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City of Stockton

UNITED STATES BANKRUPTCY COURT

EASTERN DISTRICT OF CALIFORNIA

SACRAMENTO DIVISION

In re: CITY OF STOCKTON, CALIFORNIA, Debtor.	Case No. 2012-32118 D.C. No. OHS-1 Chapter 9 **DECLARATION OF JUSTIN MCCRARY IN SUPPORT OF CITY OF STOCKTON'S REPLY TO OBJECTIONS TO ITS STATEMENT OF QUALIFICATIONS UNDER SECTION 109(C) OF THE UNITED STATES BANKRUPTCY CODE** Date: February 26, 2013 Time: 1:30 p.m. Dept: C Judge: Hon. Christopher M. Klein

I, Justin McCrary, hereby declare:

1. I am a law professor and co-director of the Law and Economics Program at the University of California, Berkeley. I am also co-director of the Crime Working Group of the National Bureau of Economic Research. I make this declaration in support of the City's Reply to Objections to Statement of Qualifications Under Section 109(c).

2. I have been engaged by the City of Stockton's counsel, Orrick, Herrington & Sutcliffe LLP, to assess the opinions, conclusions, and analysis set forth in the declarations and expert reports of David Neumark and Joseph Brann, as clarified in their depositions, and to provide my opinions regarding their reports, declarations, and testimony.

3. I have reviewed the declarations and reports of Mr. Brann and Professor Neumark, filed by the so-called Capital Markets Creditors on December 14, 2012. I also attended Mr. Brann's deposition on January 24, 2013, and Professor Neumark's deposition on February 5, 2013. While Mr. Brann has significant experience in policing and Professor Neumark in labor economics, neither has an expertise that bridges crime and labor economics, the two subject areas I believe are most germane to analyzing the City's concerns regarding the reduction of pension benefits. By contrast, I received my Ph.D. in Economics in 2003, with a specialty in labor economics and econometrics from the University of California, Berkeley, and have published articles on crime, police departments, and statistical methodology in leading economics journals such as the *American Economic Review* and the *Journal of Econometrics*. Furthermore, the Crime Working Group of the National Bureau of Economic Research that I co-direct currently holds the highest quality annual scholarly meetings in the United States regarding crime, policing, and crime policy. Together with my co-directors in the group, I co-edited the book *Controlling Crime: Strategies and Tradeoffs*, published by the University of Chicago Press in 2011. A true and correct copy of my curriculum vitae is attached hereto as Exhibit A.

4. I disagree with Mr. Brann and Professor Neumark's opinion that a modest pension benefit reduction would not lead to a mass exodus of experienced officers, and their opinion that the City of Stockton's evidence on this question is insufficient. Brann Decl., ¶ 4; Neumark Decl., ¶ 5. Given the evidence I have reviewed for this engagement, my background knowledge of

crime, policing, crime policy, police hiring, and the internal functioning of police departments, and my background knowledge of labor economics, it is my opinion that the City of Stockton ("the City") is at a crisis point in regards to police department staffing. Further downgrades to compensation and benefits, such as the modest pension benefit reductions contemplated by Mr. Brann and Professor Neumark, in my opinion, are likely to push the City over the precipice, leading to a further acceleration of police department departures, including departures of some of the most able officers, and to worsened morale among those officers who remain with the department.

5. My disagreement with Mr. Brann and Professor Neumark in this regard is primarily based on the reasons set forth below, and more fully explained in my report ("the Report"), a true and correct copy of which is attached as Exhibit B:

a. The Stockton Police Department ("the Department") has already downgraded compensation and benefits over the past four years, which has accelerated the normal process of attrition at the Department.

b. Mr. Brann and Professor Neumark's alternative explanations for why officers have been departing Stockton neither account for the timing of the departures nor comport with the findings of contemporary economic literature.

c. Police officers, young and old, value their pensions highly, as is obvious by virtue of the fact that nearly every police department nationally offers a pension.

d. As a matter of common sense and economic theory, virtually every worker in the economy has a "trigger point," that is, a sufficiently severe downgrade to compensation and benefits at which he or she will seek employment elsewhere or simply stop working.

e. Given the downgrades in compensation and benefits since 2008 and the reductions in police staffing since 2008, which has left the same or more work to be done by fewer officers, a modest pension reduction would likely

bring a significant number of Stockton police officers to their "trigger point."

6. I also disagree with Mr. Brann's conclusion that a modest pension benefit reduction would not lead to increased crime. A pension downgrade would likely lead to a significant number of officer departures, which in turn would compromise the safety of the citizens of Stockton.

7. My disagreement with Mr. Brann in this regard arises from my extensive econometric research on the effect of police on crime and my associated review of the literature in economics and criminology on the effect of police on crime and the monetization of safety. My disagreement is primarily based upon the following, and is more fully explained in the Report:

a. Contrary to Mr. Brann's position, there is extensive, multi-decade literature establishing a causal connection between crime and police staffing levels.

b. In a paper I co-authored, "The Effect of Police on Crime: New Evidence from U.S. Cities, 1960-2010,"[1] my co-author and I concluded that as of 2010, Stockton was the number 2 most under-policed city among California cities with a population of 200,000 or more. Updating our rankings for the purposes of this case, I concluded that as of 2011, Stockton was the number 1 most under-policed city among California cities with a population of 200,000 or more, surpassing the previous leader, Oakland, California.

c. The conclusion that, as of 2011, Stockton was the single most under-policed city among California cities with a population of 200,000 or more is based on my empirical estimation that every $1 less spent on police costs would result in over $3 in crime costs for Stockton residents. I predict this result will be more extreme once complete crime and police staffing data

[1] Chalfin and McCrary (2013), NBER Working Paper 18815, attached hereto as Exhibit C.

for 2012 become available given that Stockton crime rates in 2012 were relatively high compared to other California cities.

d. My methodology for monetizing safety is widely accepted by state and federal governments as well as the economics and criminology literature. This acceptance is further detailed in the Report. Moreover, my methodology for calculating Stockton's cost-benefit-ratio for police hiring and crime costs is uncontroversial and further described in the Report and my paper, Chalfin and McCrary (2013) referenced above.

8. Thus, it is my opinion as an expert in labor economics and criminology that, in the interest of public safety, Stockton has legitimate concerns that modestly reducing pensions would exacerbate its police department's present retention and recruitment problems. Indeed, taking such a measure would not be cost-effective for Stockton, as I further describe in the Report.

Executed this 14 day of February 2013, at Kensington, California. I declare under penalty of perjury under the laws of the State of California and the United States of America that the foregoing is true and correct.

