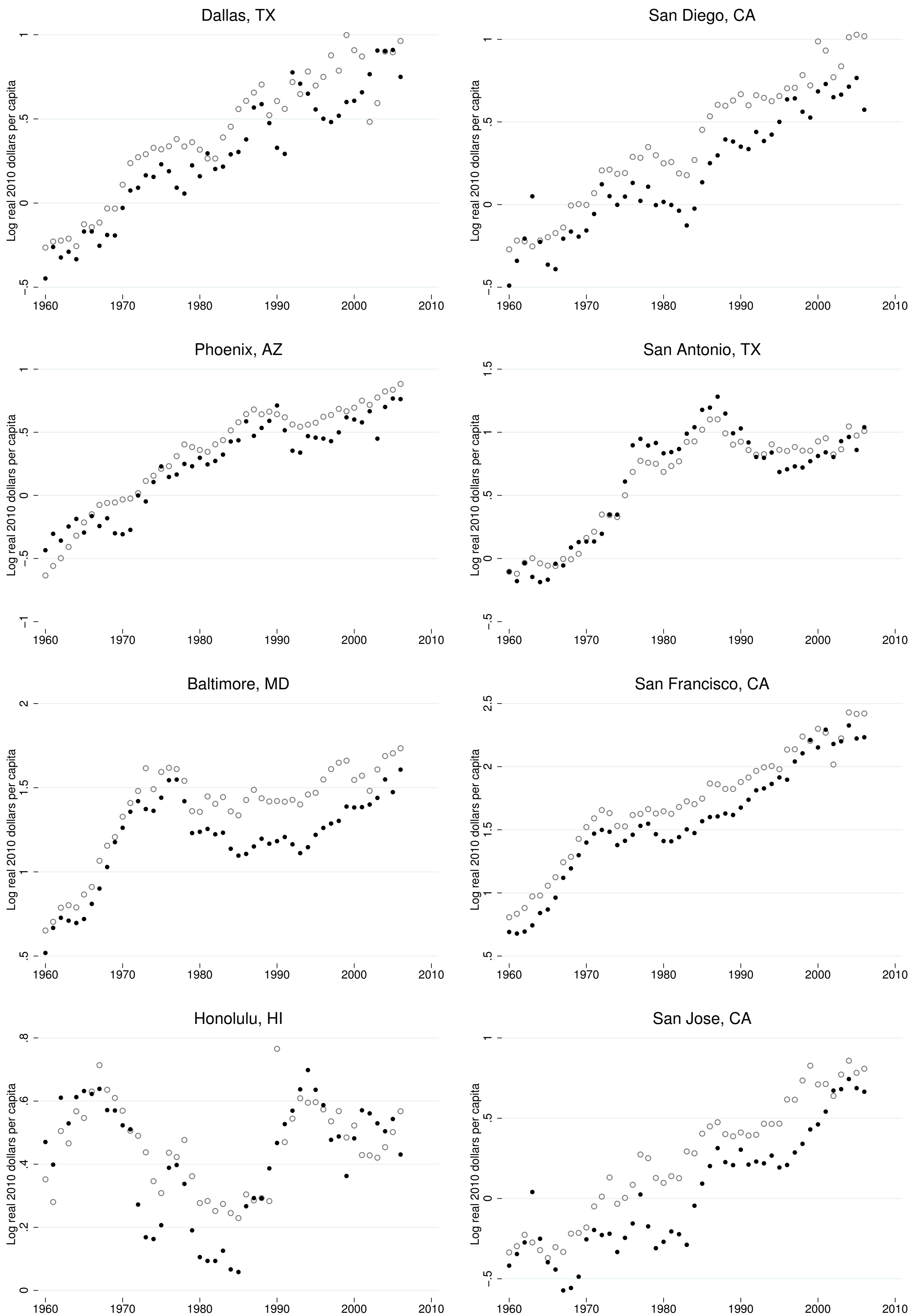
Table 7. Extant Estimates of the Effect of Police on Crime
Implied Elasticities

Article	Country	Years	Cross-Sectional Units	Research Design	Violent Crime	Property Crime
Marvell and Moody (1996)	USA	1973-1992	56 cities	lags as control variables	-0.13* (murder) -0.22* (robbery)	-0.15* (burglary) -0.30* (auto theft)
Levitt (1997)	USA	1970-1992	59 cities	mayoral elections	-0.79 -3.03 (murder) -1.29 (robbery)	0.00 -0.55 (burglary) -0.44 (auto theft)
McCrary (2002)	USA	1970-1992	59 cities	mayoral elections	-0.66 -2.69 (murder) -0.98 (robbery)	0.11 -0.47 (burglary) -0.77 (auto theft)
Levitt (2002)	USA	1975-1995	122 cities	number of firefighters	-0.44* -0.91* (murder) -0.45 (robbery)	-0.50* -0.20 (burglary) -1.70* (auto theft)
DiTella and Schargrodsky (2004)	Argentina	4/1994 -12/1994	876 city blocks	redeployment of police following a terrorist attack	n/a	-0.33* (auto theft)
Klick and Tabarrok (2005)	USA	3/12/2002 - 7/30/2003	7 districts	high terrorism alert days	0.0	-0.30* (burglary) -0.84* (auto theft)
Evans and Owens (2007)	USA	1990-2001	2,074 cities	COPS grants	-0.99* -0.84* (murder) -1.34* (robbery)	-0.26 -0.59* (burglary) -0.85* (auto theft)
Our preferred estimates	USA	1960-2008	135 cities	measurement errors	-0.42* -0.58* (murder) -0.69* (robbery)	-0.17* -0.19* (burglary) -0.38* (auto theft)

Note: This table reports implied elasticities that arise from six recent articles each of which employs a novel identification strategy to estimate a *causal* effect of police on crime. Elasticities are reported for the violent and property crime aggregates as well as for murder, robbery, burglary and auto theft. In place of the original elasticities reported in Levitt (1997), we have included elasticity estimates from McCrary (2002) which correct for a coding error in the original paper. Our preferred estimates which account for the presence of measurement error in the Uniform Crime Reports police series are shown below. Asterisks denote results that are significant, at a minimum, at the 10% level.

Appendix Figure 1. Log Real Per Capita City Revenues and Expenditures Exclusive of Police Expenditures

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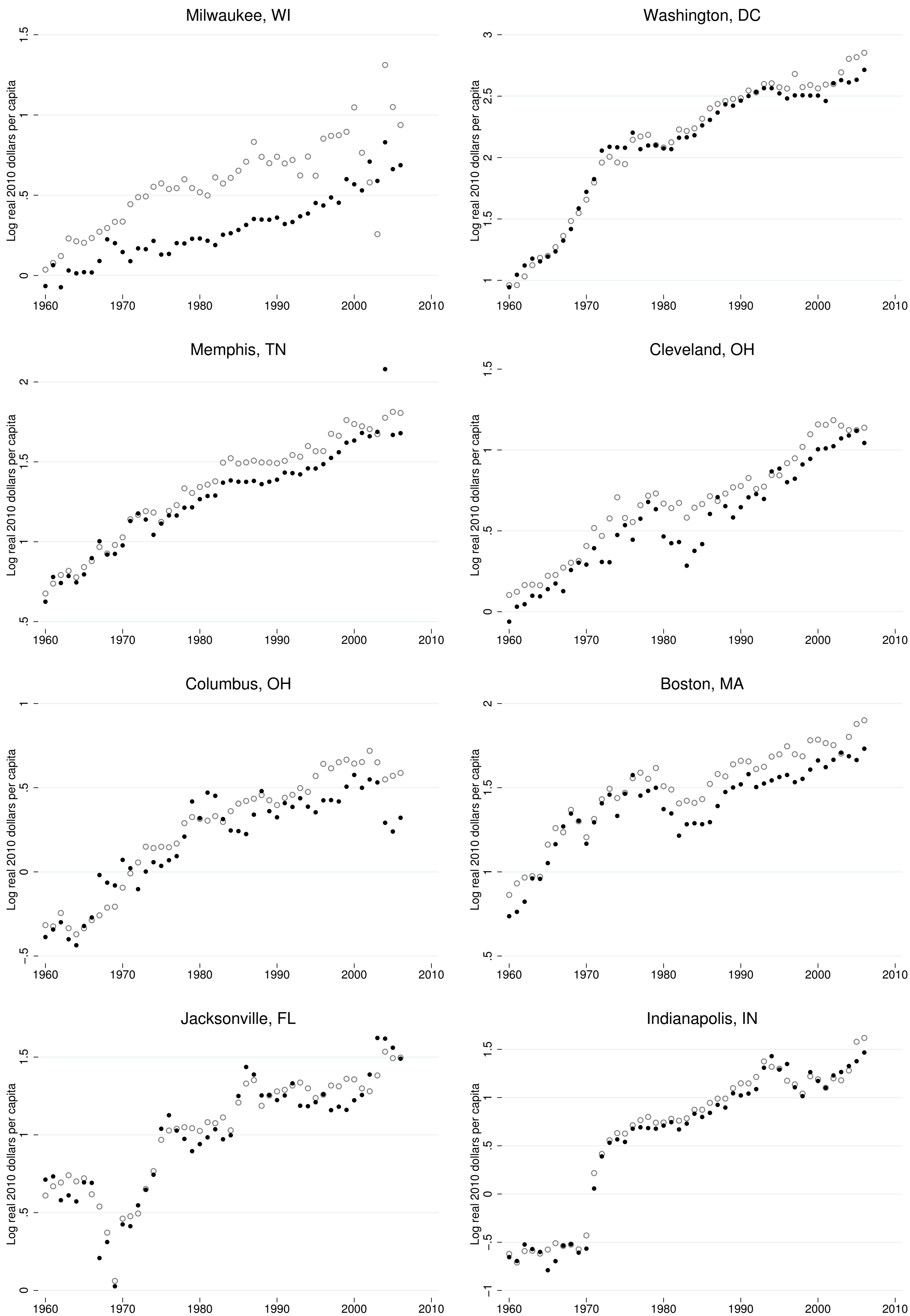
San Diego, CA
Phoenix, AZ
San Antonio, TX
Baltimore, MD
San Francisco, CA
Honolulu, HI
San Jose, CA
Log real 2010 dollars per capita
1960
1970
1980
1990
2000
2010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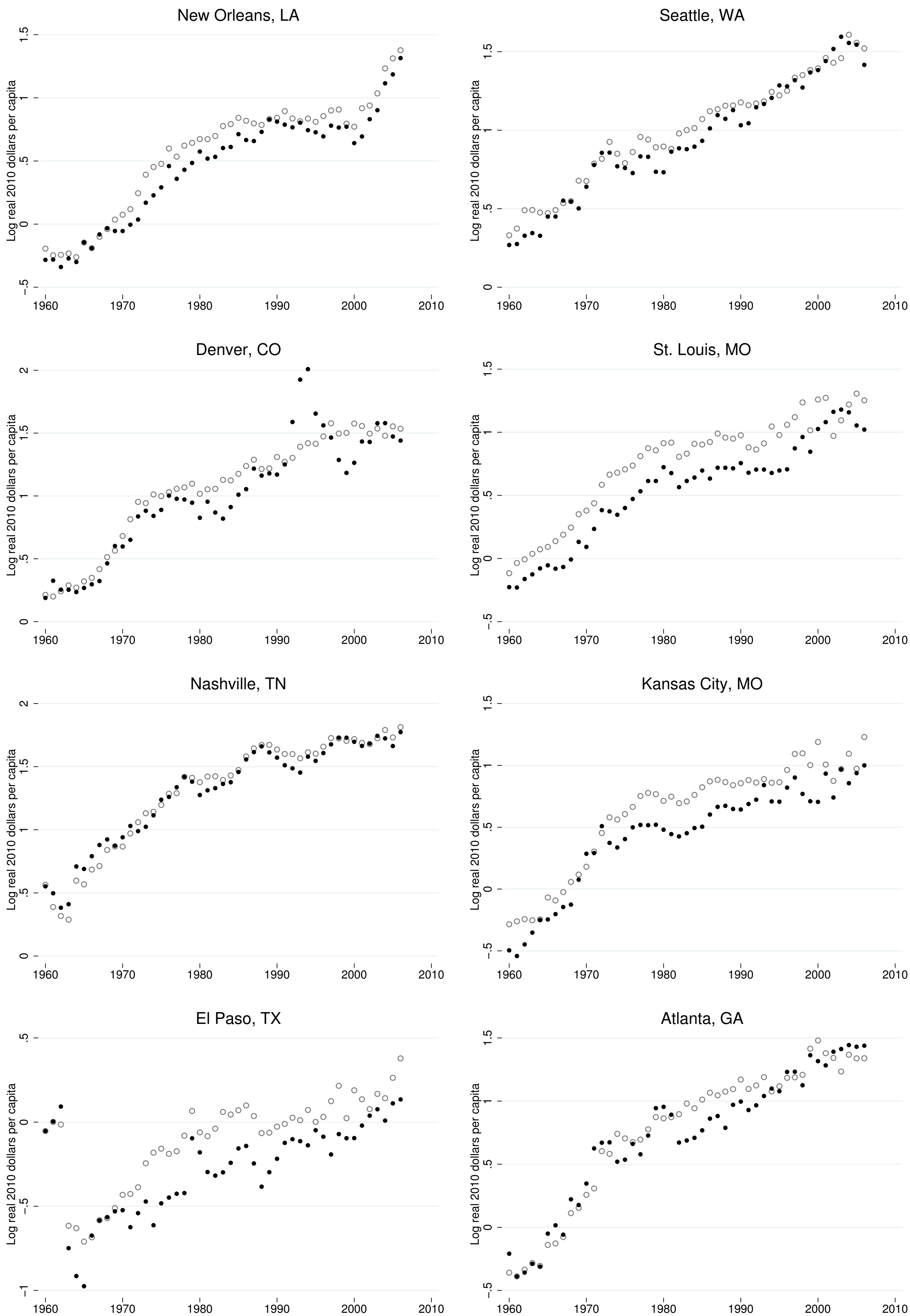
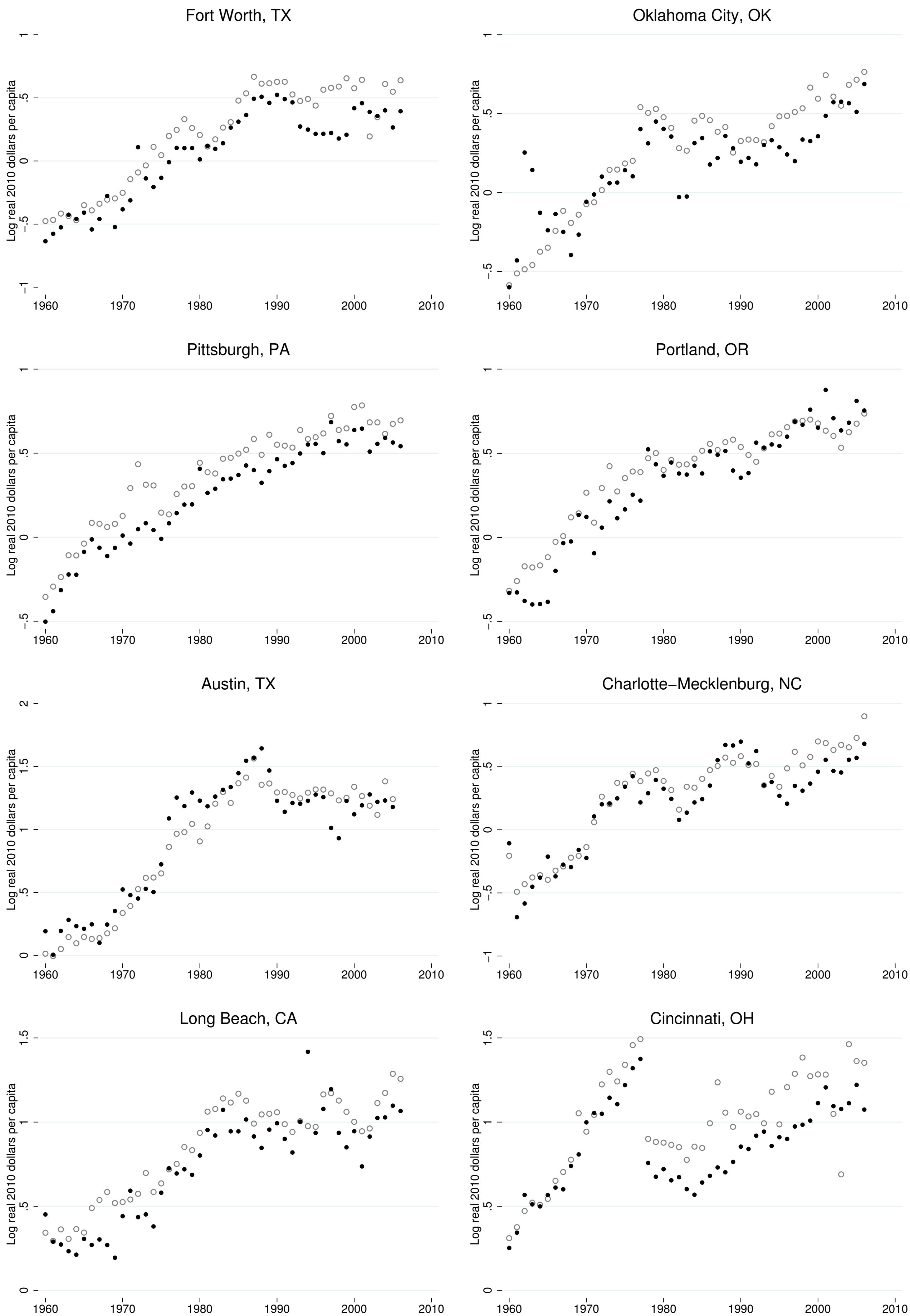
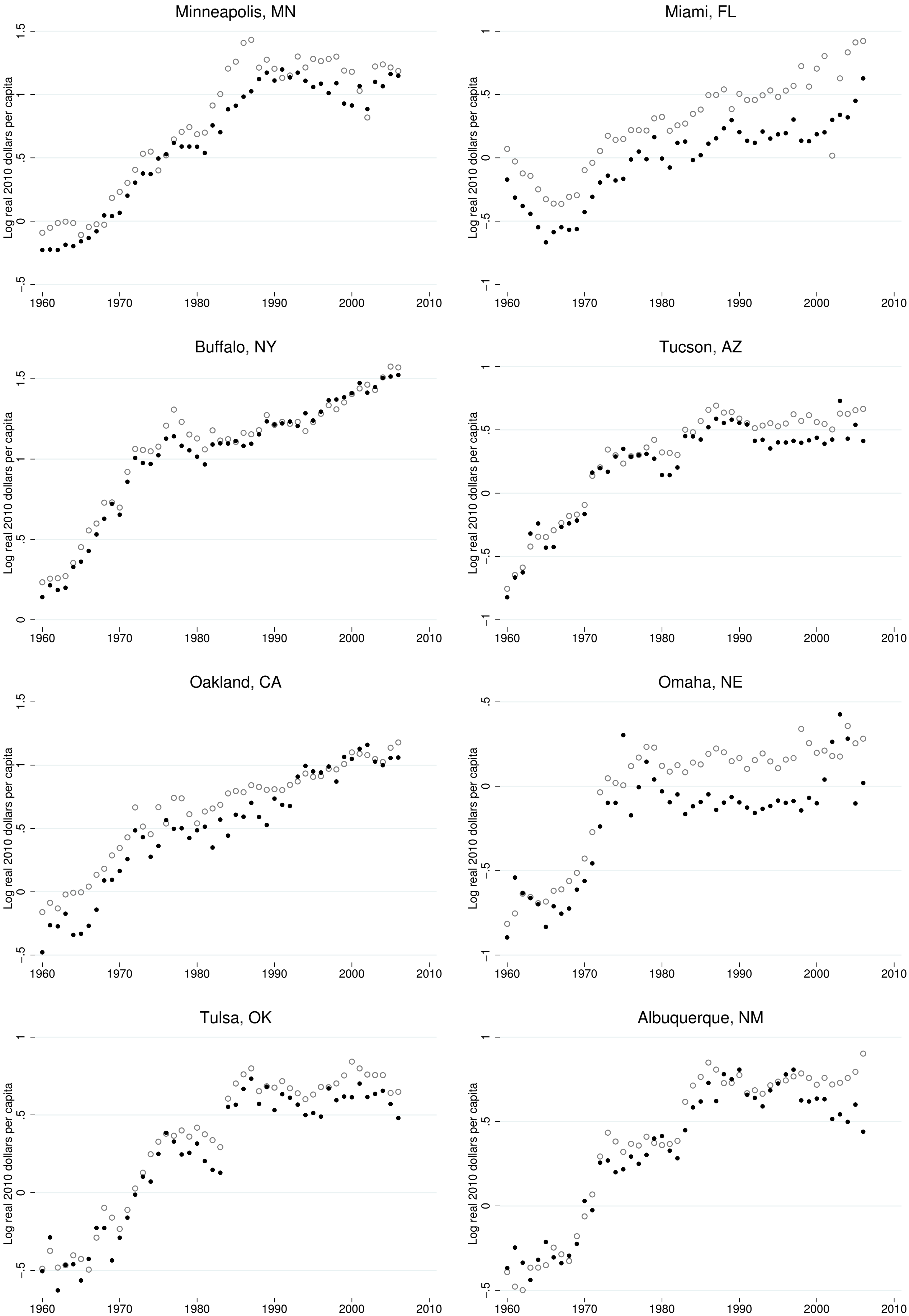
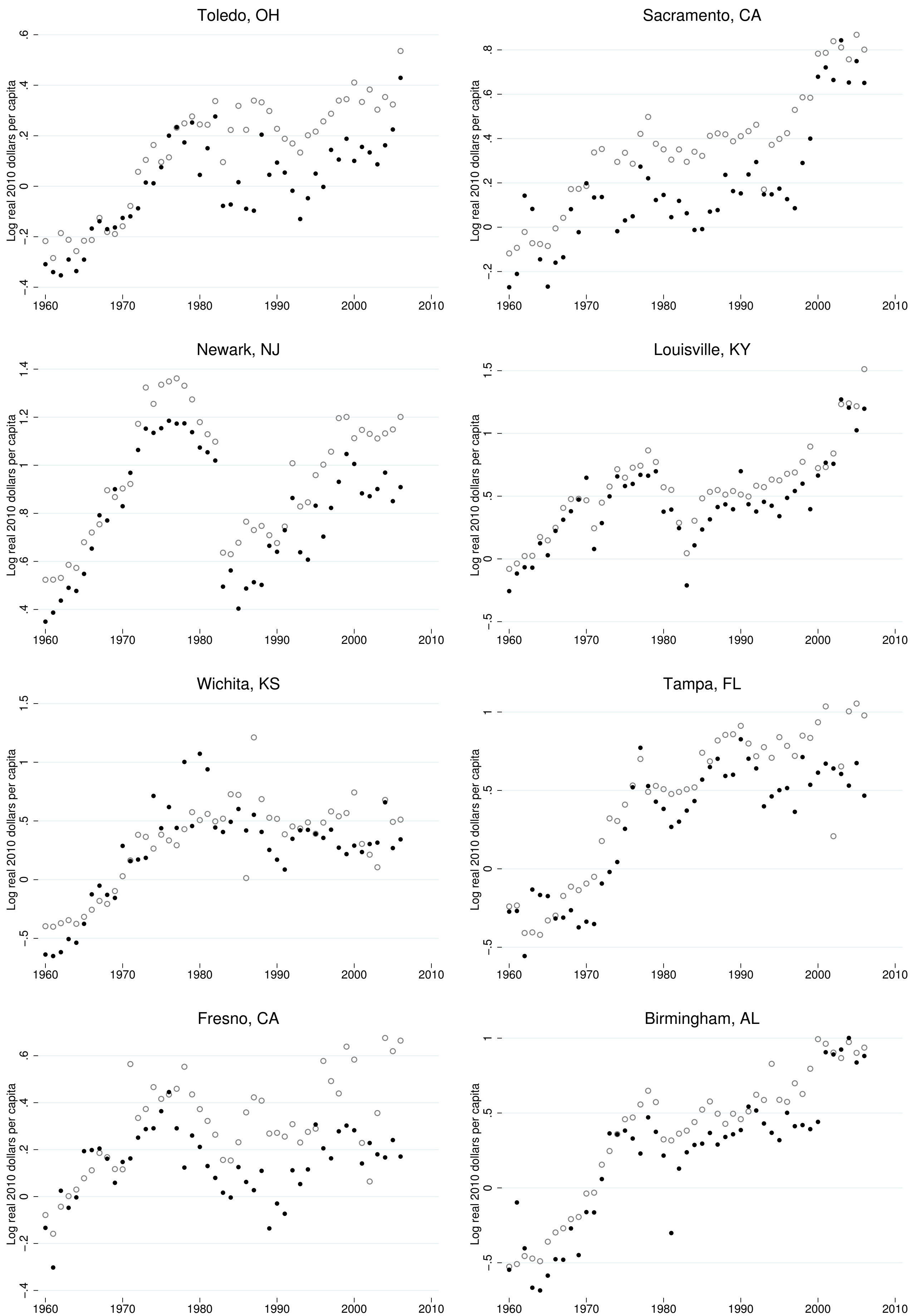
Log real 2010 dollars per capita
0
.5
1
1.5
1960
1970
1980
1990
2000
2010
Washington, DC
Log real 2010 dollars per capita
1
1.5
2
2.5
3
1960
1970
1980
1990
2000
2010
Memphis, TN
Log real 2010 dollars per capita
.5
1
1.5
2
1960
1970
1980
1990
2000
2010
Cleveland, OH
Log real 2010 dollars per capita
0
.5
1
1.5
1960
1970
1980
1990
2000
2010
Columbus, OH
Log real 2010 dollars per capita
-.5
0
.5
1
1960
1970
1980
1990
2000
2010
Boston, MA
Log real 2010 dollars per capita
.5
1
1.5
2
1960
1970
1980
1990
2000
2010
Jacksonville, FL
Log real 2010 dollars per capita
0
.5
1
1.5
1960
1970
1980
1990
2000
2010
Indianapolis, IN
Log real 2010 dollars per capita
-1
-.5
0
.5
1
1.5
1960
1970
1980
1990
2000
2010