Justin McCrary

Exhibit A

Justin McCrary

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School of Law
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Berkeley, CA 94720

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Appointments

University of California, Berkeley
2010– Professor of Law
2008–10 Assistant Professor of Law

University of Michigan
2003–07 Assistant Professor, Gerald R. Ford School of Public Policy
2003–07 Assistant Professor, Department of Economics (courtesy)

National Bureau of Economic Research
2006–12 Faculty Research Fellow (Labor Studies, Economics of Education)
2012– Faculty Research Associate (Labor Studies, Economics of Education)

Education

A.B. Public Policy, Princeton University, 1996

Ph.D. Economics, University of California, Berkeley, 2003

Publications

Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime: Comment
American Economic Review, Volume 92, Number 4, September 2002

The Effect of Court-Ordered Hiring Quotas on the Composition and Quality of Police
American Economic Review, Volume 97, Number 1, March 2007

Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test
Journal of Econometrics, Volume 142 , Issue 2, February 2008

Following Germany's Lead: Using International Monetary Linkages to Estimate the Effect of Monetary Policy on the Economy (with Julian di Giovanni and Till von Wachter)
Review of Economics and Statistics, Volume 91, Number 2, May 2009

Incomes in South Africa Since the Fall of Apartheid (with Murray Leibbrandt and James Levinsohn)
Journal of Globalization and Development, Volume 1, Issue 1, January 2010

Dynamic Perspectives on Crime
in *Handbook of the Economics of Crime*, Chapter 4, Edward Elgar, 2010

The Effect of Female Education on Fertility and Infant Health: Evidence from School Entry Laws Using Exact Date of Birth (with Heather Royer)
American Economic Review, Volume 101, Number 1, February 2011

Comment on "Free to Punish? The American Dream and the Harsh Treatment of Criminals", by Rafael di Tella and Juan Dubra
Cato Papers on Public Policy, Volume 1, 2011

Controlling Crime: Strategies and Tradeoffs (with Phil Cook and Jens Ludwig), Chicago: University of Chicago Press, 2011.

General Equilibrium Effects of Prison on Crime: Evidence From International Comparisons (with Sarath Sanga)
Cato Papers on Public Policy, Volume 2, 2012

Do Sexually Violent Predator Laws Violate Double Jeopardy or Substantive Due Process: An Empirical Inquiry (with Tamara Lave)
forthcoming, *Brooklyn Law Review*, May 2013

Working Papers

The Effect of Police on Crime: New Evidence From U.S. Cities, 1960-2010 (with Aaron Chalfin)

Measuring Benchmark Damages in Antitrust Litigation (with Danield L. Rubinfeld)
Revise and resubmit, *Journal of Econometric Methods*

New Evidence on the Finite Sample Properties of Propensity Score Matching and Reweighting Estimators (with Matias Busso and John DiNardo)
Revise and resubmit, *Review of Economics and Statistics*

The Deterrence Effect of Prison: Dynamic Theory and Evidence (with David S. Lee)

Shall We Haggle in Pennies at the Speed of Light or in Nickels in the Dark? How Minimum Price Variation Regulates High Frequency Trading and Dark Liquidity (with Robert Bartlett)

Expert Witness Activities

In the Matter of Act 111 Interest Arbitration Between *Commonwealth of Pennsylvania* and *Pennsylvania State Troopers Association*

Professional Activities

2009– Co-Director, *Law and Economics Program*, University of California, Berkeley

2006– Co-Director (with Phil Cook and Jens Ludwig), *Crime Working Group*, National Bureau of Economic Research

Courses Taught

Berkeley

2012-2013 Law 250: Business Associations (Fall); Law 209.3: Introductory Statistics (Fall)

2011-2012 Law 250: Business Associations (Fall); Law 209.3: Introductory Statistics (Fall); Law 251.31: Introduction to Law, Economics, and Business (Spring); Legal Studies 145: Law and Economics I (undergraduate)

2010-2011 Law 250: Business Associations (Fall); Law 216: Law and Economics Workshop (Fall and Spring); Legal Studies 145: Law and Economics I (undergraduate); Law 209.6: Topic in Quantitative Methods (JSP); Econ 250C: Labor Economics (graduate, shared course with 209.6)

2009–2010 Law 216: Law and Economics Workshop (Fall and Spring); Law 209.32: Quantitative Methods II (JSP)

2008–2009 Law 216: Law and Economics Workshop (Fall and Spring)

2007–2008 Legal Studies 145: Law and Economics I (undergraduate); Law 209.3: Quantitative Methods I (JSP); Law 209.32: Quantitative Methods II (JSP)

2006–2007 Legal Studies 145: Law and Economics I (undergraduate); Law 209.3: Quantitative Methods I (JSP)

Michigan

Introduction to Quantitative Methods (policy), First Econometrics Field Course (economics), Advanced Economic Theory (policy)

Grants and Fellowships

2007–2010 NIH, Constructive Proposals for Dealing With Attrition (with John DiNardo)

2009 Committee on Research, Junior Faculty Research Grant, UC Berkeley

2006–2009 NIH, The Effect of Female Education on Fertility and Infant Health (with Heather Royer, Grant # R03 HD051713)

2006–2011 NSF, New Instrumental Variables Estimates of the Effects of Schooling and Military Service: Empirical Strategies Using Non-Public-Use Data (with Josh Angrist and Stacey Chen)

2005 RWJ Foundation Health and Society Scholars Program, Small Grant Program

2004 Rackham Interdisciplinary Grant, University of Michigan

2004 CLOSUP Grant, University of Michigan

2004 National Poverty Center Grant, University of Michigan

2002–2003 Chancellor's Dissertation Year Fellowship, UC Berkeley

Presentations

Prior to 2007–2008, presentations are at departments of economics, unless otherwise noted

2002–2003 University of California, San Diego; University of California, Los Angeles; RAND Institute; University of Chicago, Graduate School of Business; University of Chicago, Harris School of Public Policy; University of Michigan, Ford School of Public Policy; Columbia University; Dartmouth College; Federal Reserve Bank of New York; Boston University

2003–2004 University of Michigan; APPAM; NBER Labor Studies Meeting (Fall); Massachusetts Institute of Technology; Harvard University, Kennedy School; University of California, Los Angeles; University of California, San Diego; Columbia University; University of California, Berkeley; NBER Summer Institute, Monetary Policy; NBER Summer Institute, Labor Studies

2004–2005 Federal Reserve Bank of Chicago; University of Illinois, Urbana-Champaign; University of Michigan, William Davidson Institute; University of Maryland; Urban Institute; American Economics Association Meetings; City University of New York Health Economics Seminar; University of Wisconsin, Madison; Stanford University; University of California, Davis; University of California, Berkeley, Labor Lunch; NBER Summer Institute, Education/Labor Studies

2005–2006 University of Michigan; University of California, Irvine; University of California, Santa Barbara; University of California, Santa Cruz; California State University, Long Beach; University of Western Ontario; University of Toronto; University of Illinois, Chicago; University of Chicago, Graduate School of Business; APPAM; University of Florida; University of California, Berkeley, School of Law; Princeton University; RAND; Hebrew University (conference in honor of Reuben Gronau); Stanford University, University of Wisconsin, Madison; Northwestern University; Crime and Economics Summer Workshop, University of Maryland

2006–2007 University of Michigan, Program in Survey Methodology; Public Policy Institute of California; Brown University

2007–2008 Northwestern University, School of Law; University of Michigan, Department of Economics; National Bureau of Economic Research, Summer Institute; Florida State University

2008–2009 University of California, Los Angeles, School of Law; University of Arizona, School of Law and Department of Economics; Stanford University, School of Law and Department of Economics; University of Miami, Department of Economics

2009–2010 University of Chicago, School of Law; Cornell University, School of Law and Department of Economics; University of Michigan, School of Law and Department of Economics; University of Virginia, School of Law, Olin Conference

2010–2011 Northwestern, School of Law; University of Wisconsin, Department of Economics; Brookings Institution; Cato Institute

2011–2012 University of Oregon, Department of Economics; University of British Columbia, Department of Economics; University of Rochester, Department of Economics; Cato Institute; National Bureau of Economic Research, Summer Institute; Harvard Law School

Last updated: February 13, 2013

`http://emlab.berkeley.edu/~jmccrary/cv.pdf`

Exhibit B

EXPERT REPORT

Justin McCrary
Professor of Law
Co-Director, Law and Economics Program
University of California, Berkeley

Faculty Research Associate
Co-Director, Crime Working Group
National Bureau of Economic Research

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I. Introduction

A. Qualifications

1. I am Professor of Law at the University of California, Berkeley. I received my A.B. in Public Policy from Princeton University in 1996. After working at National Economics Research Associates in White Plains, New York, and the Federal Reserve Bank of New York from 1996-1998, I began my Ph.D. in economics at UC Berkeley, completing the degree in June 2003 with a specialty in labor economics and econometrics. After close to 5 years at the University of Michigan, I became an Assistant Professor of Law at Berkeley in January 2008 and was promoted to Professor in July 2010. While at Berkeley, I have taught courses on introductory, intermediate, and advanced statistics to J.D. students, L.L.M. students, and Ph.D. students; on law and economics to J.D. students as well as undergraduates; on corporations law to J.D. and L.L.M. students; and on labor economics to Ph.D. students.

2. As noted, I previously worked at the University of Michigan. From 2003 through December 2007, I was Assistant Professor of Public Policy and Assistant Professor of Economics (courtesy), and while at Michigan, I taught courses in introductory statistics and advanced microeconomic theory to M.P.P. students; and in advanced econometric theory to Ph.D. students.

3. Since 2009, I have co-directed the Law and Economics Program at Berkeley Law with Bob Cooter and Dan Rubinfeld (2008-2011) and with Bob Cooter and Eric Talley (2012-present).

4. In addition to my professorial activities, I co-direct the Crime Working Group of the National Bureau of Economic Research together with Phil Cook and Jens Ludwig. Together with my co-directors, I co-edited the book *Controlling Crime: Strategies and Tradeoffs*, published by the University of Chicago Press in 2011. This group's annual meeting is currently the highest quality annual scholarly meeting in the U.S. regarding crime, policing, and crime policy.

5. Over the years, my research has been supported by the University of Michigan, UC Berkeley, the National Institutes of Health, and the National Science Foundation.

My primary areas of research are crime, policing, and crime policy. I have written several peer-reviewed articles, mostly on crime, police departments, or methodology. Many of these articles have been published in the leading journals within economics, such as the *American Economic Review* (*AER*) and the *Journal of Econometrics*. For example, a 2002 *AER* paper of mine pertained to the effect of police on crime, and a 2007 *AER* paper of mine examined in detail the extent to which police department hiring practices changed as a result of pattern or practice suits brought under Title VII of the 1964 Civil Rights Act and Section 1981.

6. I am frequently asked to review articles, particularly those pertaining to crime, policing, and crime policy, for the leading journals within economics and in criminology, including *Econometrica*, the *American Economic Review*, the *Quarterly Journal of Economics*, the *Journal of Political Economy*, the *Review of Economic Studies*, the *Review of Economics and Statistics*, the *American Law and Economics Review*, *Criminology*, and the *Journal of Quantitative Criminology*. Since coming to Berkeley Law, I have also been asked to comment on empirical papers submitted to law reviews and to peer reviewed law journals, including the *Law and Society Review*, the *Journal of Law and Economics*, and the *Journal of Empirical Legal Studies*.

7. Finally, I am frequently asked to give talks regarding the utilization of statistical methodologies in the courtroom and this summer will give for the second time one of the day-long lectures for the week-long *Causal Inference Workshop*, organized by Bernie Black (Northwestern Law) and Matthew McCubbins (USC Law). This summer I will give one of three day-long lectures at the *Advanced Causal Inference Workshop*, also organized by Black and McCubbins. Both the basic and advanced workshops are attended by members of top law faculties from around the country.

8. A copy of my curriculum vitae is included as Exhibit A to my Declaration.

B. Assignment

9. I have been engaged by the City of Stockton's counsel to assess the opinions, conclusions, and analysis set forth in the declarations and expert reports of David Neumark and Joseph Brann, as clarified in their depositions, and to provide my opinions

regarding their reports, declarations, and testimony. This report comprises my opinions, conclusions, and findings in that regard.[1]

10. The materials I have reviewed are included as Appendix B to this Report.

11. I reserve the right to modify my opinions discussed in this report based upon additional information, data, or testimony that may become available at a later date.

II. Summary of Opinions

12. I disagree with Mr. Brann and Professor Neumark's opinion that a modest pension benefit reduction would not lead to a mass exodus of experienced officers, and their opinion that the City of Stockton's evidence on this question is insufficient (Brann Decl., ¶ 4; Neumark Decl., ¶ 5). My opinion on these issues, based on the evidence I have reviewed for this engagement as well as my background knowledge of crime, policing, crime policy, police hiring, and the internal functioning of police departments, as well as my background knowledge of labor economics, is that the City of Stockton ("the City") is at a crisis point in regards to police department staffing. Further downgrades to compensation and benefits, such as the modest pension benefit reductions contemplated by Mr. Brann and Professor Neumark, in my opinion, are likely to push the City over the precipice, leading to a further acceleration of police department departures, including departures of some of the most able officers, and to worsened morale among those officers who remain with the department.

13. My disagreement with Mr. Brann and Professor Neumark in this regard is based on a number of reasons, primarily: (i) the fact that the department has already downgraded compensation and benefits, which has accelerated the normal process of attrition at the Stockton Police Department ("the Department"); (ii) that Mr. Brann and Professor Neumark's alternative explanations for why officers have been departing the City neither account for the timing of the departures nor comport with the findings of contemporary economic literature; (iii) that police officers specifically value their pensions highly, as is obvious by virtue of the fact that nearly every police department nationally offers a pension; (iv) that as a matter of common sense and economic theory, virtually every worker in the economy has a

[1] I am being compensated at the rate of $575 per hour. I have no financial interest in the outcome of this case.

"trigger point," or a sufficiently severe downgrade to compensation and benefits at which he or she will seek employment elsewhere or simply stop working; and (v) that given the downgrades in pay and benefits since 2008, the reductions in police staffing since 2008, and the proposed reduction in retiree medical benefits, a modest pension reduction would likely bring a significant number of Stockton police officers to their "trigger point."