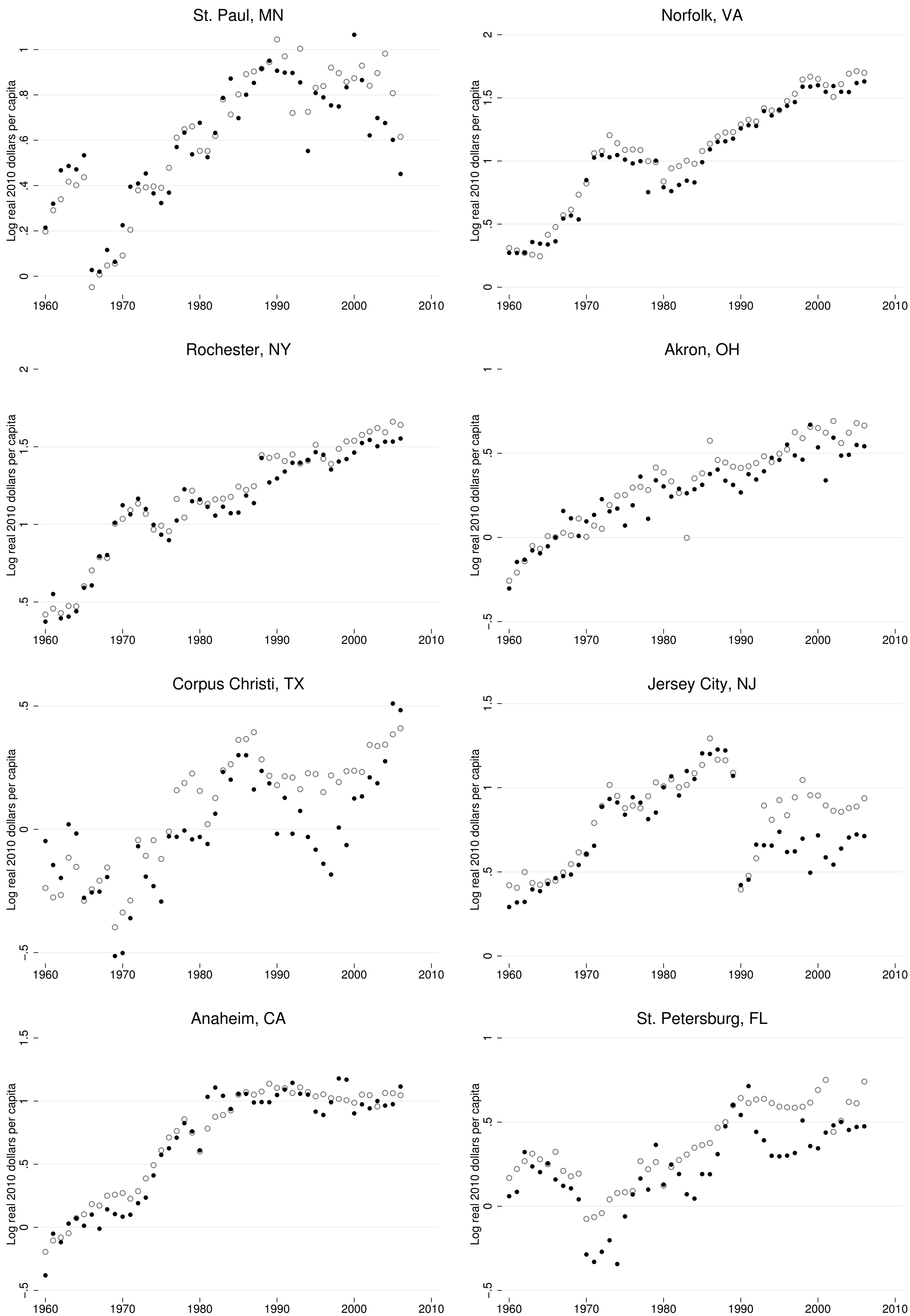

New Orleans, LA
Seattle, WA
Denver, CO
St. Louis, MO
Nashville, TN
Kansas City, MO
El Paso, TX
Atlanta, GA
Log real 2010 dollars per capita
1960
1970
1980
1990
2000
2010


Fort Worth, TX
Oklahoma City, OK
Pittsburgh, PA
Portland, OR
Austin, TX
Charlotte−Mecklenburg, NC
Long Beach, CA
Cincinnati, OH
Log real 2010 dollars per capita
1960
1970
1980
1990
2000
2010


Minneapolis, MN
Miami, FL
Buffalo, NY
Tucson, AZ
Oakland, CA
Omaha, NE
Tulsa, OK
Albuquerque, NM
Log real 2010 dollars per capita
1960
1970
1980
1990
2000
2010



Toledo, OH
Sacramento, CA
Newark, NJ
Louisville, KY
Wichita, KS
Tampa, FL
Fresno, CA
Birmingham, AL
Log real 2010 dollars per capita
1960
1970
1980
1990
2000
2010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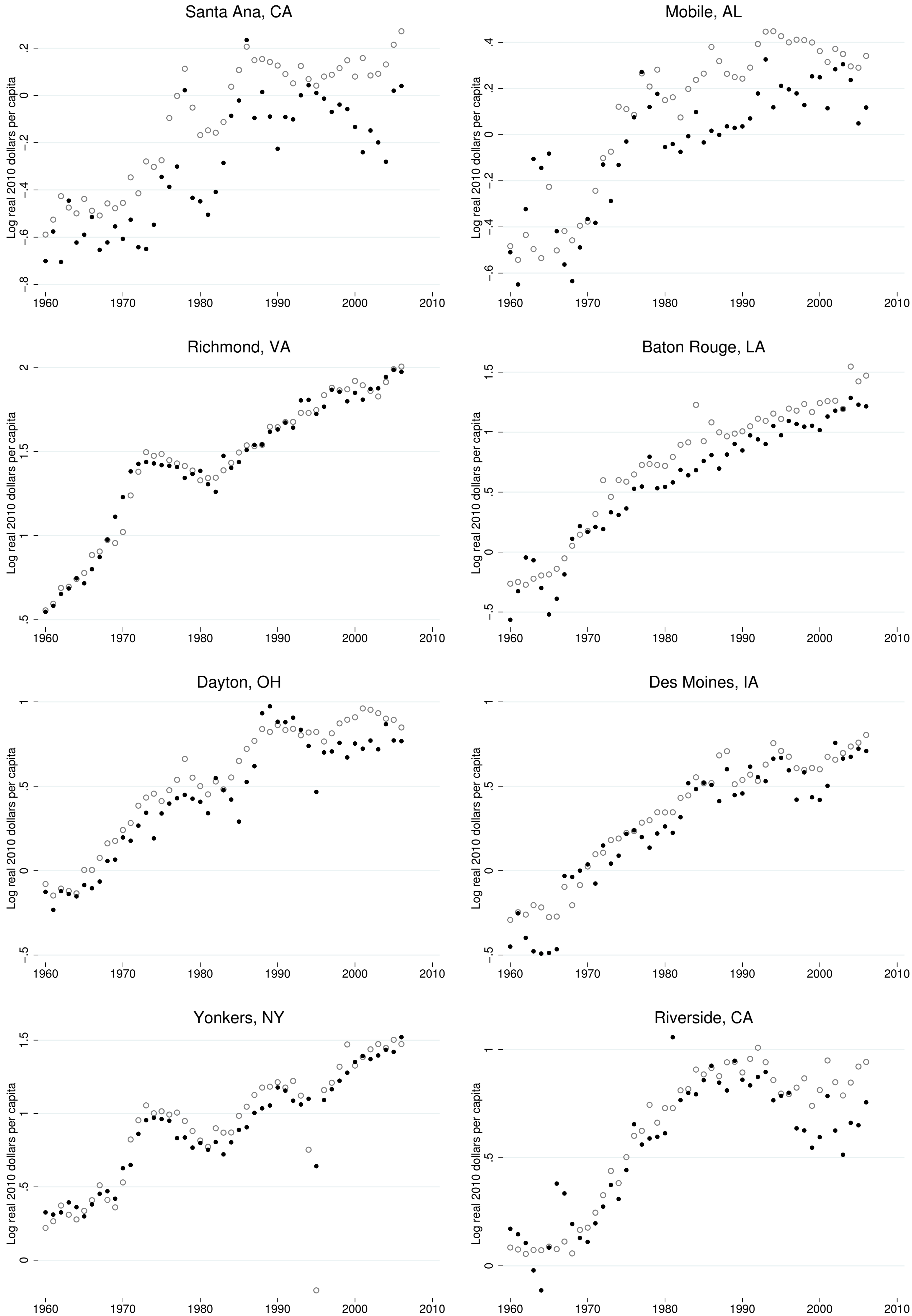
Norfolk, VA
Rochester, NY
Akron, OH
Corpus Christi, TX
Jersey City, NJ
Anaheim, CA
St. Petersburg, FL
Log real 2010 dollars per capita
1960
1970
1980
1990
2000
2010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Log real 2010 dollars per capita
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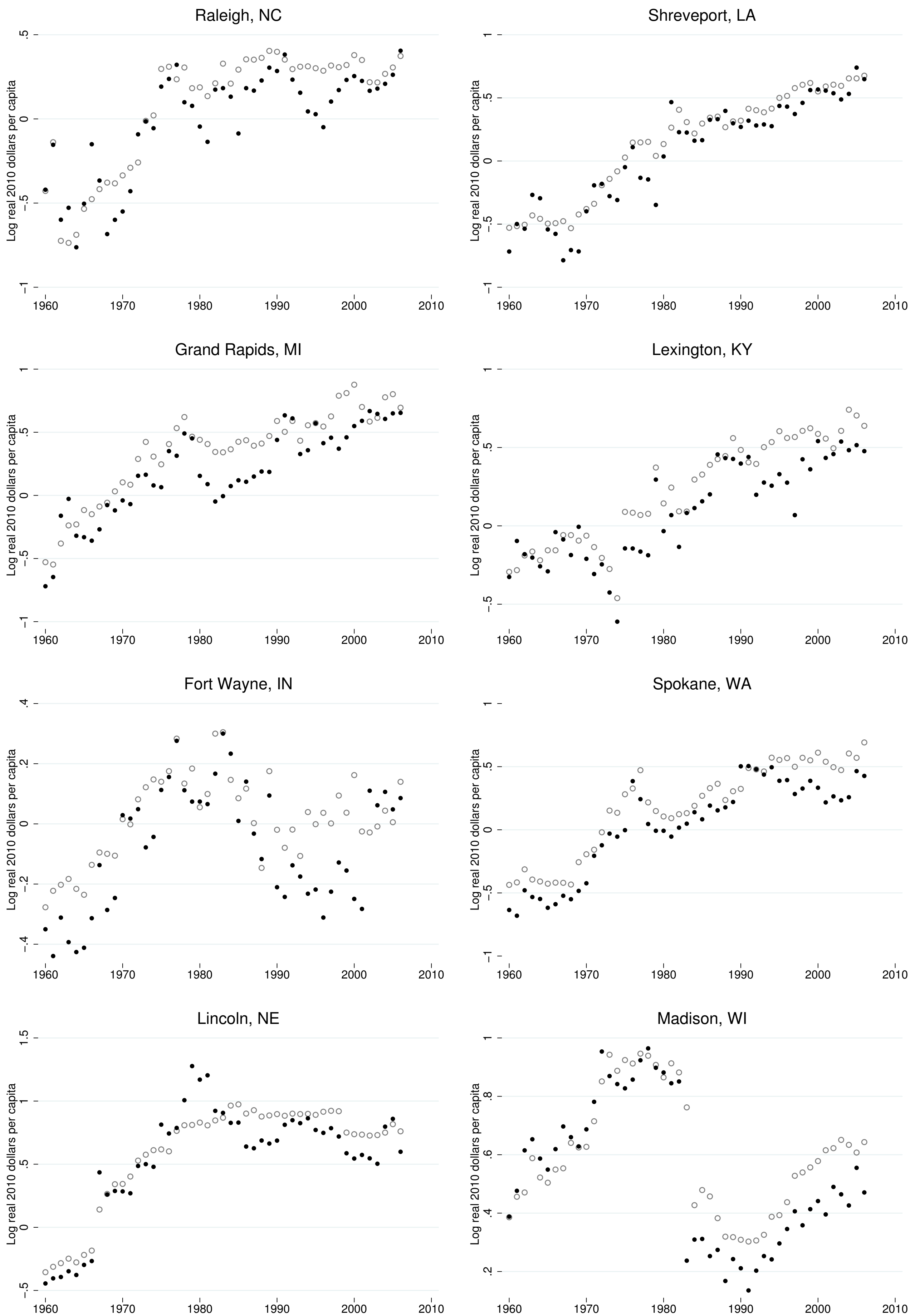
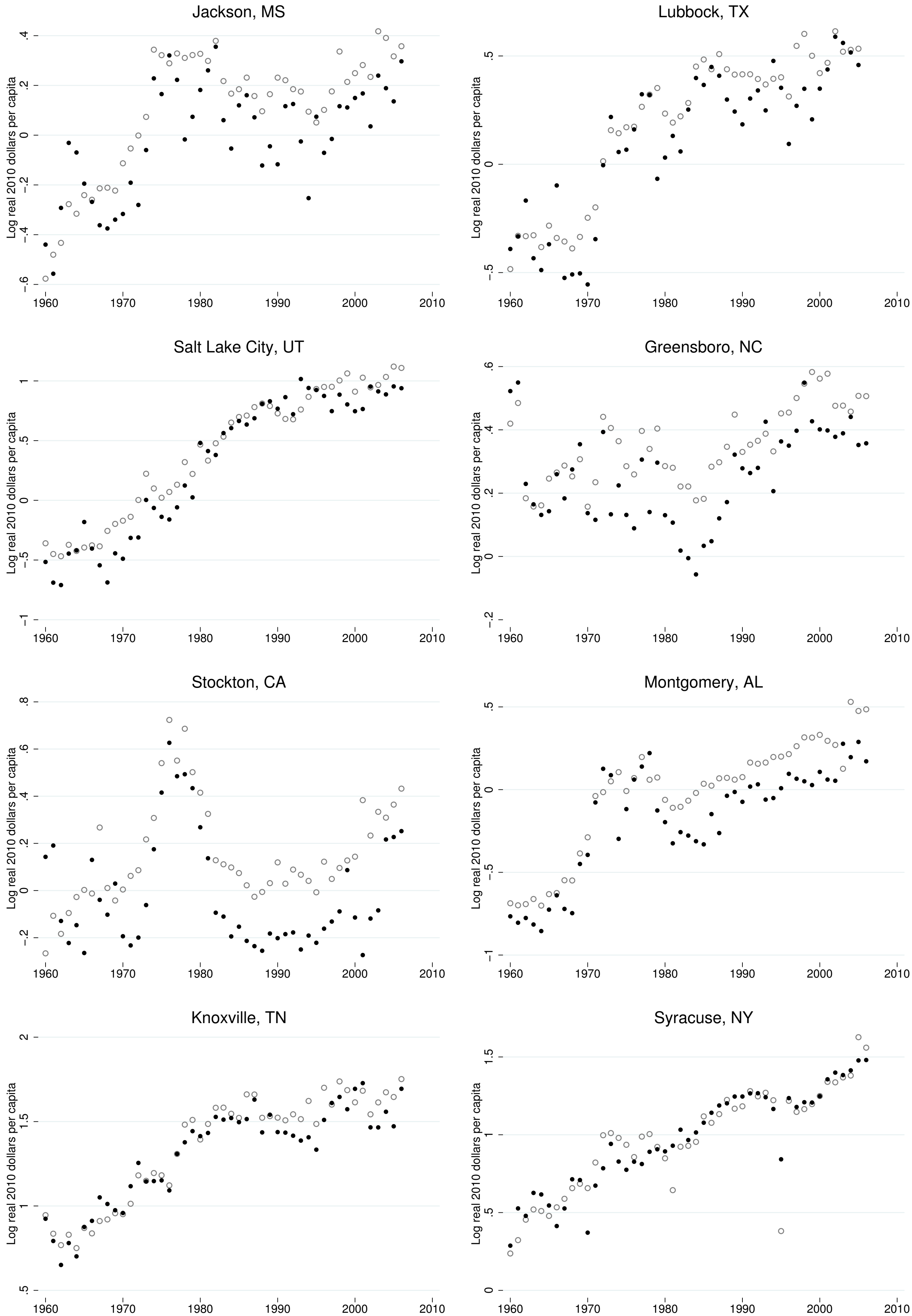
-.6
-.4
-.2
0
.2
1960
1970
1980
1990
2000
2010
Mobile, AL
Log real 2010 dollars per capita
-.6
-.4
-.2
0
.2
.4
1960
1970
1980
1990
2000
2010
Richmond, VA
Log real 2010 dollars per capita
.5
1
1.5
2
1960
1970
1980
1990
2000
2010
Baton Rouge, LA
Log real 2010 dollars per capita
-.5
0
.5
1
1.5
1960
1970
1980
1990
2000
2010
Dayton, OH
Log real 2010 dollars per capita
-.5
0
.5
1
1960
1970
1980
1990
2000
2010
Des Moines, IA
Log real 2010 dollars per capita
-.5
0
.5
1
1960
1970
1980
1990
2000
2010
Yonkers, NY
Log real 2010 dollars per capita
0
.5
1
1.5
1960
1970
1980
1990
2000
2010
Riverside, CA
Log real 2010 dollars per capita
0
.5
1
1960
1970
1980
1990
2000
2010