14. I also disagree with Mr. Brann's conclusion that a modest pension benefit reduction would not lead to increased crime (Brann Report, p. 1) to the extent that a pension downgrade would likely lead to a significant number of officer departures, which in turn would compromise the safety of the citizens of Stockton. Contrary to Mr. Brann's position, there is an extensive literature establishing a causal connection between crime and police staffing levels. Additionally, in a paper I co-authored in 2013, my co-author and I found that as of 2010, Stockton was the second most under-policed city among medium-to-large cities in California (Chalfin and McCrary (2013)) given each additional dollar spent on policing would have saved Stockton $2.30 in crime costs. Upon updating our calculations for this report, I concluded that in 2011, Stockton surpassed Oakland, CA and became the single most under-policed city among medium-to-large California cities. This conclusion is based on my empirical estimation that every $1 less spent on police costs would result in over $3 in crime costs for Stockton residents. Thus, any reduction in Stockton police staffing levels, which a modest reduction in pension benefits would likely cause, would result in tremendous costs for the residents of Stockton in terms of their safety. Stockton's public safety concerns in connection to a modest reduction in pension benefits are legitimate.

15. My disagreement with Mr. Brann in this regard is based on my extensive econometric research on the effect of police on crime; on my associated review of the literature in economics and criminology on the effect of police on crime; and on my associated review of the literature on the monetization of safety, including the literature on the value of a statistical life.

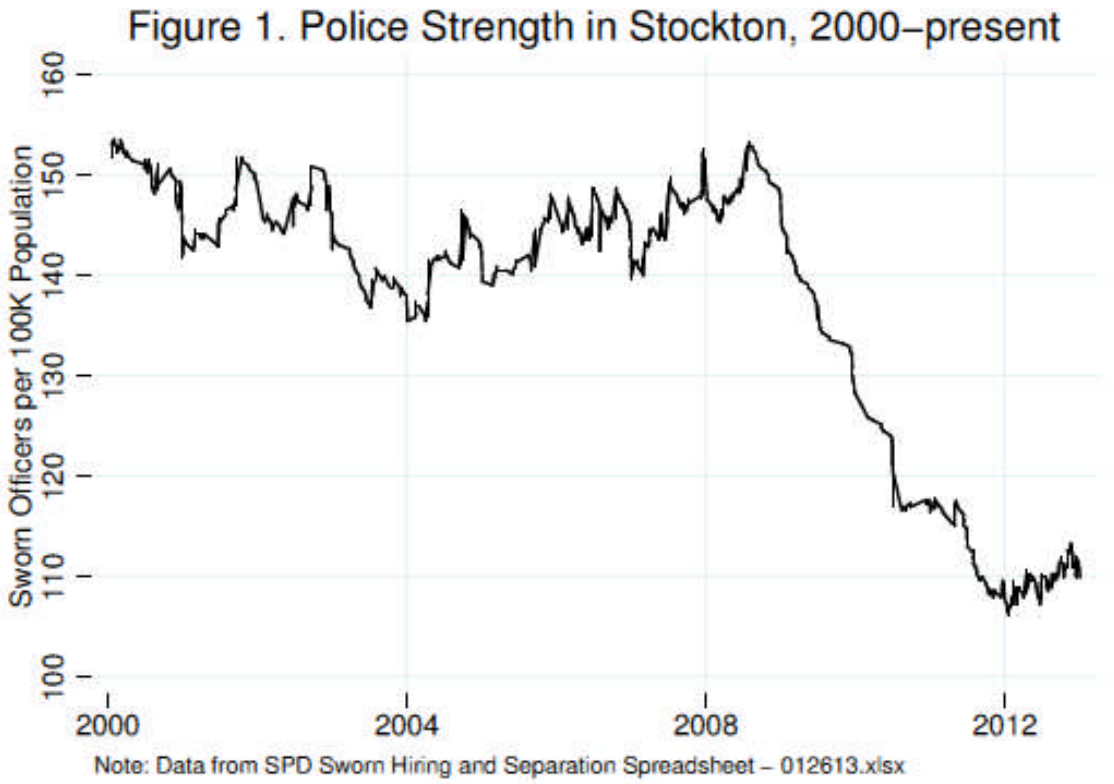
16. In the remainder of this report, I more fully explain my analysis and the conclusions that I reach.

III. Stockton's Crisis in Police Staffing and Significant Downgrades in Compensation and Benefits

17. The first and one of the most important facts to be discussed is that the Department is hemorrhaging officers, and the timing of these officer departures has coincided with significant downgrades in officer compensation and benefits. Mr. Brann and Professor Neumark attempt to discount the reality that Stockton is having recruitment and retention problems by pointing to the number of recent hires and applicants to the Department (Brann Report, pp. 17-18; Neumark Report, p. 19-20). Their rosy interpretation of the Department's staffing situation is misleading. Although a recent transfusion of new hires has somewhat stabilized the overall staffing number—which is still lower than the number the Department has budgeted for (Jones Decl., ¶ 6)—the future is highly uncertain due to the looming possibility of additional benefit reductions and the possibility of further departures of incumbent officers. Moreover, even Professor Neumark acknowledges these departures have "occurred at the same time as substantial pay and benefit cuts" (Neumark Report, p. 7). Police Chief Eric Jones shed more light on the nature of these cuts in his deposition when he characterized them as being up to "30 percent . . . of total compensation" for some officers (Jones Dep., p. 118:9-11).

18. Figure 1 shows the number of sworn officers in the Department from 2000 to the present, relative to Stockton population, the same period analyzed by Professor Neumark in his Figure 2.[2] As Figure 1 demonstrates, Stockton went through a multi-year period beginning in 2008 where it lost more officers than it gained. This drop coincided with the financial and foreclosure crisis.

[2] Here I rely on administrative data on police staffing from the City and public data on population from the U.S. Census Bureau for 2000 and 2010 and the California Department of Finance for 2011. I linearly interpolated population data for 2001-2009. Police per 100 thousand persons is a standard metric for police staffing in the economics literature.



19. Mr. Brann attempts to minimize the departures from the Department, when he says, "The City's lateral transfer rate is also consistent with data that has been gathered in the past on such transfers in California and elsewhere and is not indicative of a 'mass exodus'" (Brann Decl., ¶ 6). In support of this statement, he cites to data contained in a statewide California study on lateral transfer rates conducted in 2006 with data ending in 2005, and other data that he stated in his deposition he would no longer like to rely upon (Brann Report, p. 16; Brann Dep., pp. 228-231). Putting aside the fact that Stockton's 8.1% lateral transfer rate in 2012 exceeds the maximum 4.7% rate in the California study he cites, and that Table 36 of his own source document, *Recruitment and Retention: Best Practices Update*, indicates a state-wide lateral transfer rate of 3 to 4 percent, comparing outdated state-wide data to Stockton's present data is not informative. A sounder methodology would compare police force staffing in Stockton with contemporaneous police force staffing in comparable cities, which I have done here and describe below.