Raleigh, NC
Shreveport, LA
Grand Rapids, MI
Lexington, KY
Fort Wayne, IN
Spokane, WA
Lincoln, NE
Madison, WI
Log real 2010 dollars per capita
1960
1970
1980
1990
2000
2010


Jackson, MS
Lubbock, TX
Salt Lake City, UT
Greensboro, NC
Stockton, CA
Montgomery, AL
Knoxville, TN
Syracuse, NY
Log real 2010 dollars per capita
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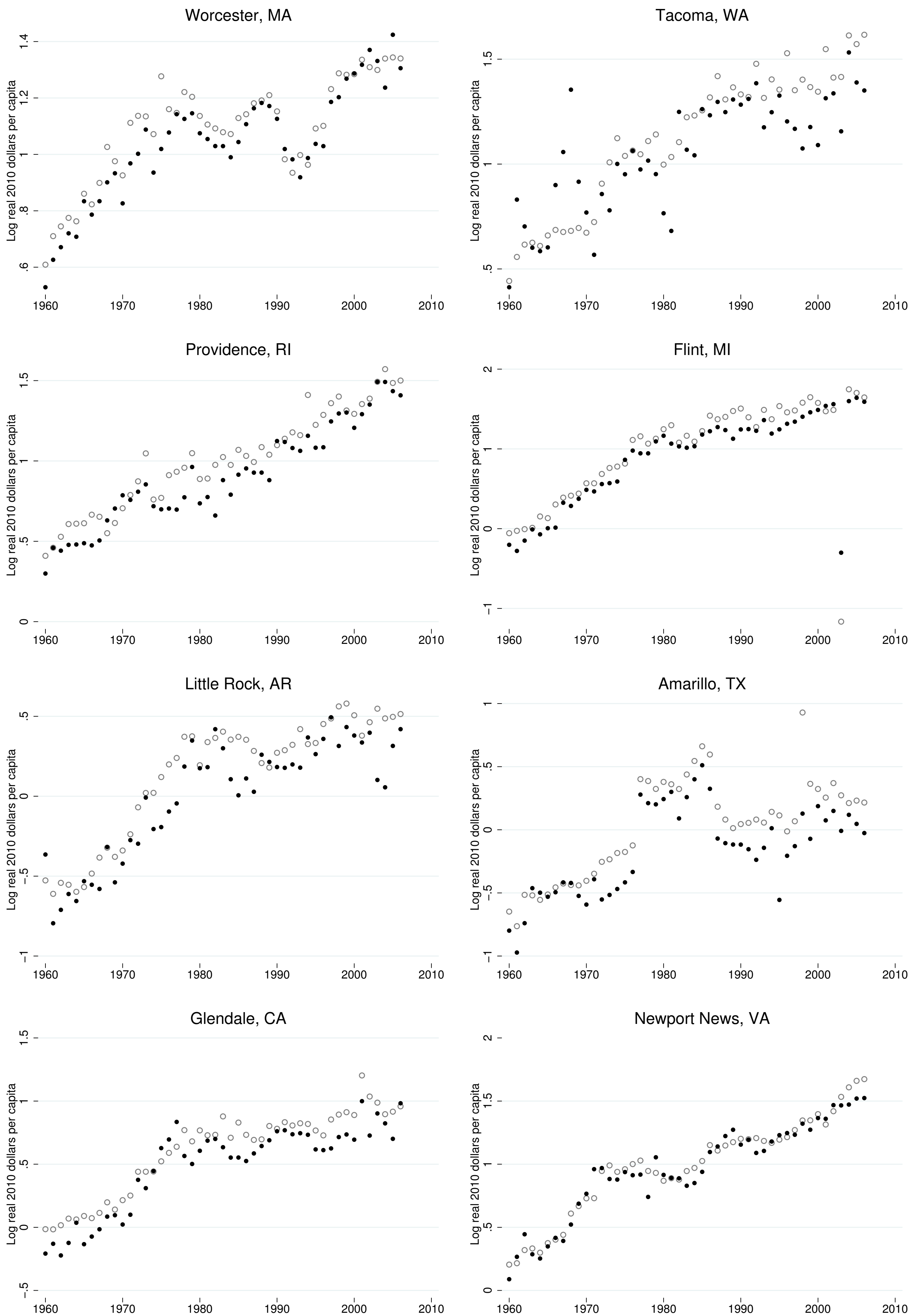
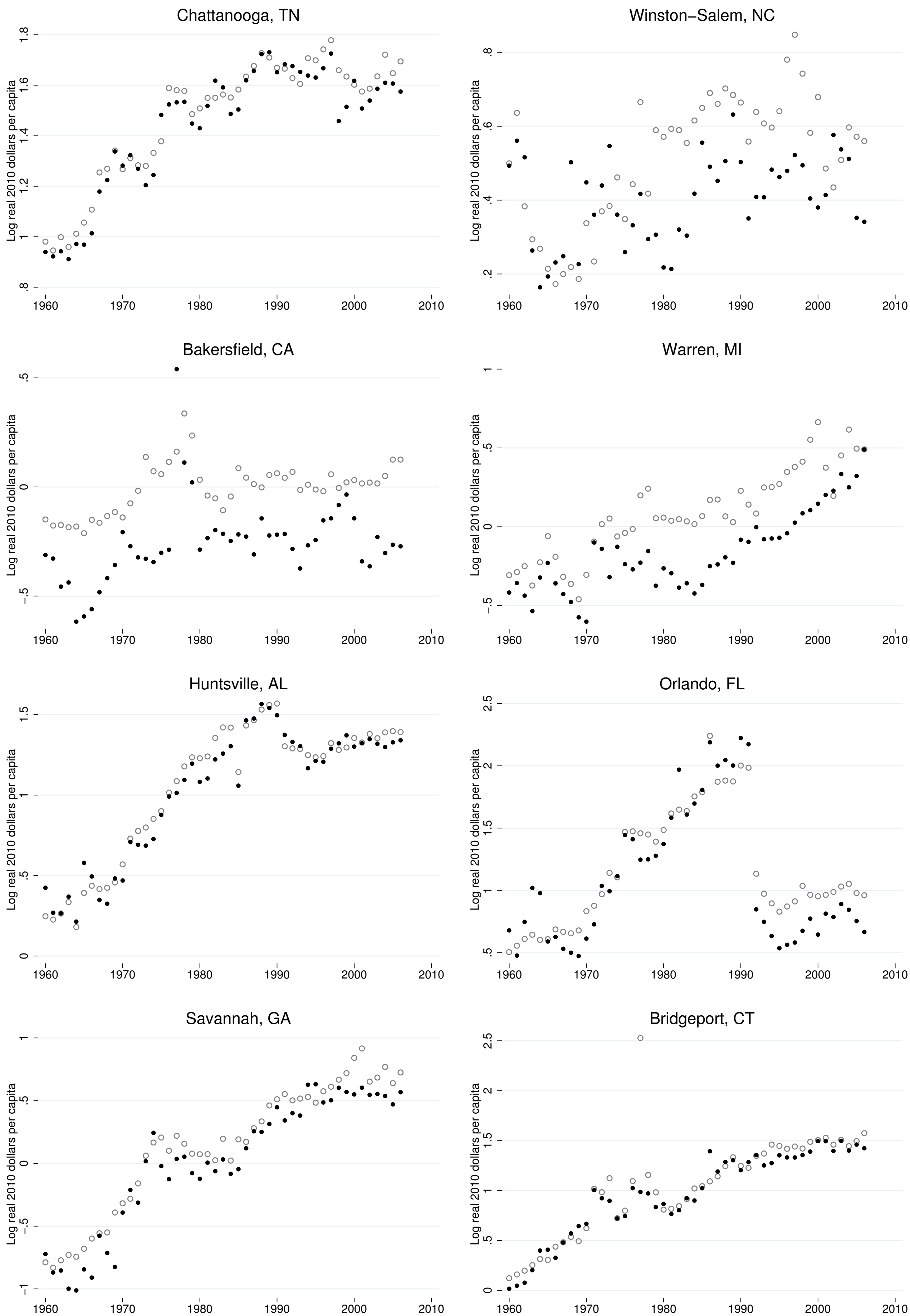
1970
1980
1990
2000
2010


Worcester, MA
Tacoma, WA
Providence, RI
Flint, MI
Little Rock, AR
Amarillo, TX
Glendale, CA
Newport News, VA
Log real 2010 dollars per capita
1960
1970
1980
1990
2000
2010


Chattanooga, TN
Log real 2010 dollars per capita
.8
1
1.2
1.4
1.6
1.8
1960
1970
1980
1990
2000
2010
Winston−Salem, NC
Log real 2010 dollars per capita
.2
.4
.6
.8
1960
1970
1980
1990
2000
2010
Bakersfield, CA
Log real 2010 dollars per capita
−.5
0
.5
1960
1970
1980
1990
2000
2010
Warren, MI
Log real 2010 dollars per capita
−.5
0
.5
1
1960
1970
1980
1990
2000
2010
Huntsville, AL
Log real 2010 dollars per capita
0
.5
1
1.5
1960
1970
1980
1990
2000
2010
Orlando, FL
Log real 2010 dollars per capita
.5
1
1.5
2
2.5
1960
1970
1980
1990
2000
2010
Savannah, GA
Log real 2010 dollars per capita
−1
−.5
0
.5
1
1960
1970
1980
1990
2000
2010
Bridgeport, CT
Log real 2010 dollars per capita
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.5
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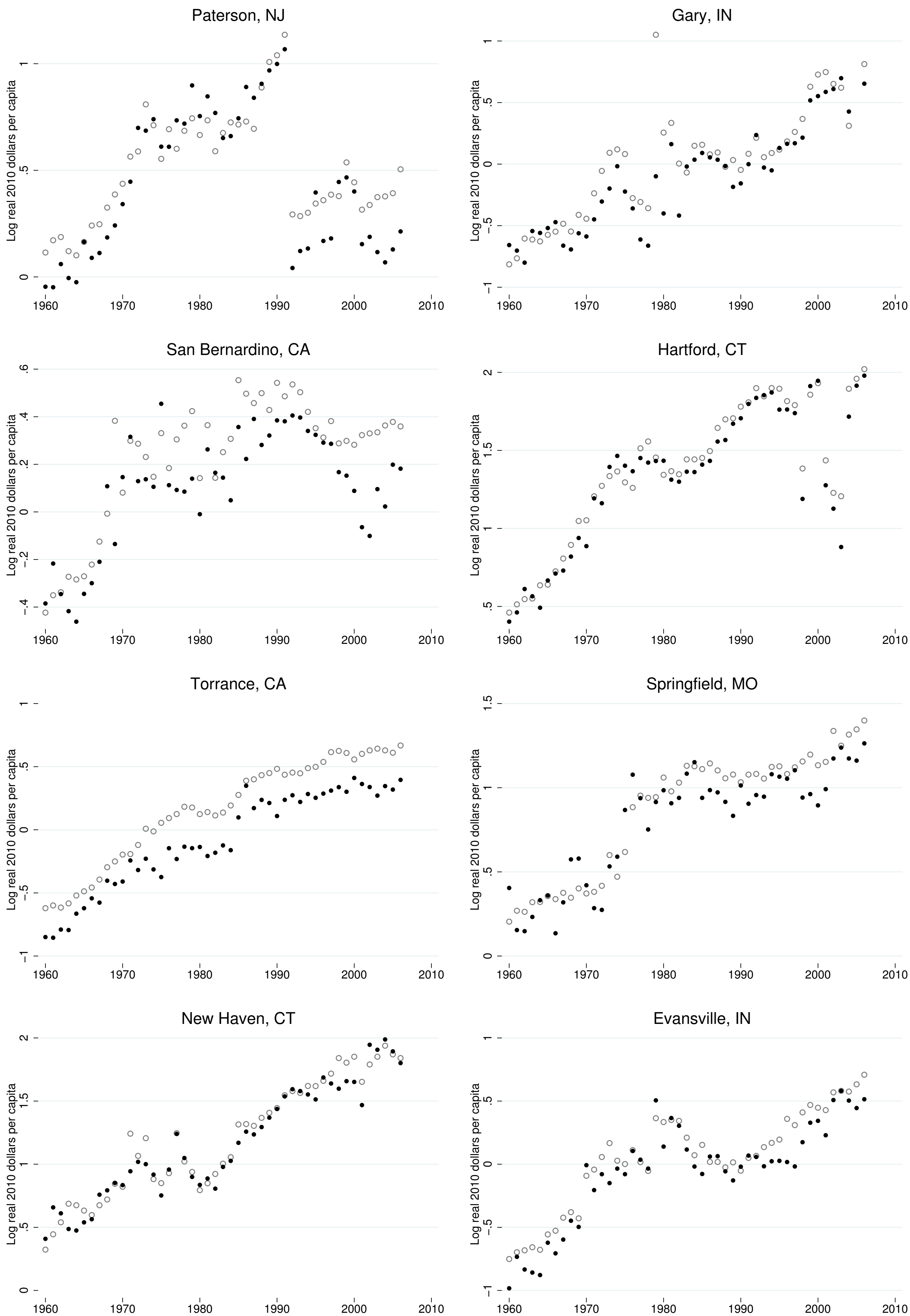
1.5
2
2.5
1960
1970
1980
1990
2000
2010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San Bernardino, CA
Hartford, CT
Torrance, CA
Springfield, MO
New Haven, CT
Evansville, IN
Log real 2010 dollars per capita
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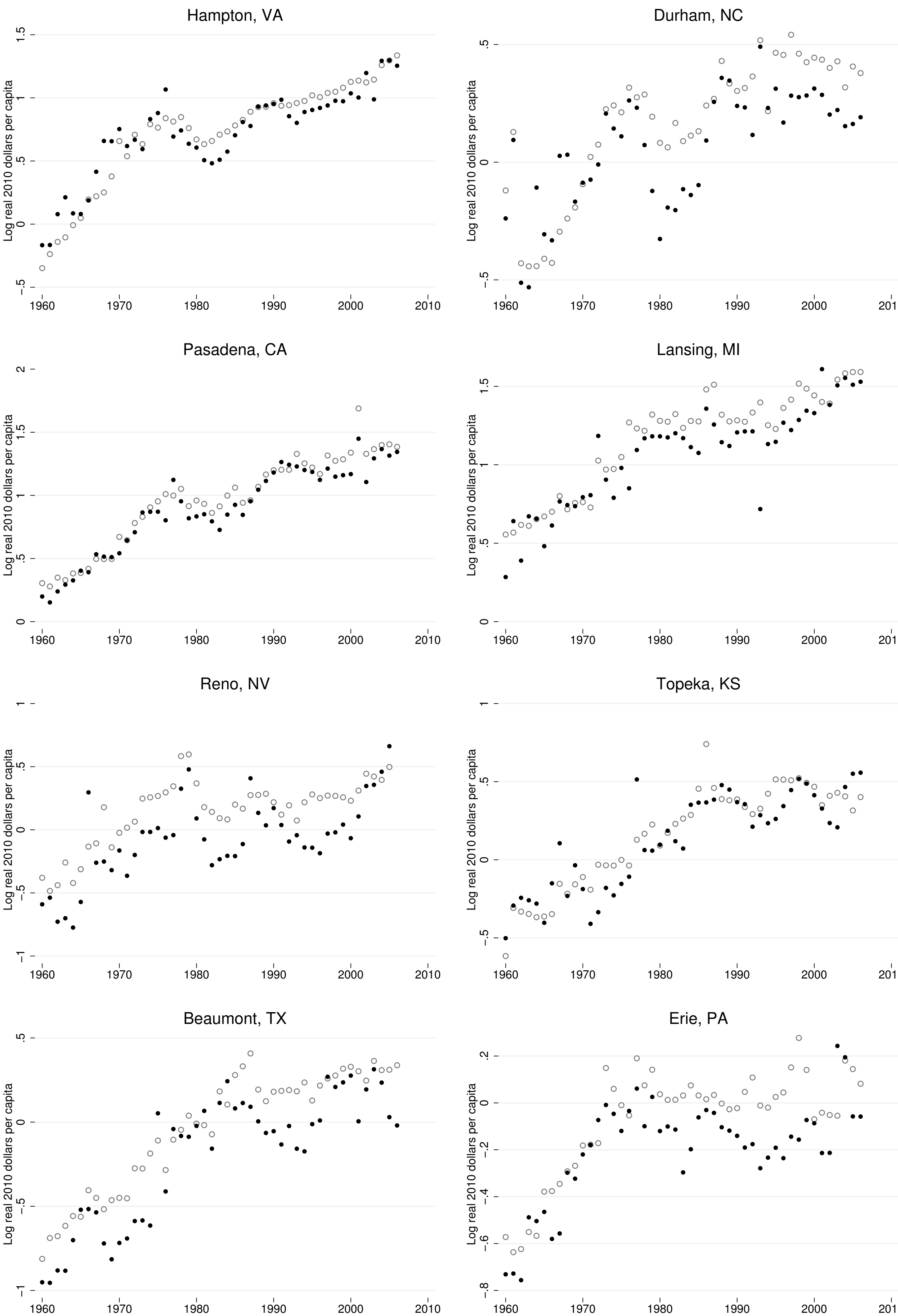
1970
1980
1990
2000
2010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Pasadena, CA
Lansing, MI
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Topeka, KS
Beaumont, TX
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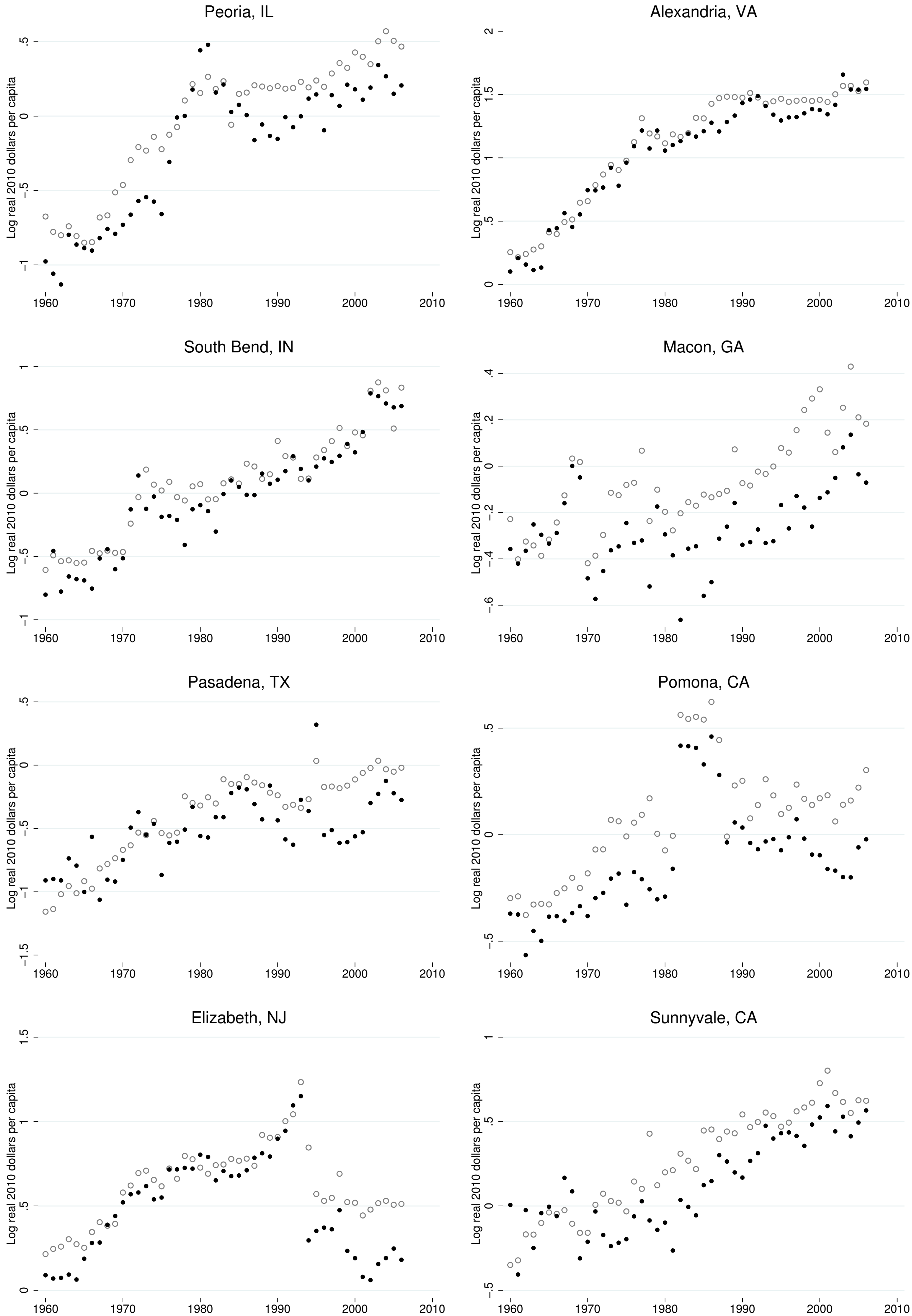
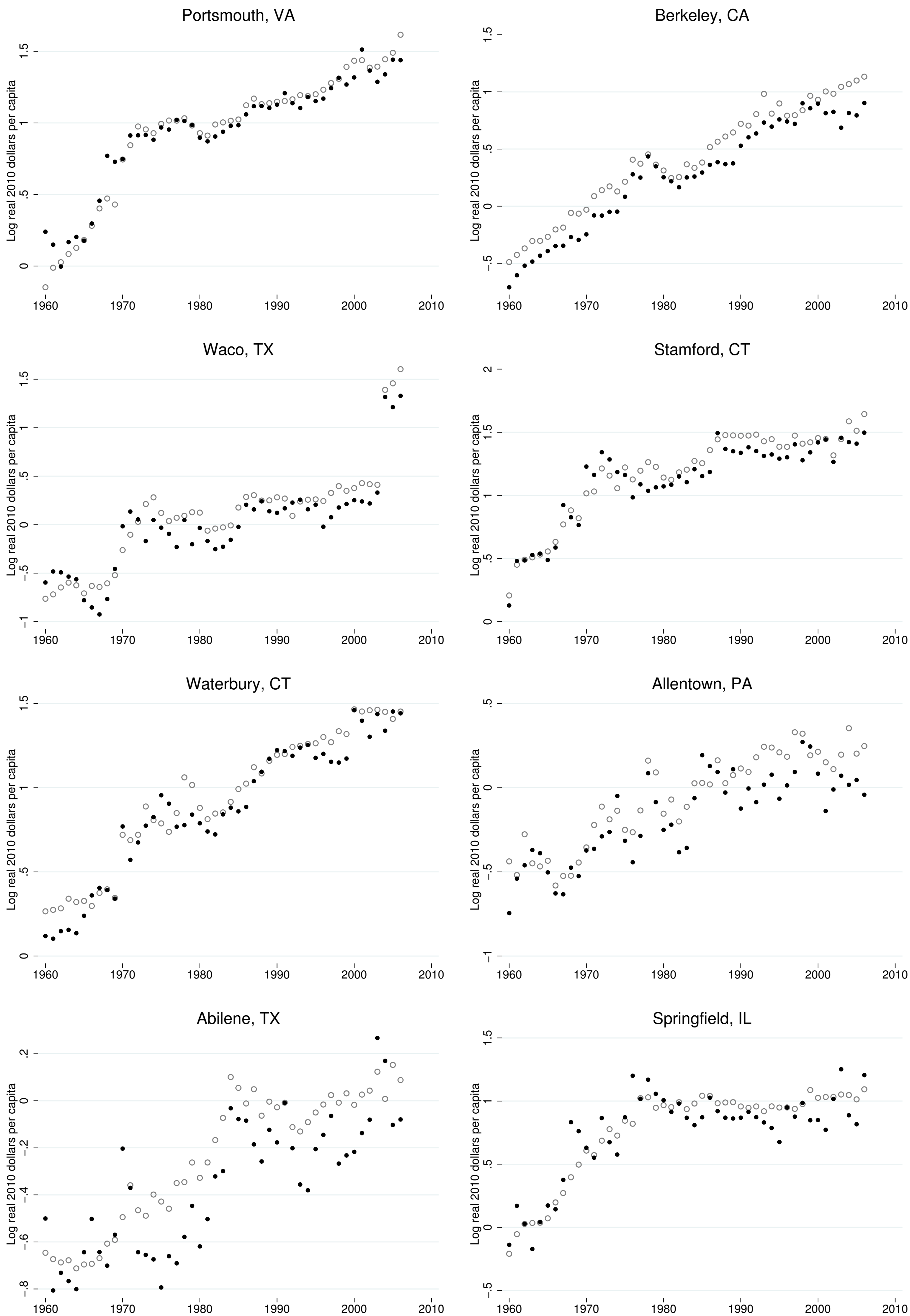
Log real 2010 dollars per capita
1960
1970
1980
1990
2000
2010


Peoria, IL
Alexandria, VA
South Bend, IN
Macon, GA
Pasadena, TX
Pomona, CA
Elizabeth, NJ
Sunnyvale, CA
Log real 2010 dollars per capita
1960
1970
1980
1990
2000
2010


Portsmouth, VA
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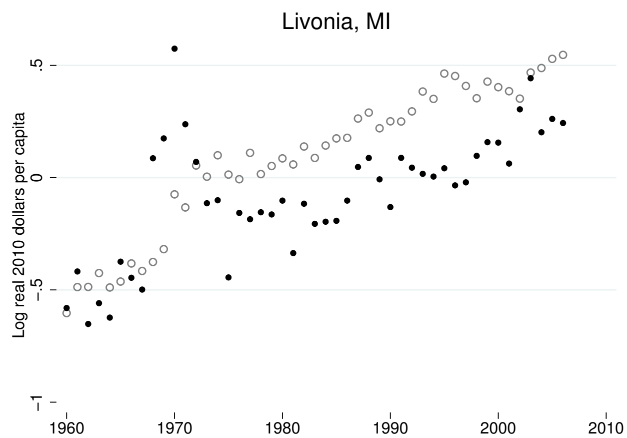
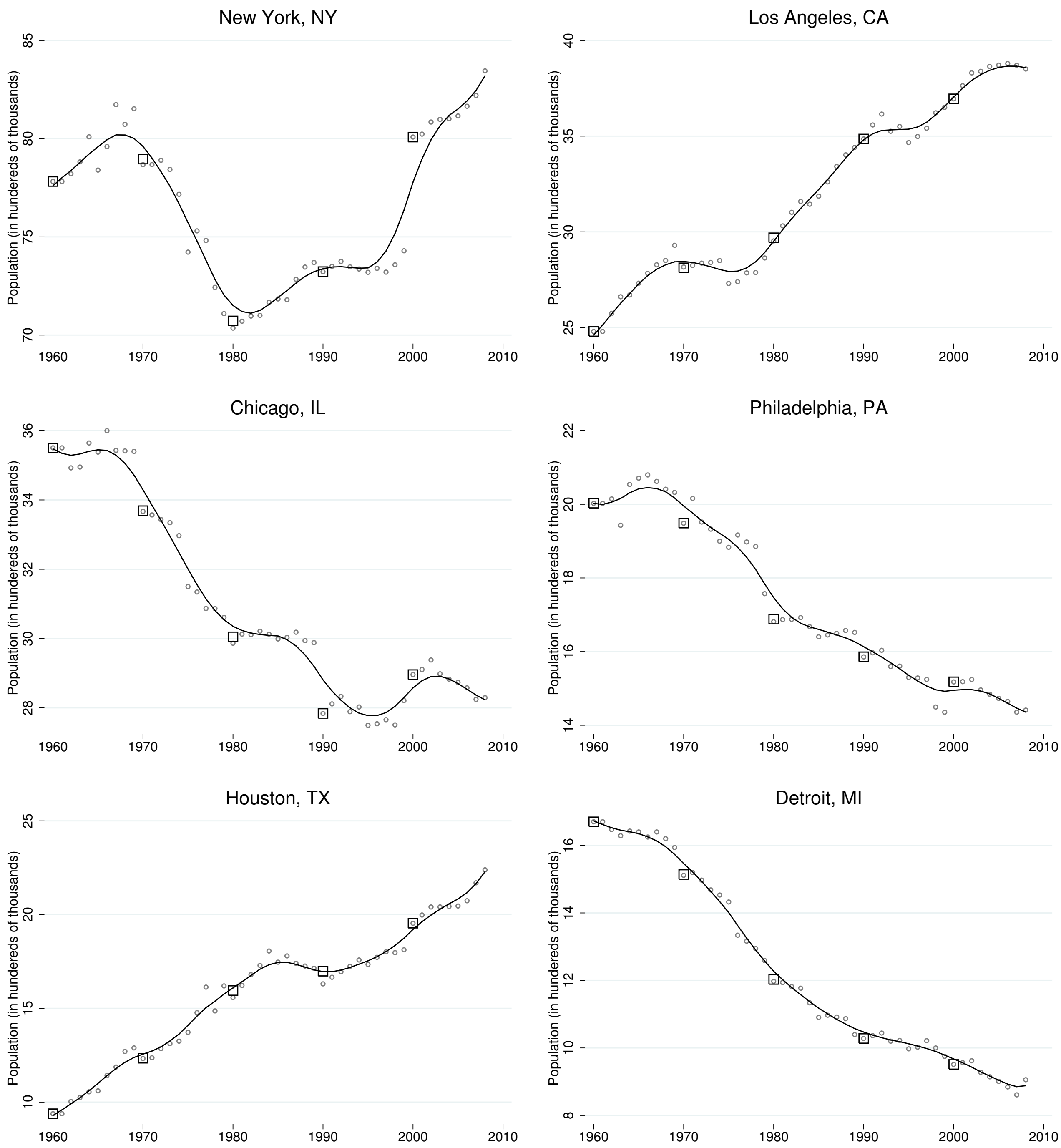
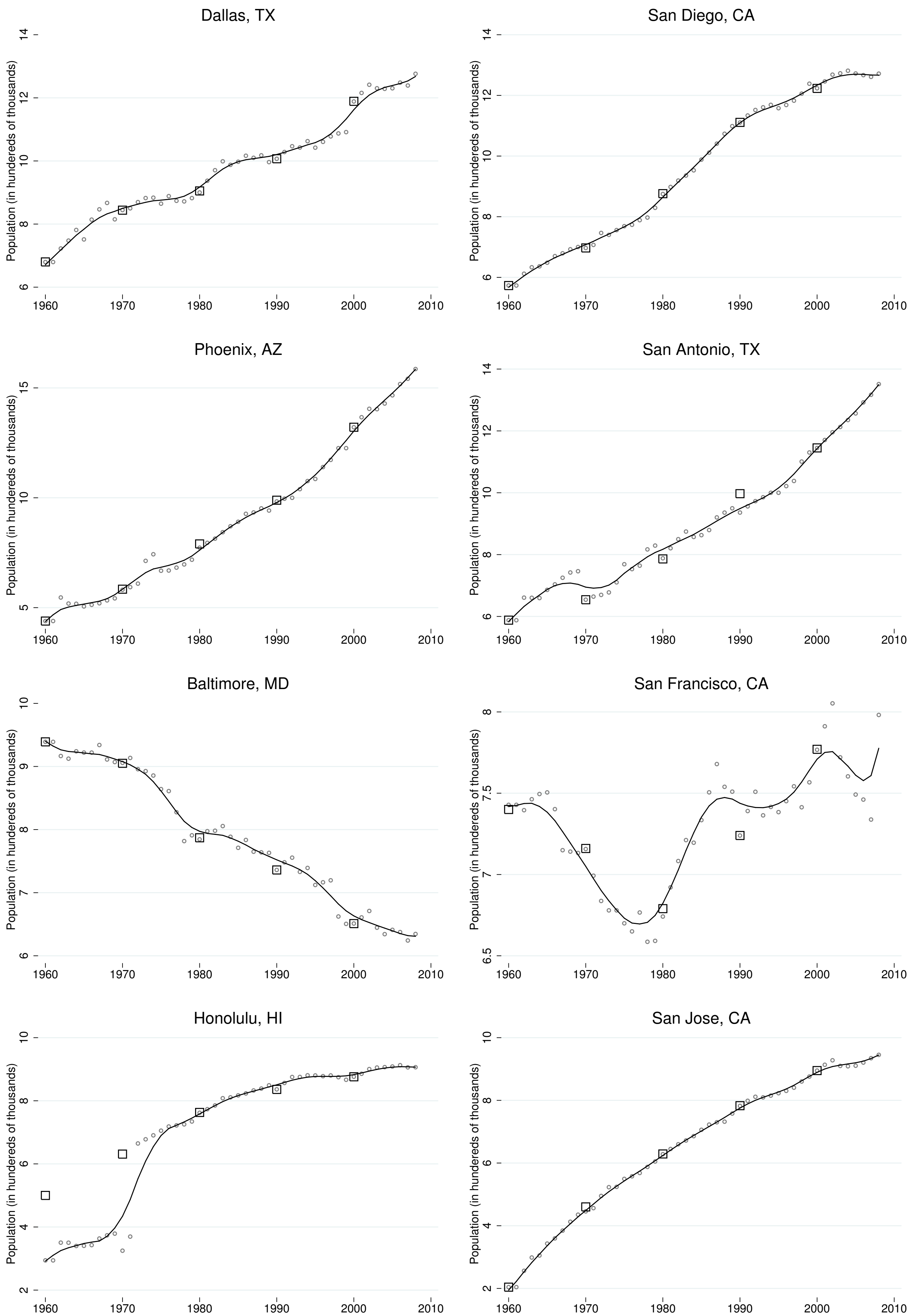
Waco, TX
Stamford, CT
Waterbury, CT
Allentown, PA
Abilene, TX
Springfield, IL
Log real 2010 dollars per capita
1960
1970
1980
1990
2000
2010