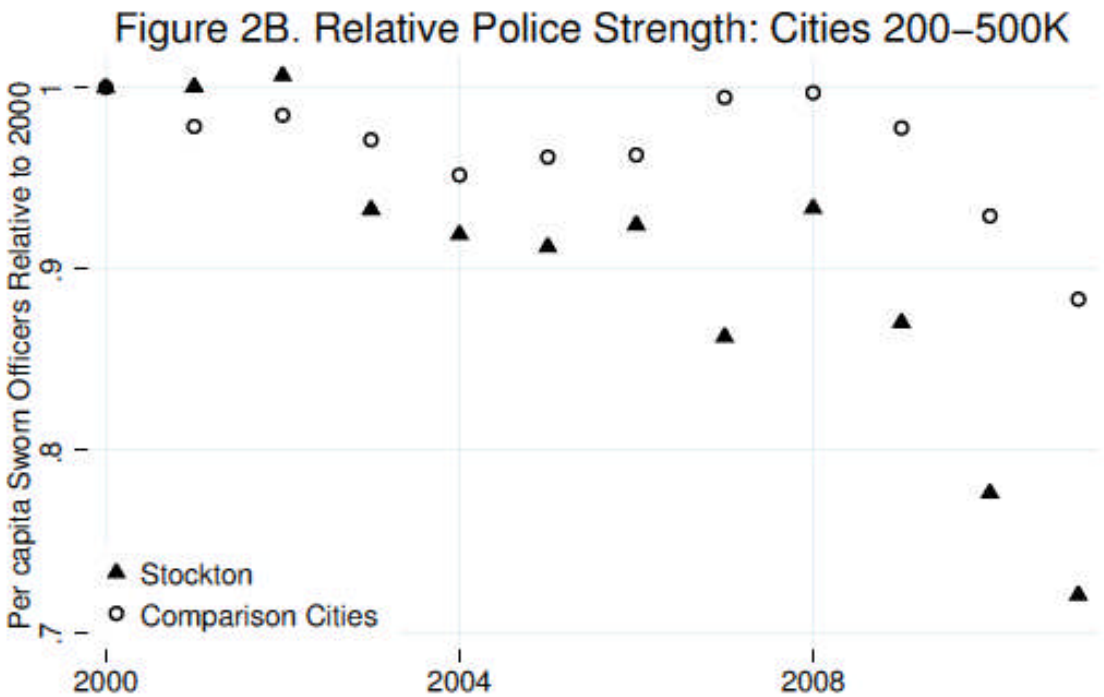
20. While other California cities have also seen staffing declines subsequent to the financial crisis, the decline in police staffing in Stockton has been greater than that experienced by other California cities. Administrative data on police staffing are not available for other jurisdictions, but I was able to analyze annual data on police staffing from the Uniform Crime Reports (UCR) system for all California jurisdictions over the period 2000-2011.[3] A summary of these data is presented in Figures 2A and 2B. These figures show the number of police per capita, relative to the 2000 level for Stockton and for a population-weighted average of comparison cities. Panels A and B differ in the set of comparison cities they plot. Panel A uses a set of cities suggested by Mr. Brann as being particularly comparable (Brann Dep., pp. 240:6-22), while Panel B uses cities of between 200,000 and 500,000 population.[4]

Figure 2A. Relative Police Strength: Brann Cities

Note: Data are from the Uniform Crime Reports. Comparison cities are population–weighted average of Sacramento, Bakersfield, Modesto, and Santa Ana.

[3] The police data for 2012 have not yet been released by the UCR system.

[4] Mr. Brann suggested in his deposition that population was one of the more important factors upon which comparable cities for a given city should be selected (Brann Dep., pp. 240:19-241:3). Stockton presently has about 300,000 population, and the comparison cities suggested by Mr. Brann range from just above 200,000 (Modesto) to just below 500,000 (Sacramento) (Brann Dep., p. 241:19-242:8). Census Bureau estimates are taken from www.google.com/publicdata.

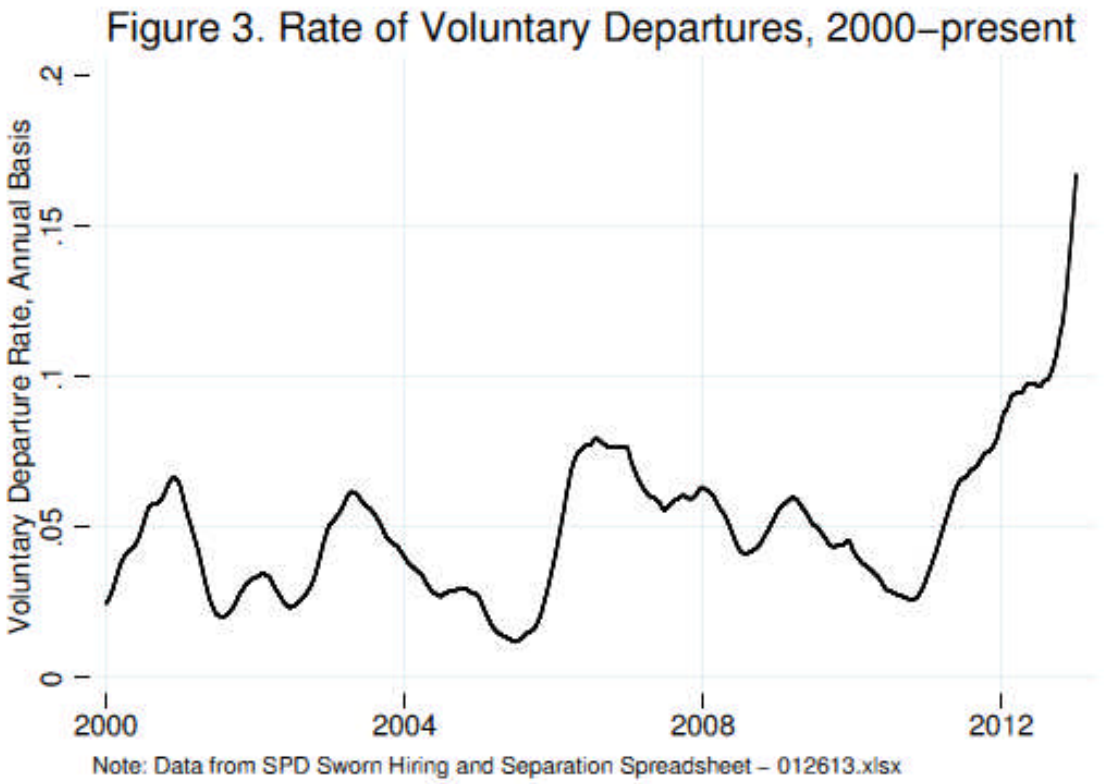


Note: Comparison cities are population–weighted average of Fresno, Sacramento, Long Beach, Oakland, Bakersfield, Anaheim, Santa Ana, Riverside, Chula Vista, Fremont, Irvine, San Bernadino, Modesto, and Oxnard.

21. These figures show clearly that police per capita in Stockton in 2011 were about 72 percent as high as they were in 2000. The comparison cities, also hit hard by the financial crisis, have about 87 or 88 percent as much staffing as they did in 2000. To summarize, Stockton has seen a much stronger decline in police staffing since 2008 than have other comparable cities. As I will discuss below, these staffing shortfalls are a likely cause of the recent crime wave in Stockton, a point Mr. Brann quibbles with when he states "it is a fallacy to attempt to establish a causal relationship between crime and police staffing levels" (Brann Report, p. 7).