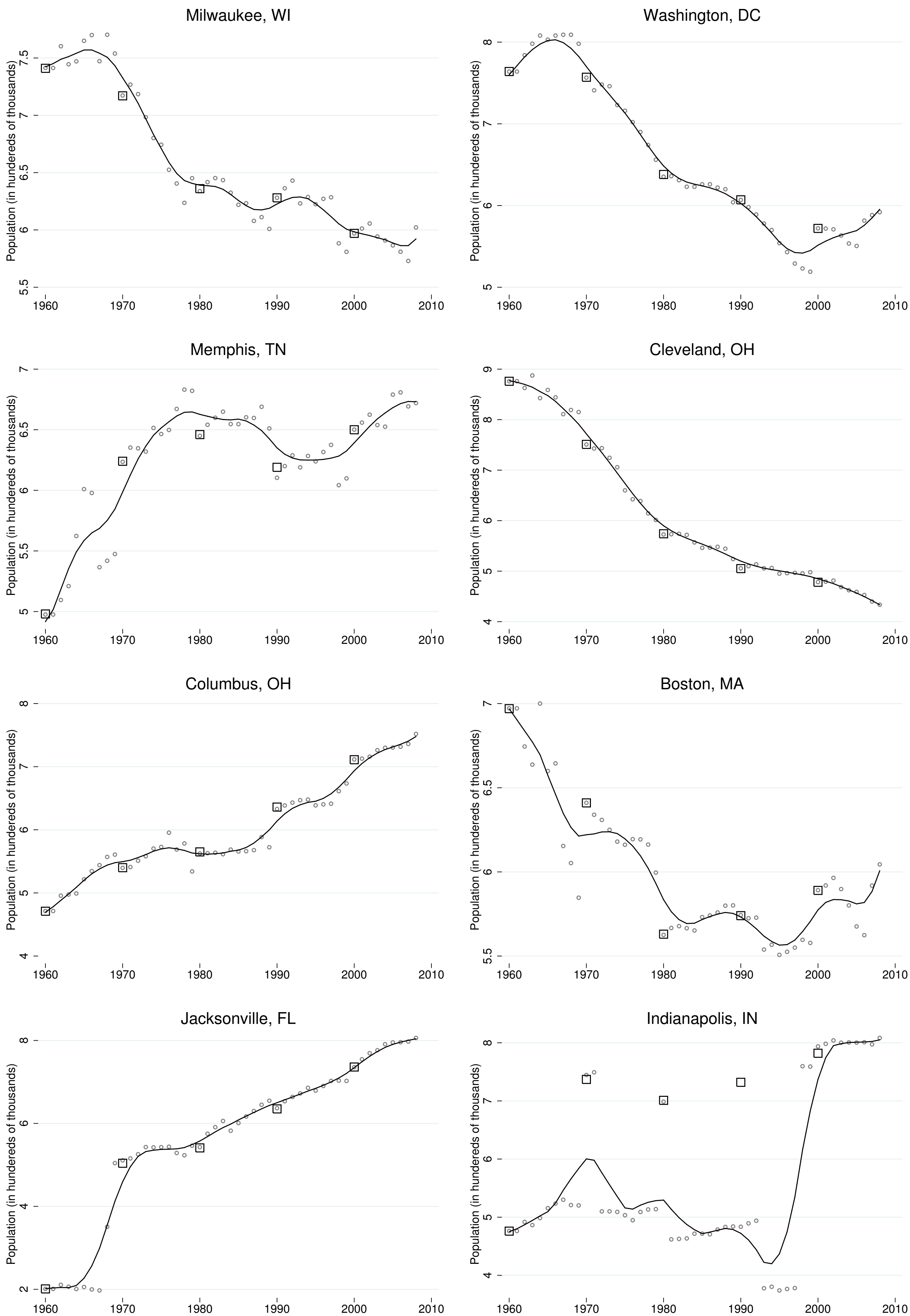
Livonia, MI
Log real 2010 dollars per capita
.5
0
−.5
−1
1960
1970
1980
1990
2000
2010

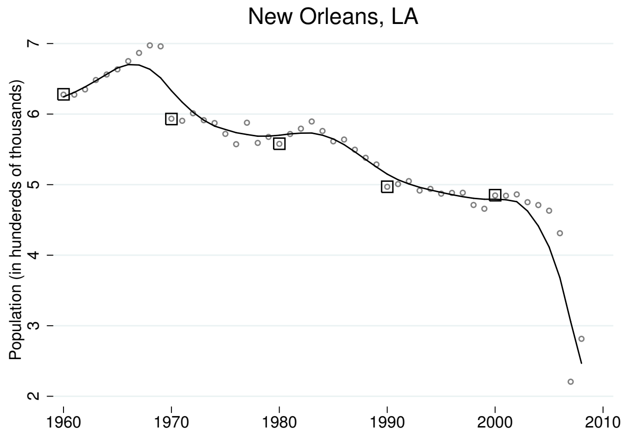
Appendix Figure 2. Estimated City Population

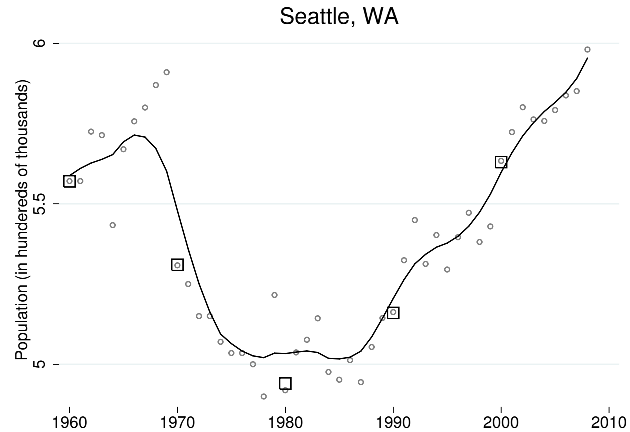


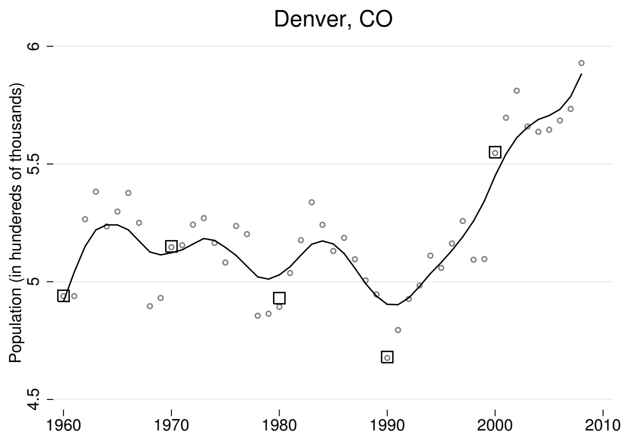


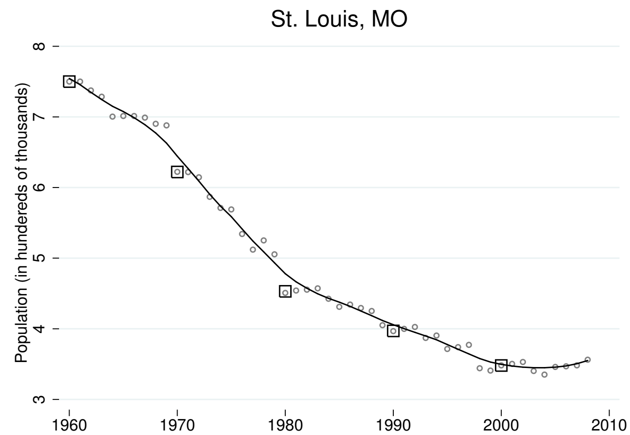
Dallas, TX
San Diego, CA
Phoenix, AZ
San Antonio, TX
Baltimore, MD
San Francisco, CA
Honolulu, HI
San Jose, CA
Population (in hundereds of thousands)
1960
1970
1980
1990
2000
2010

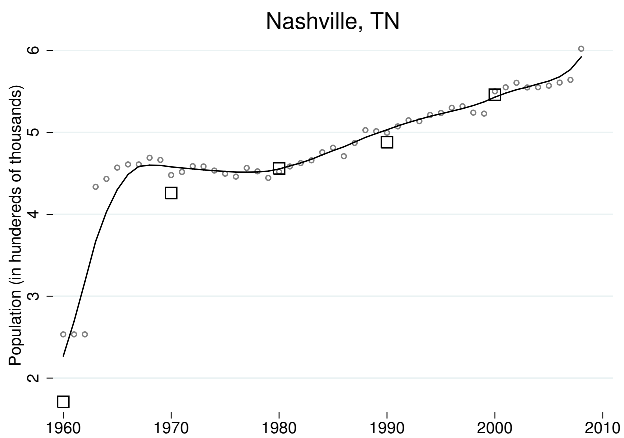

Milwaukee, WI
Washington, DC
Memphis, TN
Cleveland, OH
Columbus, OH
Boston, MA
Jacksonville, FL
Indianapolis, IN
Population (in hundereds of thousands)
1960
1970
1980
1990
2000
2010

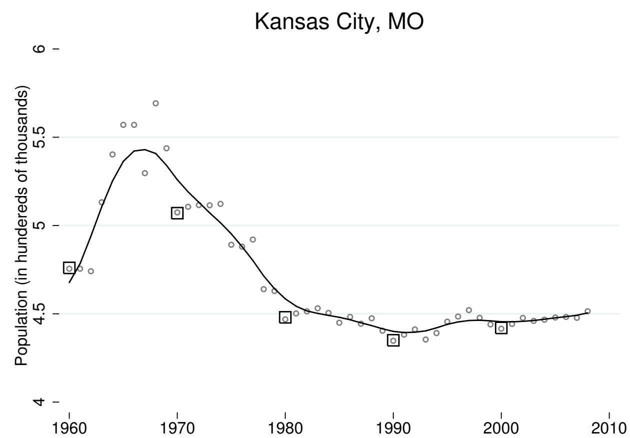

New Orleans, LA
Population (in hundereds of thousands)
2 3 4 5 6 7
1960 1970 1980 1990 2000 2010

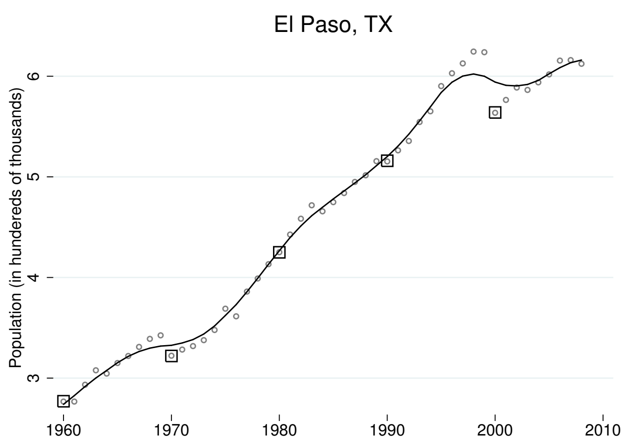

Seattle, WA
Population (in hundereds of thousands)
5 5.5 6
1960 1970 1980 1990 2000 2010

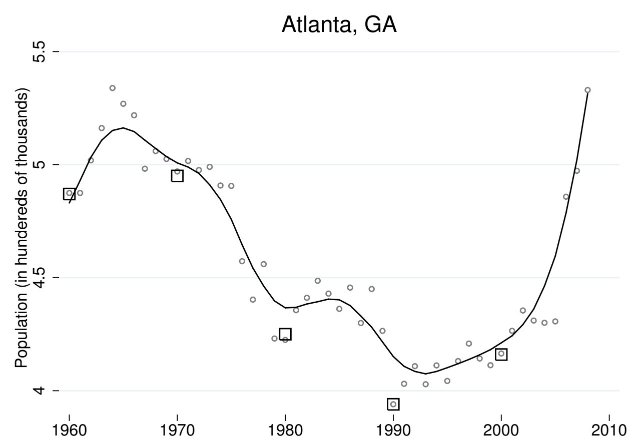

Denver, CO
Population (in hundereds of thousands)
4.5 5 5.5 6
1960 1970 1980 1990 2000 2010

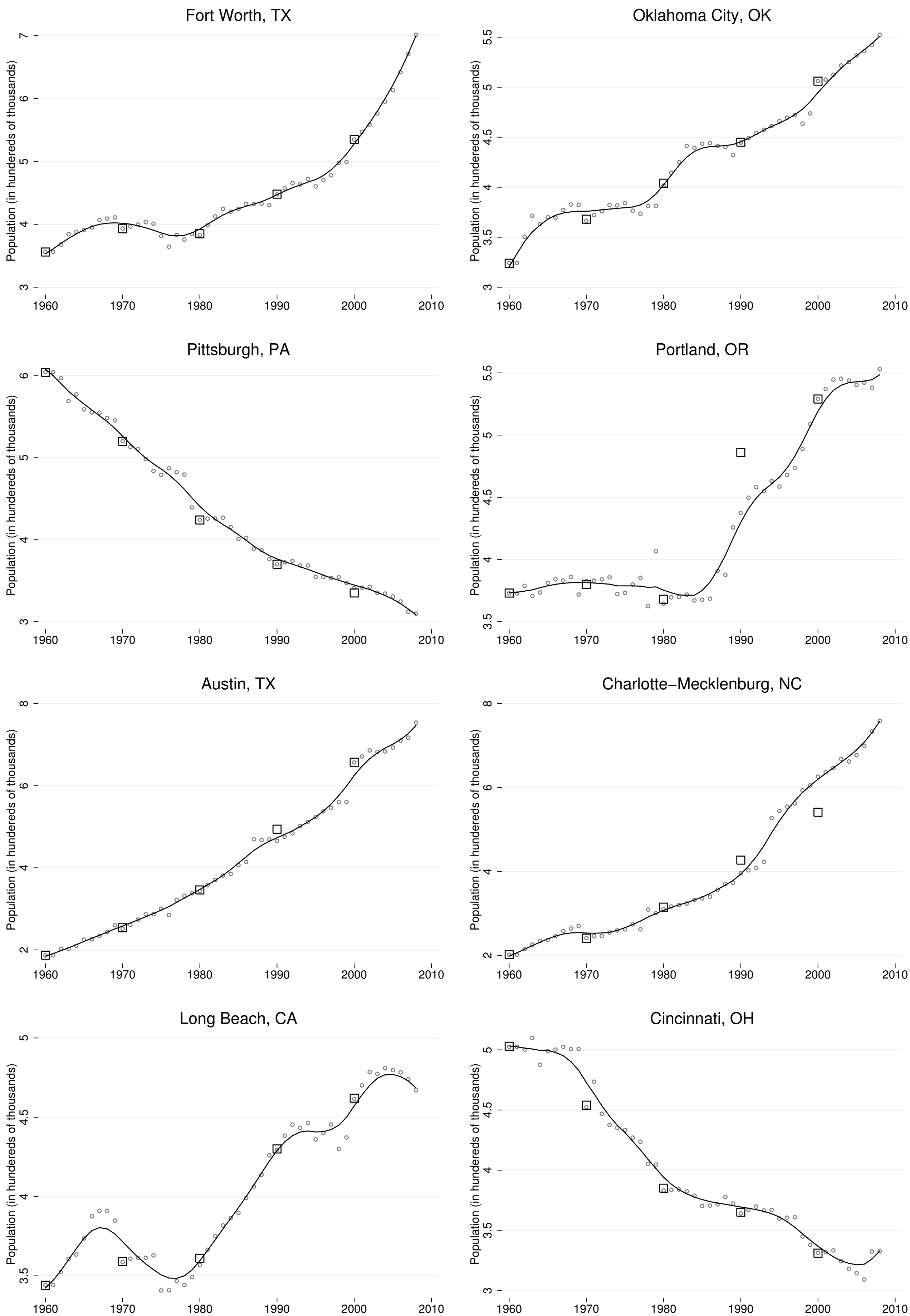

St. Louis, MO
Population (in hundereds of thousands)
3 4 5 6 7 8
1960 1970 1980 1990 2000 2010


Nashville, TN
Population (in hundereds of thousands)
2 3 4 5 6
1960 1970 1980 1990 2000 2010

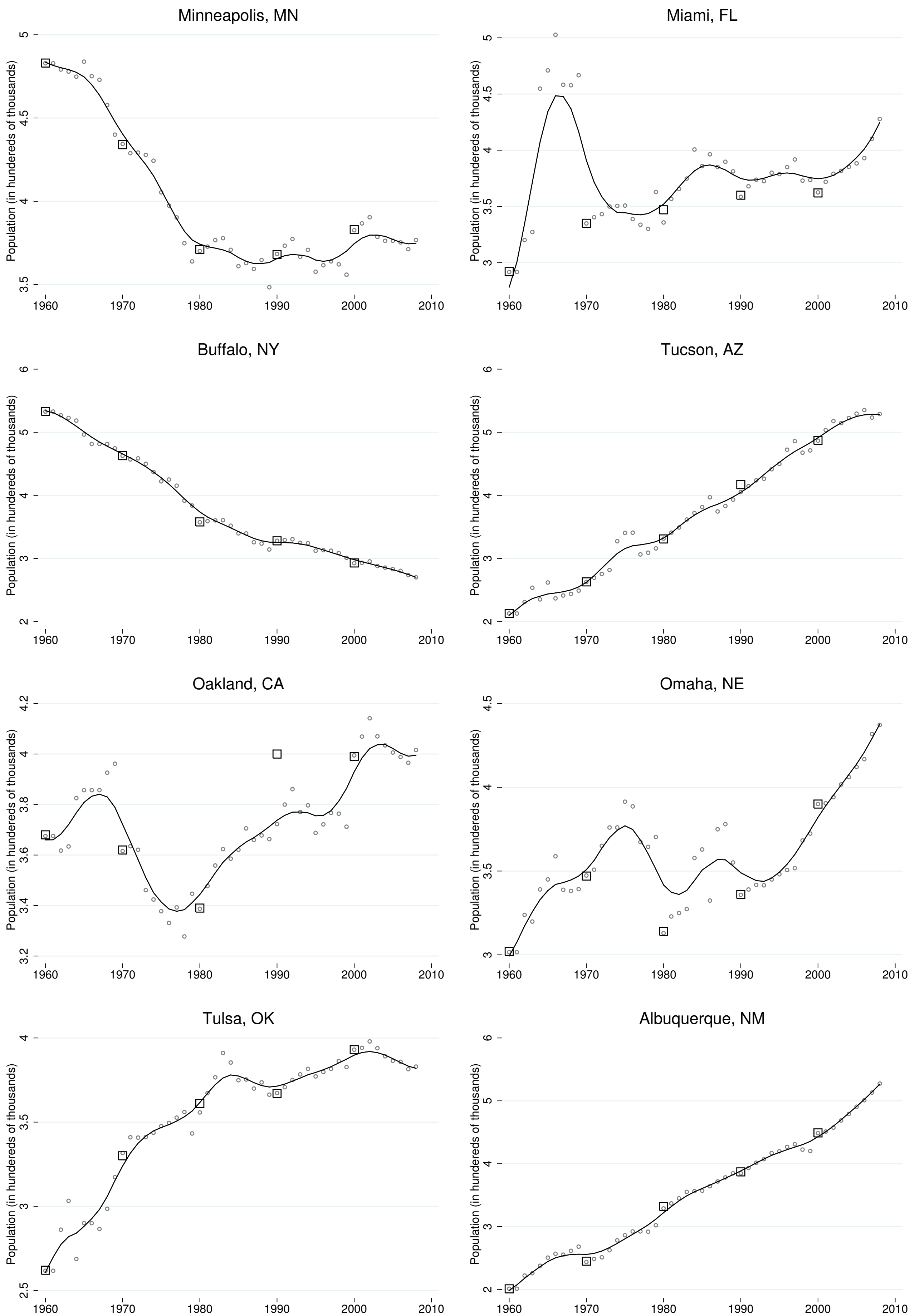

Kansas City, MO
Population (in hundereds of thousands)
4 4.5 5 5.5 6
1960 1970 1980 1990 2000 2010

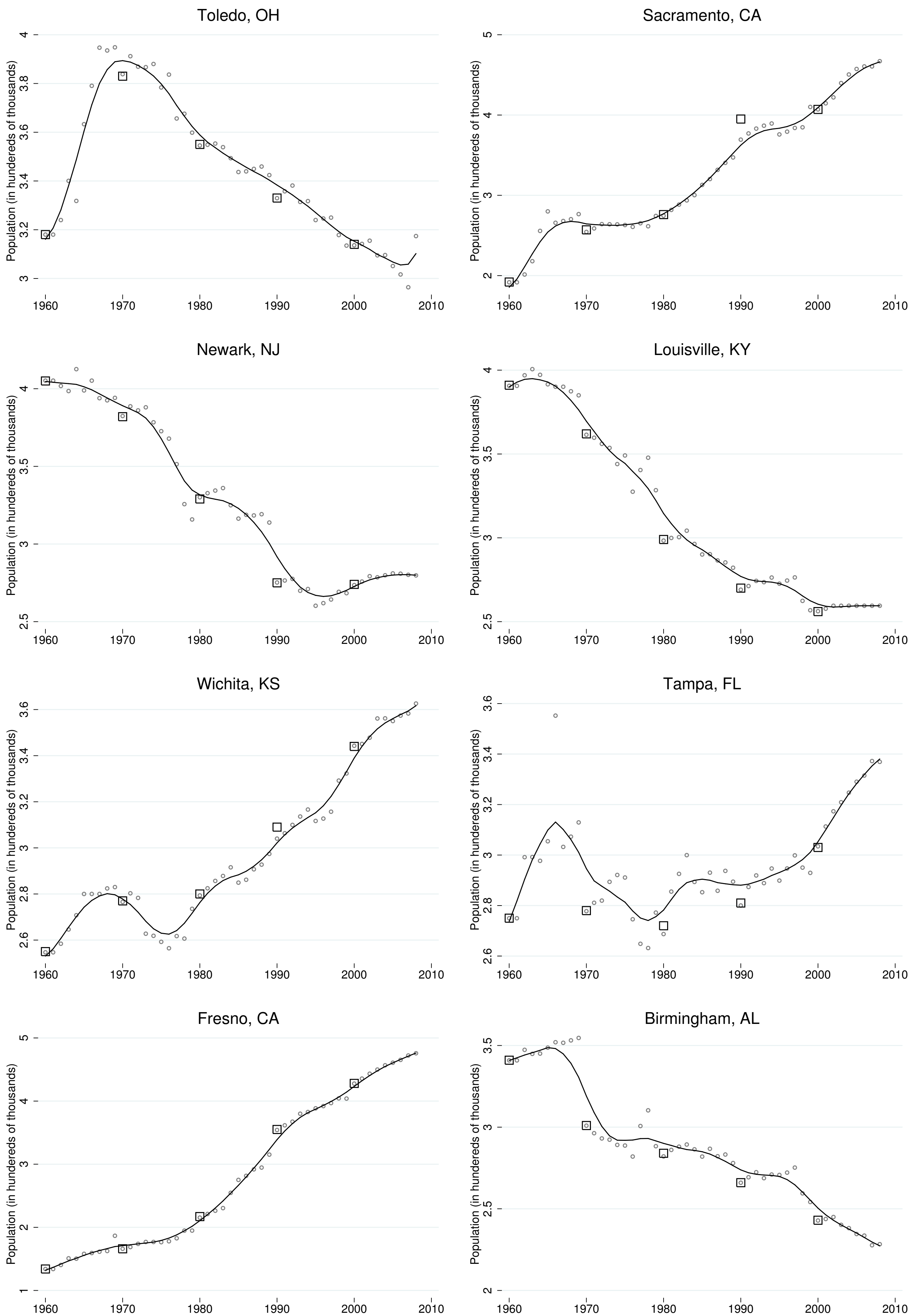

El Paso, TX
Population (in hundereds of thousands)
3 4 5 6
1960 1970 1980 1990 2000 2010

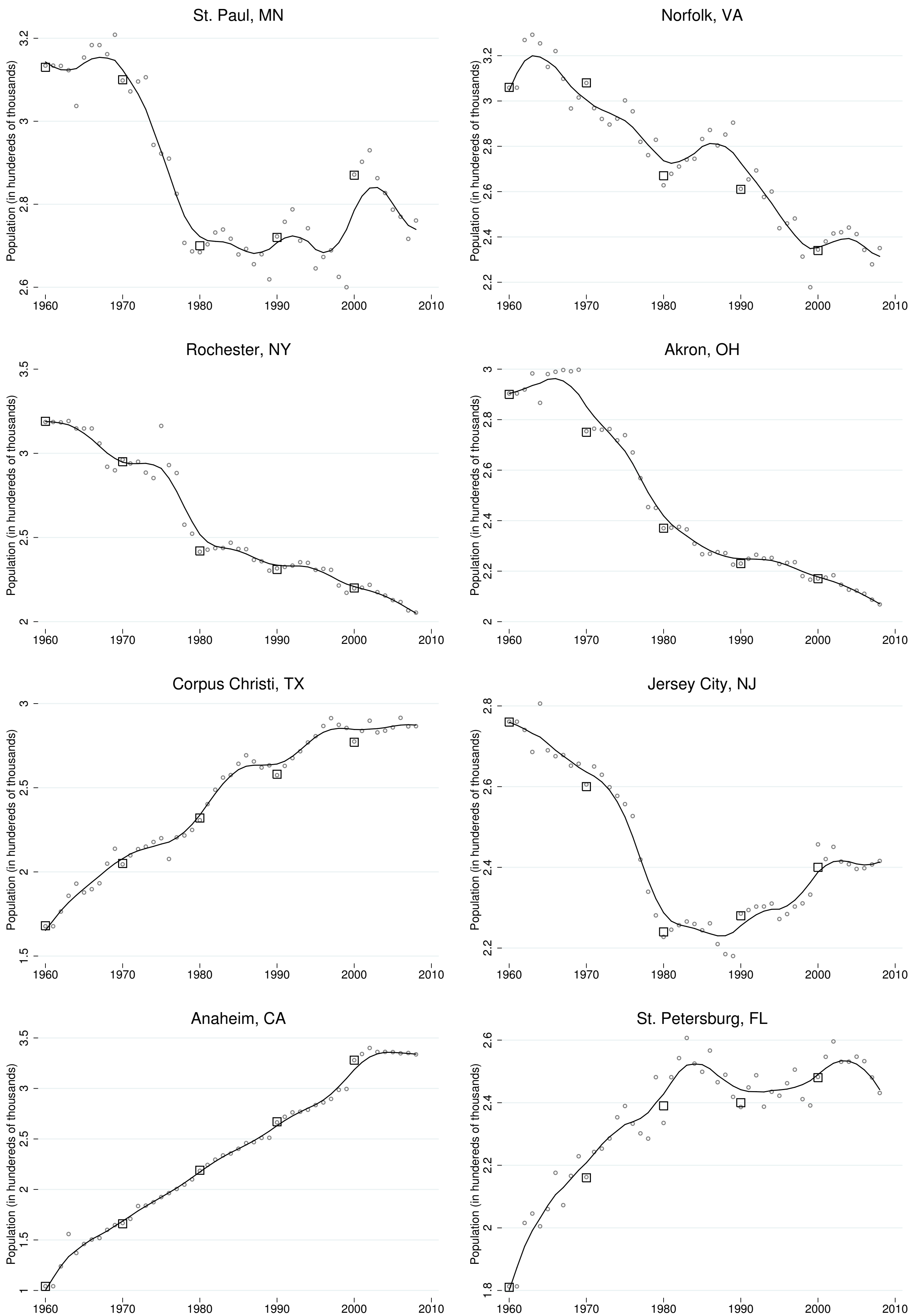

Atlanta, GA
Population (in hundereds of thousands)
4 4.5 5 5.5
1960 1970 1980 1990 2000 2010

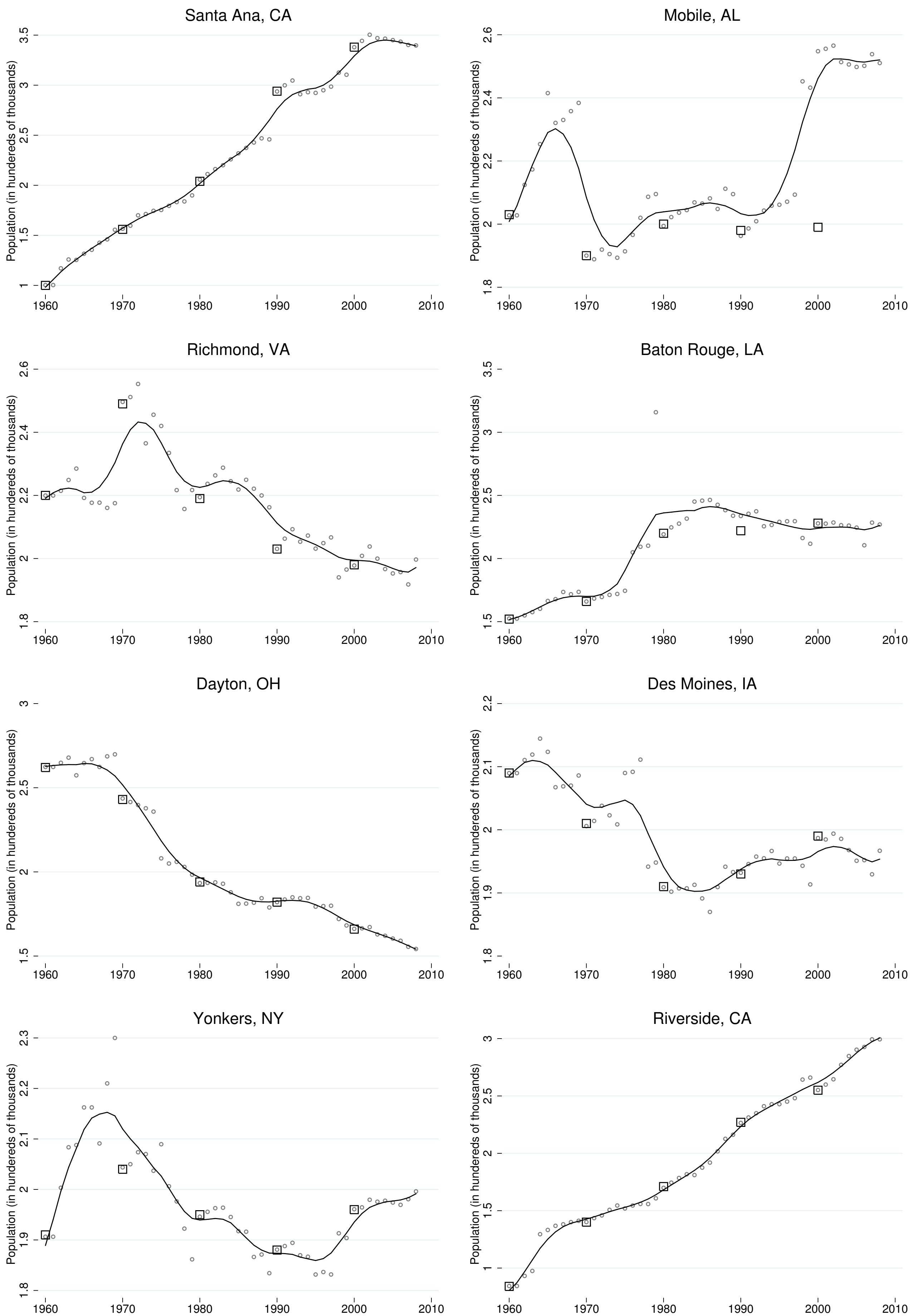


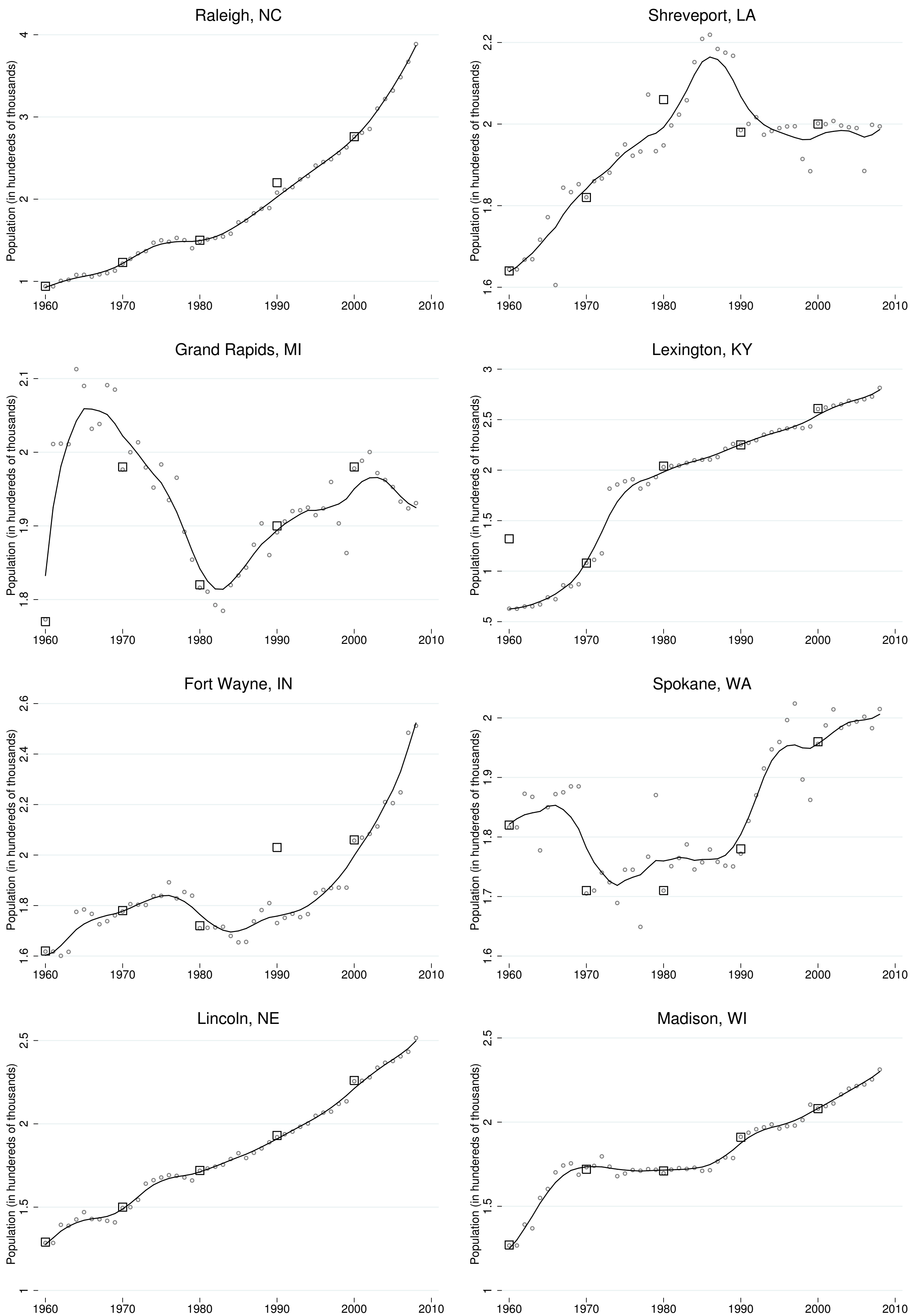
Fort Worth, TX
Oklahoma City, OK
Pittsburgh, PA
Portland, OR
Austin, TX
Charlotte−Mecklenburg, NC
Long Beach, CA
Cincinnati, OH
Population (in hundereds of thousands)
1960
1970
1980
1990
2000
2010


Minneapolis, MN
Miami, FL
Buffalo, NY
Tucson, AZ
Oakland, CA
Omaha, NE
Tulsa, OK
Albuquerque, NM
Population (in hundereds of thousands)