22. Figure 3 presents the "annualized" rate of voluntary departures from the Department for 2000 to the present.[5] This figure shows how unsustainably high voluntary departures figures in Stockton have become, and how Professor Neumark and Mr. Brann are wrong to discount the Department's retention problems. In the wake of the financial crisis, the rate of voluntary departures from the Department initially held steady at historic norms and ranged between 3 and 5 percent for 2008-2010. Presumably, during this period few individuals were able to obtain work from other police employers. The first few years after the financial crisis were a highly uncertain time and few workers were willing to relocate at that time. With the slowly improving economy, however, options opened up for workers, and that appears to include employees of the Department. In 2011 and 2012, the voluntary departure rate has surged, from the historical norm of 3-5 percent to 10, 15, and even 17 percent. Quarter by quarter, the voluntary departure rates for 2011 were 4, 6, 7, and 8 percent, while those for 2012 were 9, 10, 10, and 13 percent. My estimate of the annualized voluntary departure rate as of December 31, 2012 is 17 percent. Departure rates of 10 to 17 percent are unsustainably high and would require that the department recruit at a commensurate rate, just to maintain current staffing levels. To do so would require Stockton to lower its standards for police officers and lead to liability risk for Stockton, an issue I now discuss.

[5] The calculations presented in this figure are standard calculations done in the economics literature. The technical details are as follows: the figure presents $1-(1-e)^{365}$, where e is an estimate of the daily voluntary departure rate. To see where this formula comes from, suppose that 2 percent of officers left today, where today there were N_0 officers. Then tomorrow there would be $N_1=0.98N_0$ officers. If another 2 percent of officers left tomorrow, then two days hence there would be $N_2=0.98N_1=0.98^2N_0$ officers. If the pattern continued for the remainder of the year, then at year's end there would be $N_{365}=0.98^{365}N_0$ officers, and the percent departures would be $(N_0-N_{365})/N_0=1-0.98^{365}=1-(1-0.02)^{365}$. The estimate of the daily voluntary departure rate was obtained by smoothing the daily voluntary departure rate data using local linear regression with a bandwidth of 180 days and the triangle kernel (Fan and Gijbels 1996). The daily departure rate data was calculated by merging the administrative data on police staffing from Stockton with a list of all possible dates from 2000 to January 2, 2013, the most recent hiring event covered by the data provided by Stockton. Dates with no departures have a zero departure rate; the departure rate for other dates is the number of departures relative to department strength.



23. In my opinion, if this pace continues, then (a) recent hiring will be insufficient to prevent further declines in police staffing in Stockton, (b) additional emergency hiring may be necessary to prevent the size of the Department from falling to 50 percent of its 2007 level, (c) the quality of any emergency hires will be lower than is customary for the Department and for comparable jurisdictions, and, finally, that (d) the lower quality of the emergency hires will present serious problems to the Department in terms of the training and productivity of those officers and will pose a liability risk for Stockton. Mr. Brann and Professor Neumark fail to appreciate these likelihoods in their reports when they argue that recent hires and application numbers mean Stockton's staffing situation is no source of concern (Brann Report, pp. 17-18; Neumark Report, p. 19-20).

24. Mr. Brann, who unlike Professor Neumark is familiar with the details of police department recruitment and retention, seems to appreciate the difficulties associated with en masse hiring, something that will likely happen if Stockton reduces pensions or takes other steps that provoke the departure of more officers. Mr. Brann notes that he does not

have direct information on the quality of entry level and lateral candidates applying to work for the Department (Brann Dep., pp. 114:7-10, 178:22-179:1). He concedes that in the first few years, officers are not as productive as they will be later on (Brann Dep., pp. 145:12-146:3, 151:11-22, 151:17-24). Mr. Brann notes in particular that a department needs a range of experience levels (Brann Dep., p. 152:11-18).

25. To appreciate the short-term importance of Stockton needing to avoid emergency hiring, consider the experience of Washington, D.C., in the late 1980s and early 1990s. Washington, D.C. confronted a serious problem with a retirement bulge among police officers. Facing the rise in the distribution of crack cocaine and ensuing municipal chaos, Congress took emergency action. Describing the situation, Harriston and Flaherty (1994a) note that the:

> hiring spree [in police] 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.

26. To hire that many officers, the Washington, D.C. police department was forced to lower standards. It simply could not persuade enough qualified people to apply, despite best efforts at advertising. Harriston and Flaherty (1994a) go on to report that:

> graduates in those two years alone, who make up about one-third of the force, account for: (i) More than half of the 201 D.C. police officers have been arrested since 1989 on charges ranging from shoplifting and forgery to rape and murder. Some have been arrested more than once and in more than one year. (ii) More than half of those involved in departmental disciplinary proceedings for breaches such as neglecting duty, making false statements and failing to obey orders, which have doubled since 1989. (iii) Half of those on a list of 185 D.C. officers were so tainted by their own criminal problems that prosecutors won't put them on a witness stand as officers of the law.

27. Harriston and Flaherty later documented the number of criminal defendants who were unable to be successfully processed due to errors on the part of police officers from the 1989 and 1990 hiring cohorts (Harriston and Flaherty 1994b). Summarizing

their findings from a cost perspective, these journalists noted, "[c]onsidering the $22,000 it costs to train an officer at the academy and the roughly $27,000 salary paid a first-year officer, the District has spent at least $3 million because of bad hiring: half to pay and to train officers who later were fired and half for those officers' replacements." In other words, taking the long view, and particularly because Stockton can be liable for actions taken by officers, lowering standards has the potential to create long-term problems with morale, shoddy policework, and extra expense for the jurisdiction. For a discussion of the importance of morale in police departments, see Cruickshank (2013).

28. To appreciate the long-term ramifications of large scale emergency hiring, observe that once departments find themselves in a situation where they need to replace workers en masse, such a department will face a wave of retirements in 20 to 25 years when those officers retire. En masse hiring can lead to a cycle of retirement bubbles that result in having to replace a large crop of police officers, a cycle that could possibly repeat itself over many generations. This retirement bubble phenomenon was experienced in Boston as a consequence of the Boston Police Strike of 1919. Then-governor Calvin Coolidge replaced all striking workers (about two-thirds of the department). The enormous burst of hiring that resulted led to a 20 to 25 year cycle in police hiring in Boston. It was many decades before Boston's years-of-service distribution shed its lumpy pattern (Boston Police Department 1940, Boston Police Department 1955). In an organization of generally stable size where 20-25 years of service are typical, it is ideal to hire 4-5 percent of the workforce each year. This eventually creates a rhythm where 4-5 percent of the workforce retires each year, and is replaced in turn by an annual inflow of rookies. Failing to hire in such a steady fashion leads to lower quality officers overall, as it eventually requires "catching up."

29. In sum, both the Washington D.C. and Boston experiences described above could indeed become Stockton's experience if it is unsuccessful in remedying its staffing crisis. To this end, at a time when Stockton should have serious concerns about en masse hiring initiatives and about staving-off a hiring bubble, Stockton should not be taking steps that increase the likelihood that yet more officers will leave the Department, such as instituting

modest pension reductions—a measure Mr. Brann and Professor Neumark improperly discount as something that would not significantly affect staffing.