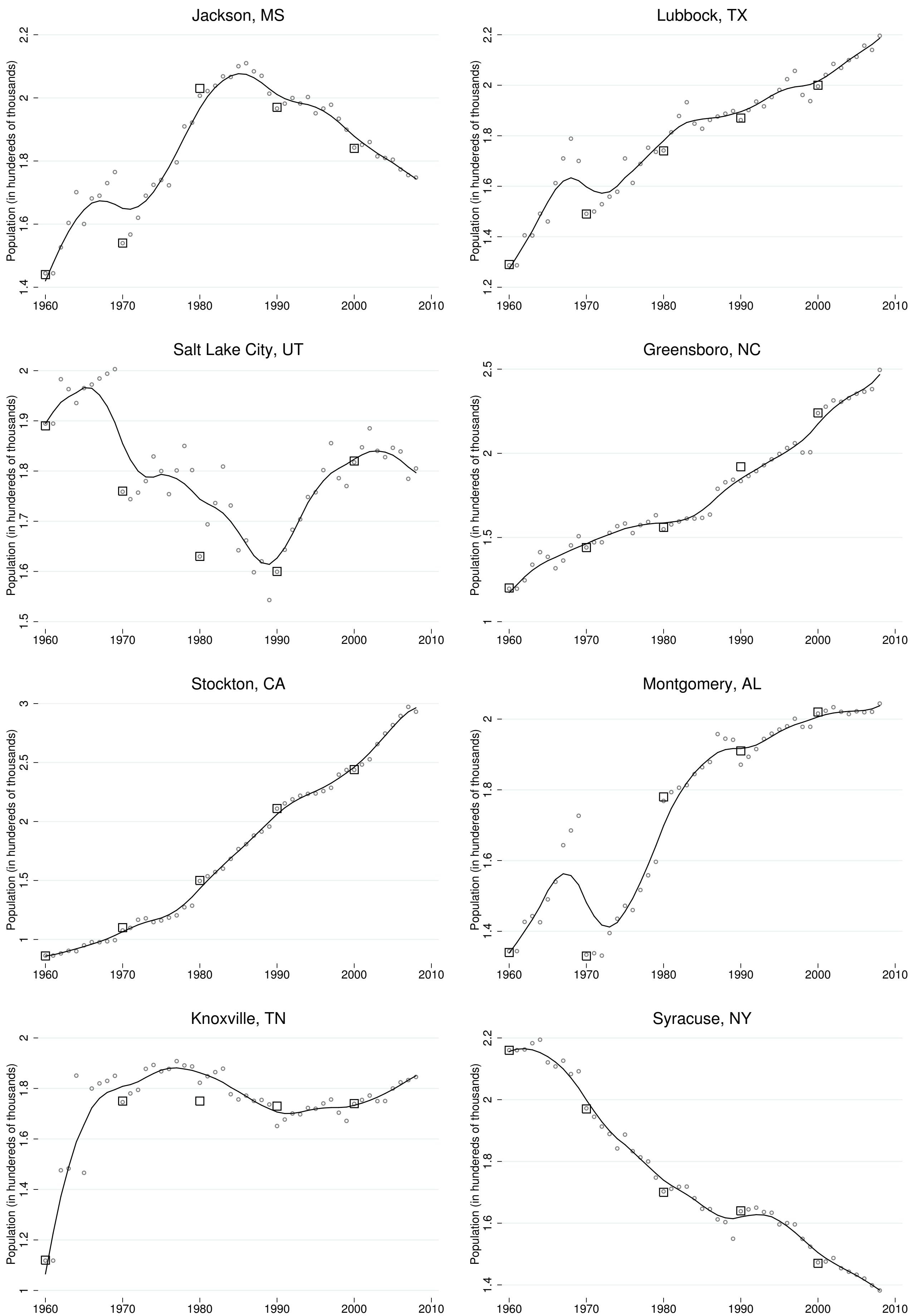

Toledo, OH
Sacramento, CA
Newark, NJ
Louisville, KY
Wichita, KS
Tampa, FL
Fresno, CA
Birmingham, AL
Population (in hundereds of thousands)
1960
1970
1980
1990
2000
2010

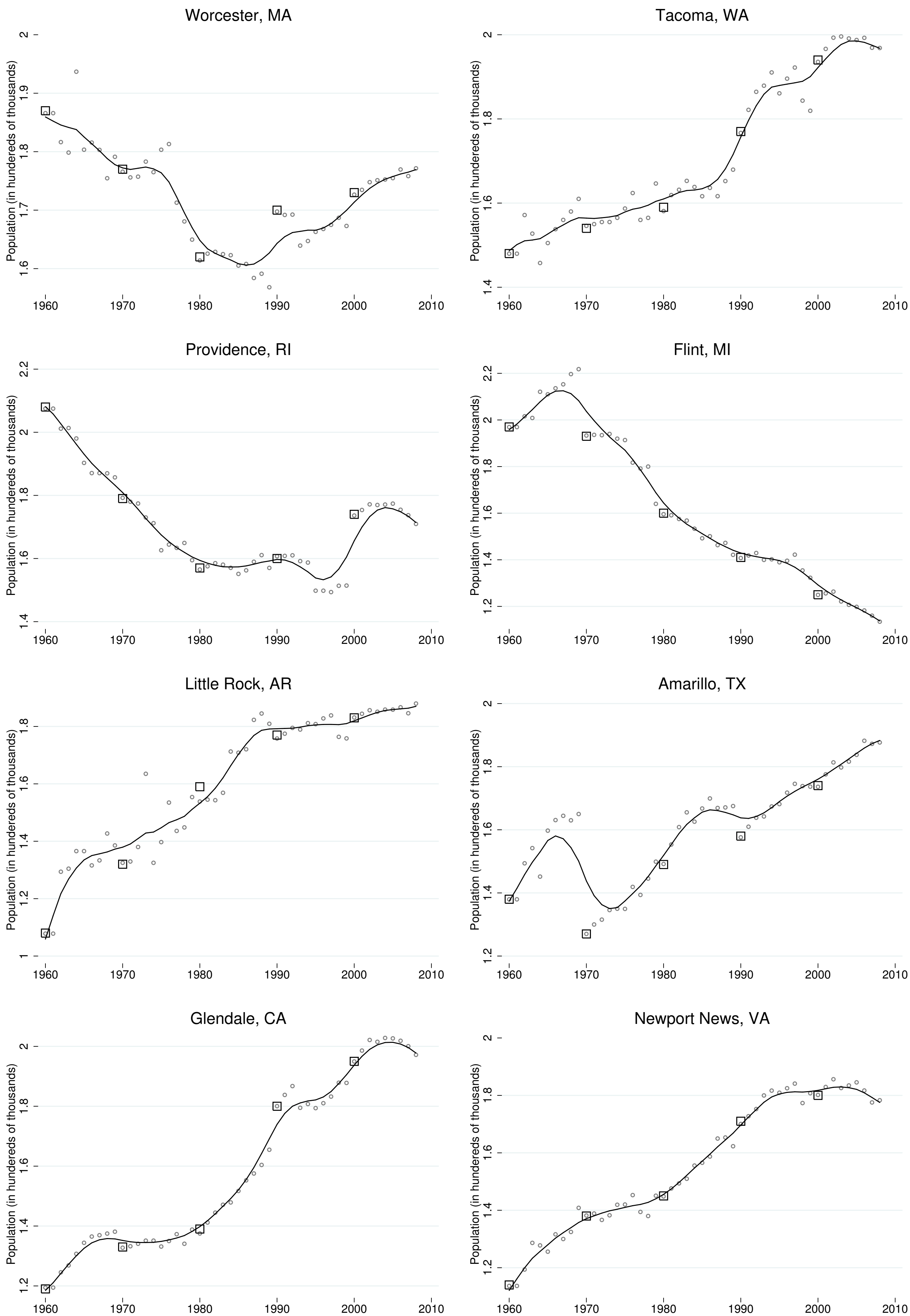

St. Paul, MN
Norfolk, VA
Rochester, NY
Akron, OH
Corpus Christi, TX
Jersey City, NJ
Anaheim, CA
St. Petersburg, FL
Population (in hundereds of thousands)
1960
1970
1980
1990
2000
2010

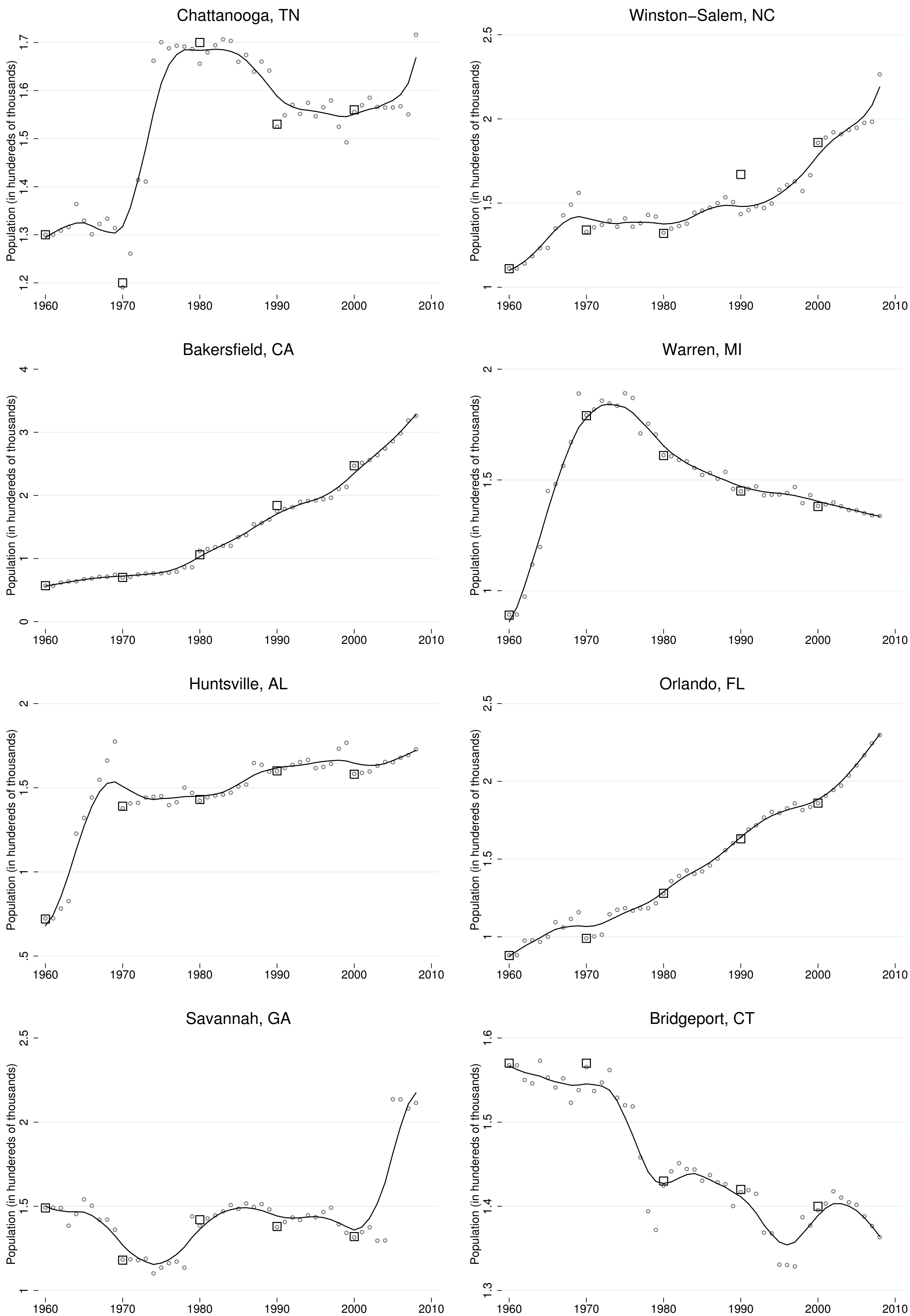

Santa Ana, CA
Mobile, AL
Richmond, VA
Baton Rouge, LA
Dayton, OH
Des Moines, IA
Yonkers, NY
Riverside, CA
Population (in hundereds of thousands)
1960
1970
1980
1990
2000
2010

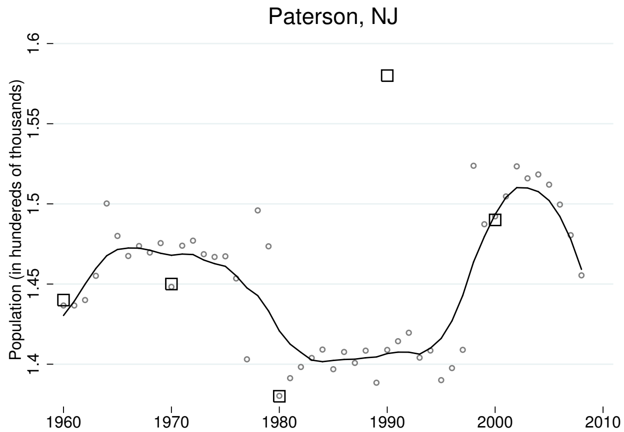


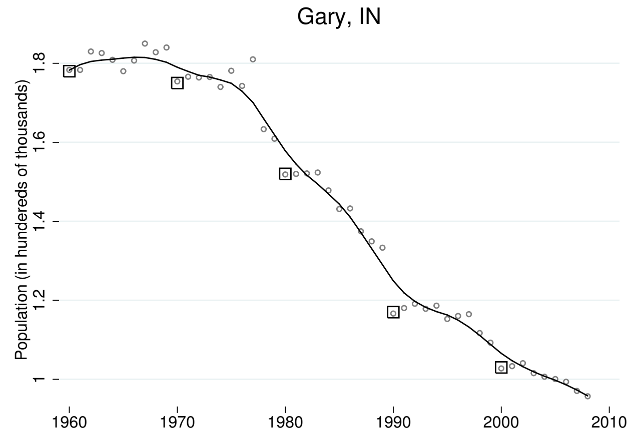
Raleigh, NC
Shreveport, LA
Grand Rapids, MI
Lexington, KY
Fort Wayne, IN
Spokane, WA
Lincoln, NE
Madison, WI
Population (in hundereds of thousands)
1960
1970
1980
1990
2000
2010

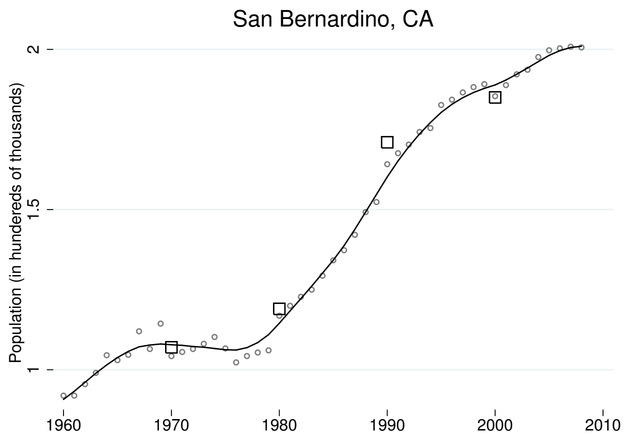

Jackson, MS
Lubbock, TX
Salt Lake City, UT
Greensboro, NC
Stockton, CA
Montgomery, AL
Knoxville, TN
Syracuse, NY
Population (in hundereds of thousands)
1960
1970
1980
1990
2000
2010

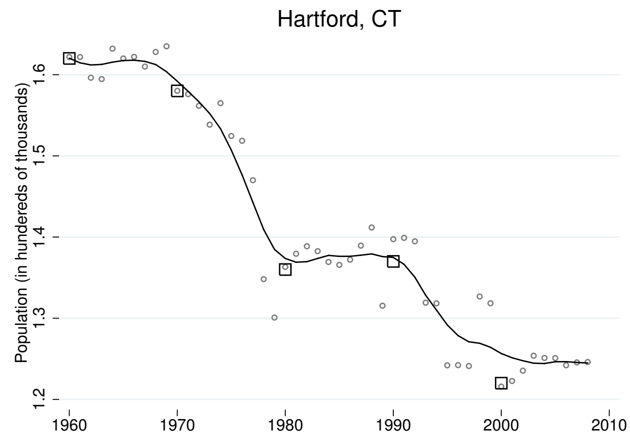

Worcester, MA
Tacoma, WA
Providence, RI
Flint, MI
Little Rock, AR
Amarillo, TX
Glendale, CA
Newport News, VA
Population (in hundereds of thousands)
1960
1970
1980
1990
2000
2010

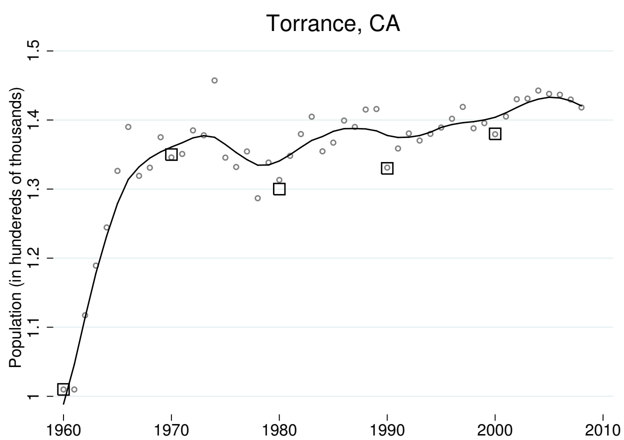

Chattanooga, TN
Winston−Salem, NC
Bakersfield, CA
Warren, MI
Huntsville, AL
Orlando, FL
Savannah, GA
Bridgeport, CT
Population (in hundereds of thousands)
1960
1970
1980
1990
2000
2010

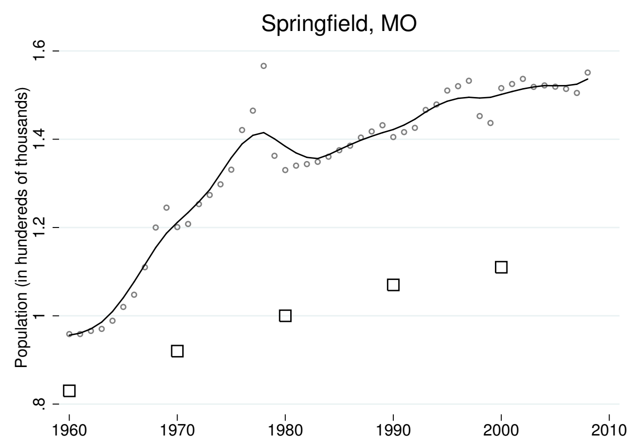

Paterson, NJ
Population (in hundereds of thousands)
1.4
1.45
1.5
1.55
1.6
1960
1970
1980
1990
2000
2010

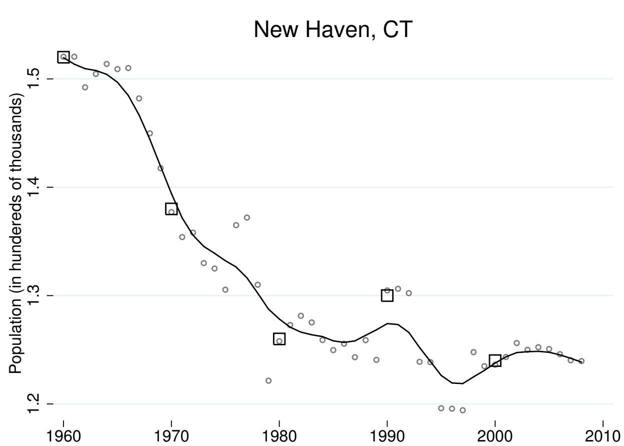

Gary, IN
Population (in hundereds of thousands)
1
1.2
1.4
1.6
1.8
1960
1970
1980
1990
2000
2010

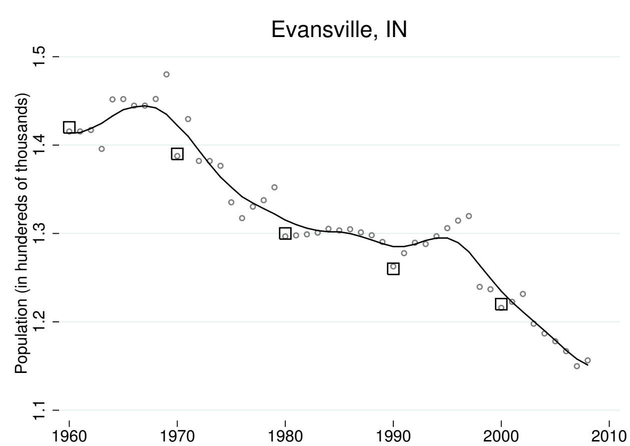

San Bernardino, CA
Population (in hundereds of thousands)
1
1.5
2
1960
1970
1980
1990
2000
2010

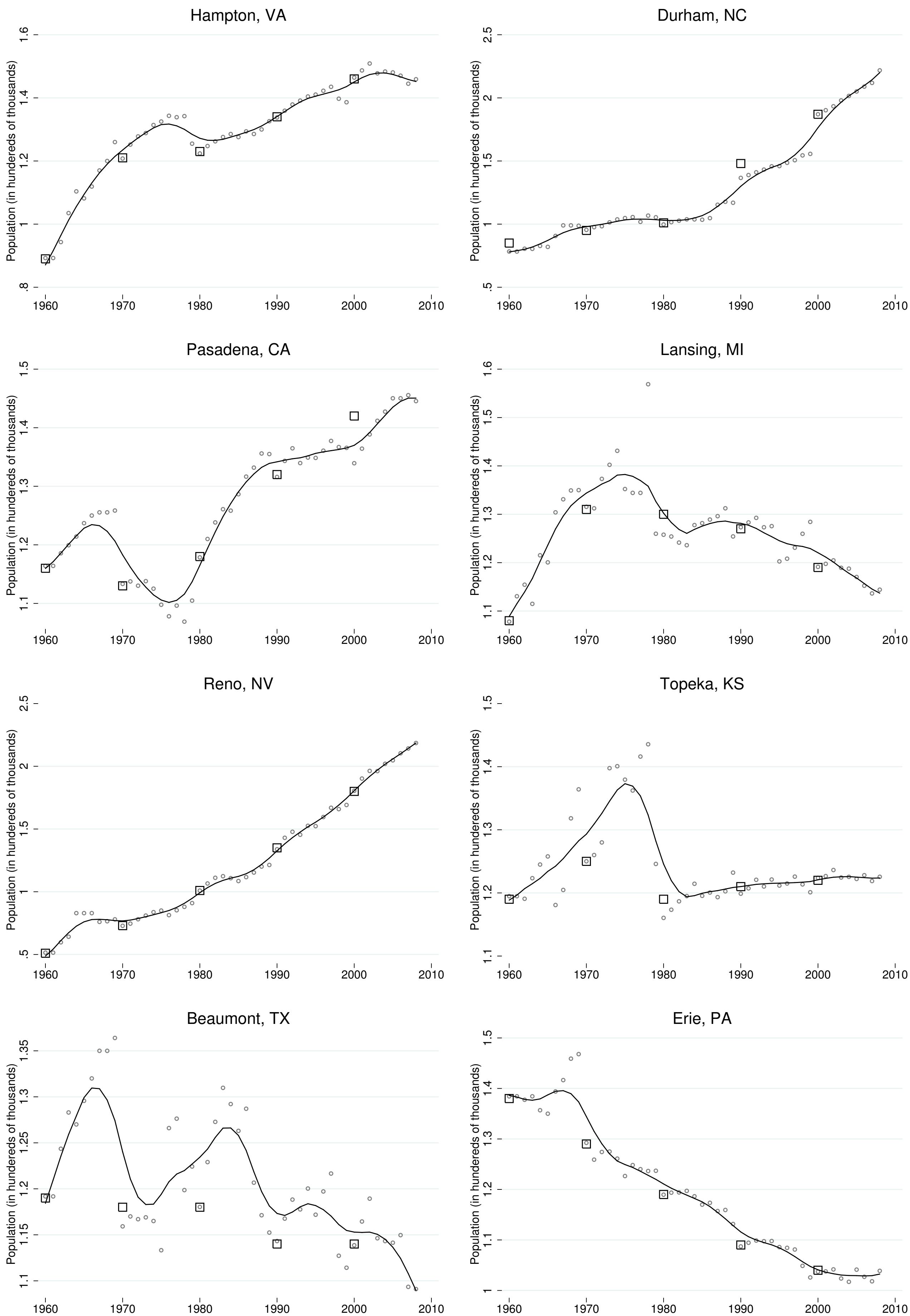

Hartford, CT
Population (in hundereds of thousands)
1.2
1.3
1.4
1.5
1.6
1960
1970
1980
1990
2000
2010

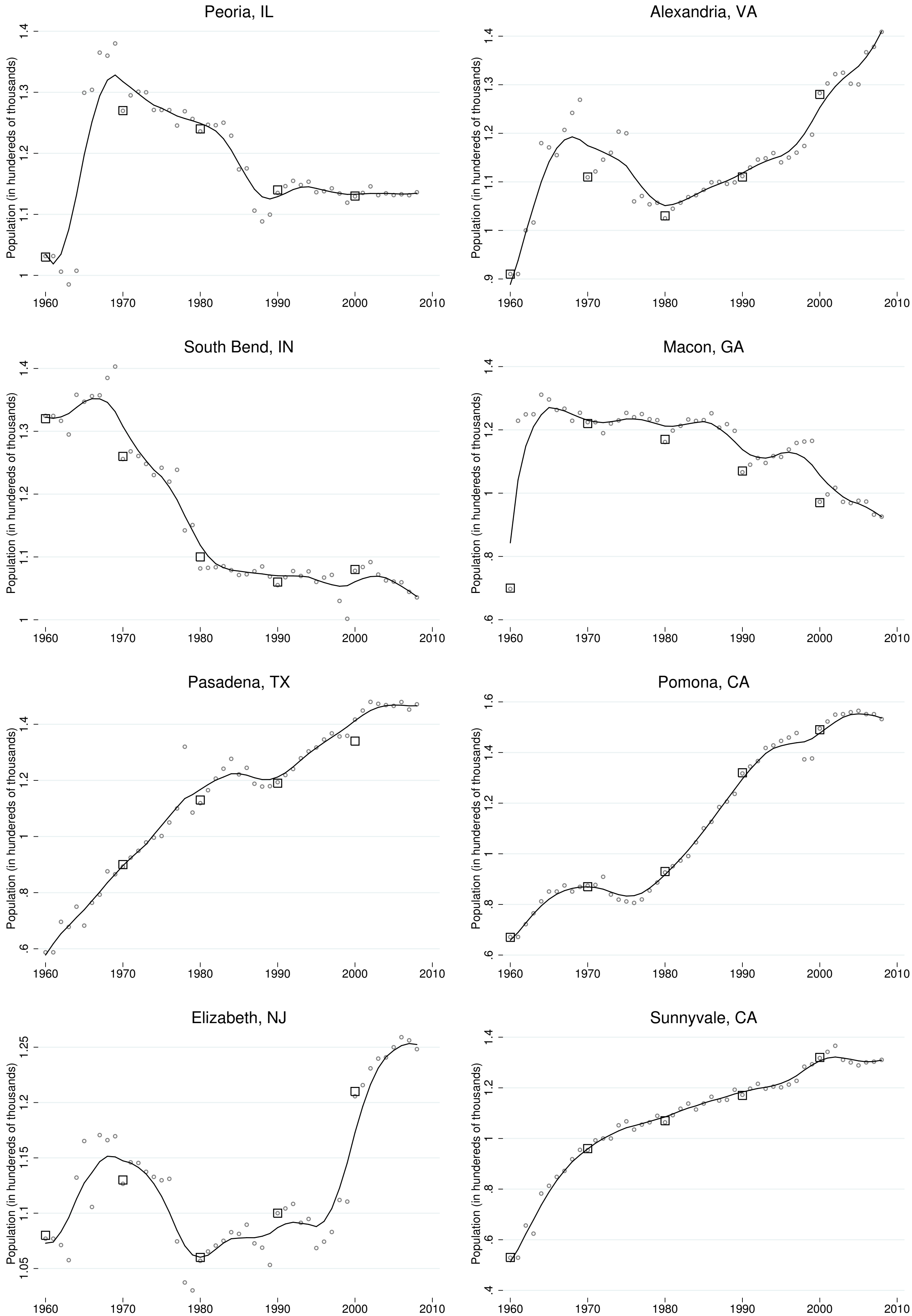

Torrance, CA
Population (in hundereds of thousands)
1
1.1
1.2
1.3
1.4
1.5
1960
1970
1980
1990
2000
2010

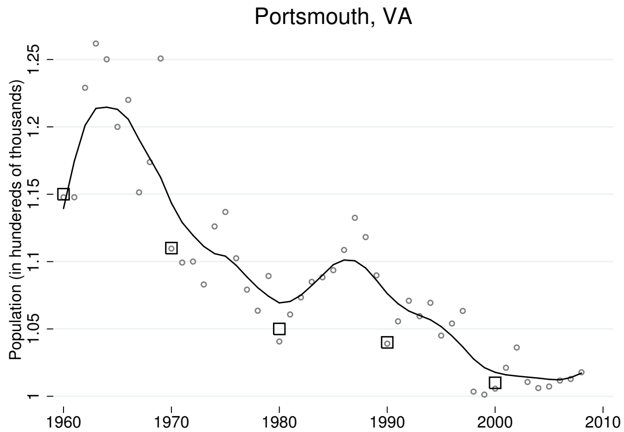

Springfield, MO
Population (in hundereds of thousands)
.8
1
1.2
1.4
1.6
1960
1970
1980
1990
2000
2010

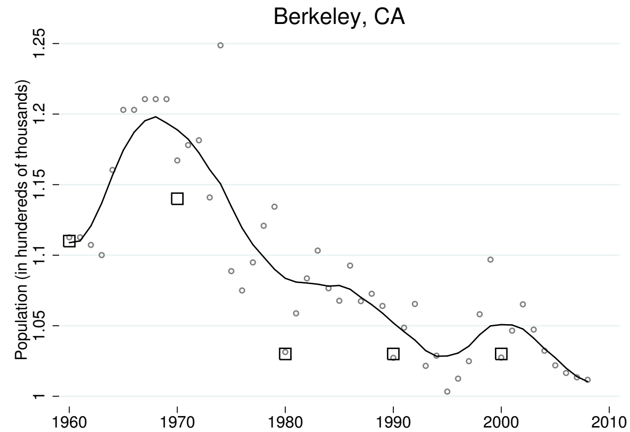

New Haven, CT
Population (in hundereds of thousands)
1.2
1.3
1.4
1.5
1960
1970
1980
1990
2000
2010

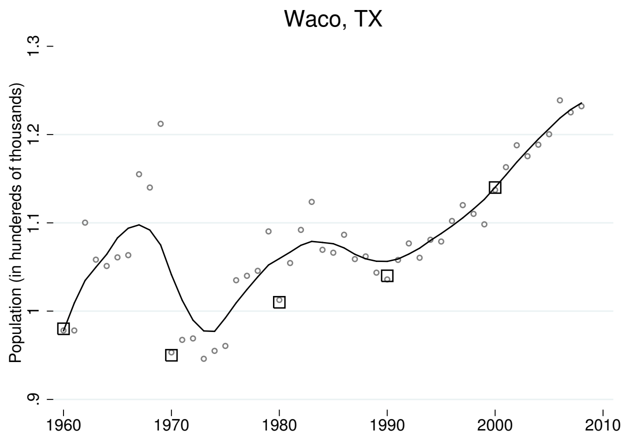

Evansville, IN
Population (in hundereds of thousands)
1.1
1.2
1.3
1.4
1.5
1960
1970
1980
1990
2000
2010

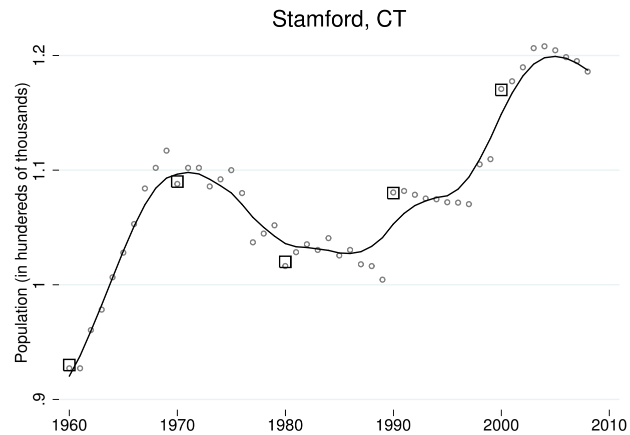

Hampton, VA
Population (in hundereds of thousands)
.8
1
1.2
1.4
1.6
1960
1970
1980
1990
2000
2010
Durham, NC
Population (in hundereds of thousands)
.5
1
1.5
2
2.5
1960
1970
1980
1990
2000
2010
Pasadena, CA
Population (in hundereds of thousands)
1.1
1.2
1.3
1.4
1.5
1960
1970
1980
1990
2000
2010
Lansing, MI
Population (in hundereds of thousands)
1.1
1.2
1.3
1.4
1.5
1.6
1960
1970
1980
1990
2000
2010
Reno, NV
Population (in hundereds of thousands)
.5
1
1.5
2
2.5
1960
1970
1980
1990
2000
2010
Topeka, KS
Population (in hundereds of thousands)
1.1
1.2
1.3
1.4
1.5
1960
1970
1980
1990
2000
2010
Beaumont, TX
Population (in hundereds of thousands)
1.1
1.15
1.2
1.25
1.3
1.35
1960
1970
1980
1990
2000
2010
Erie, PA
Population (in hundereds of thousands)
1
1.1
1.2
1.3
1.4
1.5
1960
1970
1980
1990
2000
2010

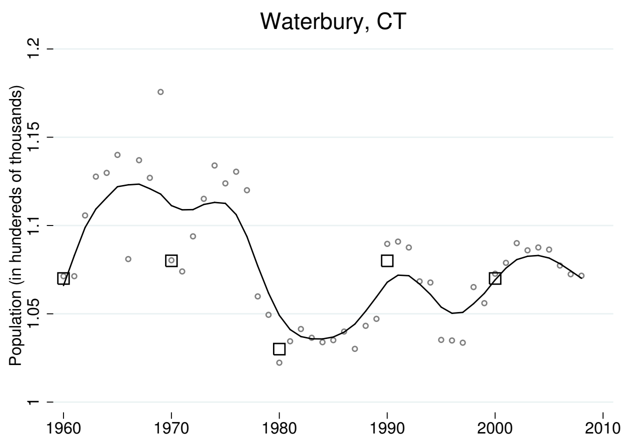

Peoria, IL
Alexandria, VA
South Bend, IN
Macon, GA
Pasadena, TX
Pomona, CA
Elizabeth, NJ
Sunnyvale, CA
Population (in hundereds of thousands)
1960
1970
1980
1990
2000
2010

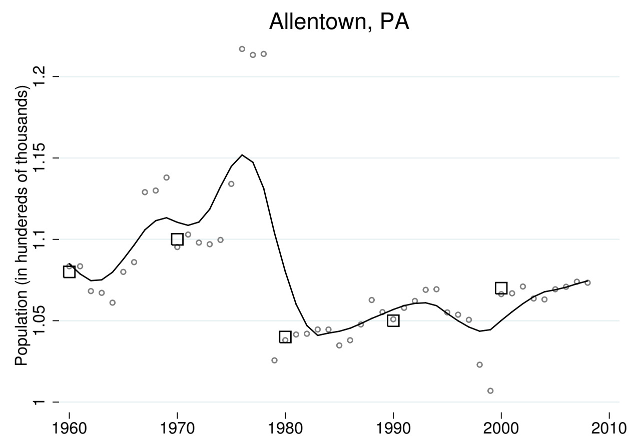

Portsmouth, VA
Population (in hundereds of thousands)
1 1.05 1.1 1.15 1.2 1.25
1960 1970 1980 1990 2000 2010

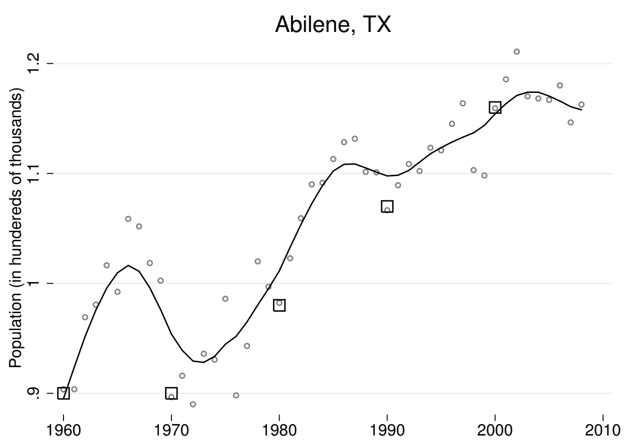

Berkeley, CA
Population (in hundereds of thousands)
1 1.05 1.1 1.15 1.2 1.25
1960 1970 1980 1990 2000 2010

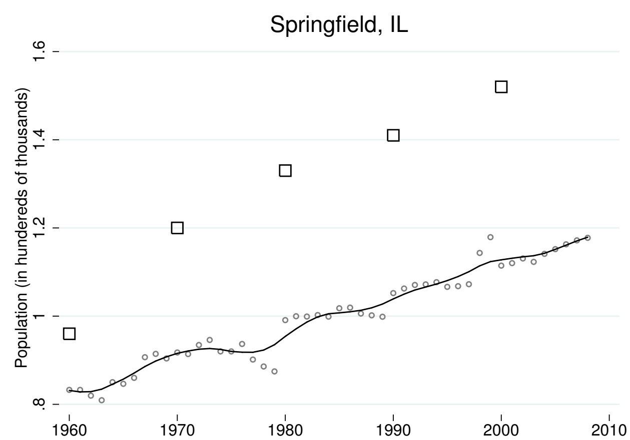

Waco, TX
Population (in hundereds of thousands)
.9 1 1.1 1.2 1.3
1960 1970 1980 1990 2000 2010

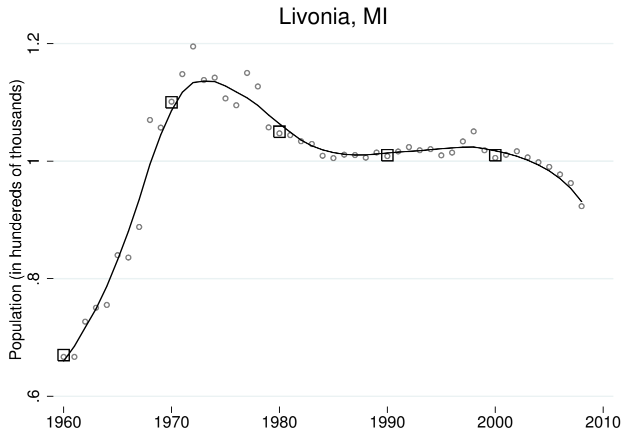

Stamford, CT
Population (in hundereds of thousands)
.9 1 1.1 1.2
1960 1970 1980 1990 2000 2010


Waterbury, CT
Population (in hundereds of thousands)
1 1.05 1.1 1.15 1.2
1960 1970 1980 1990 2000 2010


Allentown, PA
Population (in hundereds of thousands)
1 1.05 1.1 1.15 1.2
1960 1970 1980 1990 2000 2010


Abilene, TX
Population (in hundereds of thousands)
.9 1 1.1 1.2
1960 1970 1980 1990 2000 2010


Springfield, IL
Population (in hundereds of thousands)
.8 1 1.2 1.4 1.6
1960 1970 1980 1990 2000 2010



Livonia, MI
Population (in hundereds of thousands)
1.2
1
.8
.6
1960
1970
1980
1990
2000
2010