IV. Alternative Explanations Put Forward by Neumark and Brann for Departures Fail to Explain Timing of Departures

30. Professor Neumark and Mr. Brann both refer to the idea that the relocation of officers from Stockton to other locations, such as Oceanside, is over-determined. That is, Neumark and Brann claim that officers could either be relocating because of economic factors, or they could be pursuing proximity to the beach. Their speculative statements do not reasonably account for recent departures because they do not consider the timing of the recent departures. As shown in Figure 3, above, voluntary departures are accelerating among Stockton police officers. However, the amenities ("lifestyle" factors) Professor Neumark and Mr. Brann discuss have not changed in several decades. Consequently, those relative amenities are good explanations for why Stockton would have found it necessary over the years to offer a particularly competitive compensation and benefits package—in order to persuade individuals to work in Stockton rather than Oceanside—but it is hardly an explanation for increases in voluntary departures that have taken place over the last 2-3 years.

V. Lack of Decisive Evidence "Rising to Social Science Standards" Regarding a Causal Effect Does Not Imply No Causal Effect Exists

31. Neither Professor Neumark nor Mr. Brann have presented any evidence that there will *not* be a decline in police staffing as a result of a modest reduction in pension benefits. Professor Neumark argues primarily that the City has not presented any evidence that rises to his standards. "No convincing evidence has been presented by the City that any cut in pension benefits—or even a modest cut—would lead to a 'mass exodus' of experienced police officers" (Neumark Decl., ¶ 6). However, Professor Neumark presents no evidence of his own that an exodus will *not* occur if there is a reduction in pension benefits.

32. The absence of decisive evidence regarding a particular causal effect does not indicate that the causal effect in question does not exist. No economic theory would justify the conclusion that declines in police staffing will *not* result, simply because there are no

"findings that stand up to social science standards of evidence" (Neumark Report, p. 6). Yet Professor Neumark insists that "one cannot conclude that the City would face a mass exodus of experienced… police officer[s]…, or face problems associated with recruitment and retention of… police officers… as a result of any, or modest, cuts in pensions" (Neumark Decl., ¶ 5). While he chides the City for not having any decisive evidence that declines in police staffing will result from cuts to pensions, Professor Neumark's conclusions are not based on any evidence at all, except to the extent that he (mis)interprets some limited information from the City, as I discuss below. He acknowledges that he has never studied, and that the economics literature does not contain estimates of, the effect of reduced pensions on retention of public workers (Neumark Dep, pp. 43:8-13, 53:5-14). He even acknowledges that it may be difficult to find data sets that would allow for a study rising to what he terms "social science standards."

33. Consequently, Professor Neumark's position can only be that the lack of decisive evidence of a causal effect implies that no causal effect exists. This is a claim that is unsupported by economic theory and defies common sense. A city applying the fallacious principle that it should assume workers would not leave based on downgrades to compensation and benefits until there was decisive evidence of such an effect would be doomed to downgrade compensation and benefits until the best workers in fact left, at which point the city would have to try its best to hire back the lost workers. Some things we are confident are true without much direct evidence at all, because we have sufficient indirect evidence. No previously faithful husband would assume that his wife would *not* contemplate a separation and possibly a divorce, were he to stray.

34. At root, Professor Neumark's position on these issues is most similar to a spoof article in the *British Medical Journal* in 2003, surveying the (fictitious) literature on the effects of parachute usage on those jumping out of airplanes. "The effectiveness of an intervention has to be judged relative to non-intervention. Understanding the natural history of free fall is therefore imperative…. studies are required to calculate the balance of risks and benefits of parachute use" (Smith and Pell (2003), p. 1460). Stockton should not have to become the petri dish in a social science experiment regarding pension cuts and employee

retention, simply because we do not have decisive evidence that opening the parachute while in free fall is a good idea.

35. In my professional opinion as a labor economist, Professor Neumark's own deposition testimony indicates that declines in police staffing are likely. Professor Neumark acknowledges that workers value compensation (Neumark Dep., pp. 107:19, 203:12). He acknowledges that compensation and benefits are an important factor in deciding whether to migrate somewhere else to pursue work (Neumark Dep., pp. 76:18-77:4, 188:12-17, 225:13-24). He acknowledges that reduced pensions specifically will lead to worker dissatisfaction (Neumark Dep., p. 176:9). Indeed, Professor Neumark concedes that a very large pension cut of 90 percent or the complete elimination of the pension *would* lead to a mass exodus (Neumark Dep., pp. 27:6-19, 151:12-18). He further concedes that he can't say with "any degree of certainty" that more than a dozen departures (about 4 percent of officers) would not occur and that it is "not inconceivable [that] twelve [officers] would depart" as a result of a modest reduction in pension benefits (Neumark Dep., pp. 156:14-25). Professor Neumark and I agree that "in social science research… one can generalize" from one set of findings to reach related conclusions (Neumark Dep., p. 94:24-25). Based on my background as a labor economist and Professor Neumark's own deposition statements, without even considering additional information, I would generalize that further police departures in Stockton are, in fact, likely to result from further reductions to compensation or benefits. This conclusion is only enhanced when I consider facts specific to Stockton, such as those outlined in Section III, above.

36. A fundamental precept of economics is that if, holding other factors constant, the effect of increasing one variable by 10 percent is to reduce another variable by 5 percent, then a highly accurate approximation for what would happen, all else held constant, were the one variable were increased by 5 percent, would be a decline in the other variable by 2.5 percent. The ratio of 5 to 10 and 2.5 to 5 is known in economics as an *elasticity* and is used to make predictions regarding hypothetical scenarios such as those contemplated in this case. This is a fundamental form of generalization used in economics going back to a leading

nineteenth century economist, Alfred Marshall, if not before.[6] Professor Neumark's concession that a 10 percent decrease in pensions might lead to a 4 percent increase in departures (12 officers), cited above, and perhaps more, indicates that he is not asserting that there is no relationship, but rather is playing semantics with words like "modest" and "mass." Since Professor Neumark has not presented any direct evidence of his own, he cannot reasonably have any basis for saying that a 4 percent departure rate is conceivable but that an 8 percent departure rate is inconceivable. Stated in terms of elasticities, Professor Neumark testified that an elasticity of 0.4 is conceivable, and I further conclude that he cannot have a basis for believing that an elasticity of 0.8 is inconceivable. Earlier in his testimony, Professor Neumark's stated that "modest reductions" in officer pensions could mean as much as a 17 or 18 percent reduction (Neumark Dep., pp. 150:8-15). His testimony that the elasticity is 0.4 would then imply that modest pension reductions would lead to a reduction of 22 officers. If the elasticity were instead 0.8 then Professor Neumark would expect a reduction of 44 officers.[7] However, earlier in his testimony, as well as in his report, Professor Neumark stated that a departure of 20 to 40 officers is a reasonable definition of a "mass exodus" of officers (Neumark Dep., pp. 148:4-12; Neumark Report, p. 6). Consequently, I conclude that Professor Neumark's testimony is internally inconsistent. His testimony at pages 148 and 150 of his deposition, together with core principles of economics, implies that Professor Neumark *does* believe that modest reductions in pension benefits would lead to a mass exodus of officers. His testimony thus directly contradicts his declaration.

37. Like Professor Neumark, Mr. Brann fails to present any evidence that declines in police staffing will not result from further reductions to compensation and benefits. Mr. Brann acknowledges that compensation is "important to every employee" and that compensation and benefits are a significant part of what people consider (Brann Dep., p. 98:11-24). Mr. Brann asserts, however, based on his experience, that a modest reduction is not "likely to have a significant effect" (Brann Dep., pp. 88:22-89:1). In the next breath, however, Mr.

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[6] The formal argument in economics is that a point-elasticity may be well approximated by an arc-elasticity over modest changes and that two nearby arc-elasticities should be similar in magnitude.

[7] That is, 0.4 times 0.17 times 325 officers is 22.1 officers and 0.8 times 0.17 times 325 officers is 44.2 officers. I use 325 officers since this is representative of the number of officers on the Stockton Police Department in December 2012 and January 2013.

Brann concedes that more than modest reductions would have large effects (Brann Dep., p. 85:18-25). I cannot understand these fine distinctions in predictions, unless Mr. Brann, like Professor Neumark, is playing semantics or using unusual definitions for words like "modest" and "significant." Mr. Brann further agrees that it is reasonable for Police Chief Jones to have "serious concerns regarding the effect any further reduction in benefits" might have on retention, and that Chief Jones would be "foolish" not to have such concerns (Brann Dep., pp. 133:8-134:9).

VI. The Fact That Many Factors Affect Migration Decisions Does Not Imply Compensation and Benefits are Unimportant Factors

38. In their declarations and reports, both Professor Neumark and Mr. Brann argue that migration decisions are not affected by economic factors because they are affected by other factors, or that there is no evidence that ongoing departures are related to economic factors. For example, Mr. Brann argues that "the variables [involved in a decision to migrate] are so complex, so interwoven and their meaning to each employee so unique, that any action is the result of and interplay among a variety of influences" (Brann Report, p. 13). In his deposition, Mr. Brann states, "My point in this is there is no cause-effect relationship there" (Brann Dep., p. 62:19-20). Similarly, Professor Neumark argues that "one cannot conclude that past police force departures are solely due to compensation changes…. Economic studies on economic migration indicate that factors such as compensation, both current and future, are not alone decisive in migration decisions" (Neumark Decl., ¶ 7).

39. Despite Professor Neumark's and Mr. Brann's attempts at muddying the waters in this regard, the fact that migration decisions reflect a variety of factors—for example, local amenities such as proximity to mountains, beaches, or family and friends—has no bearing on the question of whether, other things held equal, economic factors are important considerations in a person's decision of where to work. In deposition, Mr. Brann tacitly accepts this point when, after emphasizing the complexity of the migration decision, including the role of "intangible" factors, he agrees that nonetheless "[p]ay and benefits is a significant part of what people consider" (Brann Dep., p. 98:22-24).

40. Professor Neumark cites to economic literature to justify his assertion that economic factors play little to no role in the migration decision. Much of the literature that he cites to, as he acknowledges, is dated. In addition, based on my own review of this literature, I believe Professor Neumark mischaracterizes Greenwood's (1997) review of the literature.[8] Based on my own review of this literature, I also conclude that much of the migration literature is methodologically flawed. In my opinion, the key shortcomings of the literature include: (a) at any point in time a potential migrant has an enormous number of potential migration destinations, (b) the economist does not observe the terms of any job offers workers receive in alternate locations, (c) the economist does not observe the *expected* terms of any job offers workers might receive, were they to relocate to a potential migration destination, and (d) the decision to migrate is fundamentally a dynamic one, as evidenced by the prevalence of reverse and repeat migrants.

41. One of the best papers in the migration literature is Kennan and Walker (2011), which was published in the leading journal within economics, *Econometrica*, and draws upon the most recent and accurate techniques within economics to inform its conclusions. They write "[o]ur main substantive conclusion is that interstate migration decisions are indeed influenced to a substantial extent by income prospects" (p. 213). I note that these authors have solved some of the methodological challenges outlined above, but not all. In general, the methodological problems outlined above suggest that the validity of the empirical claims in the migration literature may not be sufficient to overcome an initial presumption, based on economic theory, common sense, or both, that individuals migrate based on economic factors, particularly when those economic factors are changing decisively.

[8] A theme of Greenwood (1997) is the conclusion that the empirical literature has been unsuccessful in uncovering the true nature of migration as highly responsive to economic factors. This is the perspective stressed by the father of the migration literature, Ernst Ravenstein (see, for example, Ravenstein 1889). The article is rife with discussion of "perverse signs" and "expected signs" of effects. Referring to his own work, Greenwood states approvingly "[t]he results developed by Greenwood and Hunt (1989) indicate that disequilibrium forces, specifically relative wages and employment opportunities, are important determinants of migration" (Greenwood 1997, p. 680).

VII. Professor Neumark Misinterprets the Evidence Regarding New Recruits

42. Professor Neumark discounts the possibility that downgrades to compensation and benefits would present a problem for the Department, inasmuch as it is possible to replace departing officers with new officers. He places great stock in the fact that Stockton has finally begun replacing departing officers with new officers, reasoning that if you can replace some workers, then maybe you can replace all of them. This is true as far as it goes, but ignores the essential issue of officer quality and experience. Hiring additional officers is nearly always possible by lowering standards, but this is not cost-effective for a jurisdiction in the long-term, as I have already explained. I note that Professor Neumark's position on these issues is informed by his expertise in labor markets generally, rather than any expertise regarding police departments specifically, and that he may fail to appreciate the scope of Stockton's liability for acts of police officers.

43. Finally, even if one could ignore the decline in the number of officers and the need to replace departing officers with experienced lateral or even new officers, downgrades to compensation and benefits obviously lead to morale problems. Department-wide and individual officer morale problems may have their own implications for the safety of Stockton's citizens (Cruickshank 2013).

44. In regards to Professor Neumark's claim that the City will be able to recruit because it has been able to hire, based on his lack of experience in the field of police work, Professor Neumark seems to not appreciate the difficulty of police hiring. A traditional estimate of the fraction of *minimally* qualified officers out of an applicant pool is less than 5 percent. The key issue is the ability to select the best possible officer from among that 5 percent. The corollary is that although there may be a pool of 5 percent minimally qualified candidates, not all of them will likely meet the desired qualifications and some may decide to accept jobs elsewhere. Even an apparently large applicant pool such as 1,000 officers is likely to yield only 50 minimally qualified candidates. When hiring needs are dire, it may become necessary to take on officers normally viewed as below the bar, and it may be that it is not possible to hire as many officers as would be needed to replace existing officers. Mr. Brann states that in California only about 3-4 percent of applicants are hired (Brann Dep., p. 180:4-22).

Recent reports in the popular press regarding police department recruitment indicate 1-2 percent may be an emerging norm (von Quednow 2012).

VIII. Both Young and Old Officers Value Their Pensions

45. Professor Neumark argues that younger workers will not mind pension cuts because they are far removed from being eligible to retire. This ignores a central precept of labor economics, that employers offer a mixture of pay and benefits that are especially valued by the kinds of workers they seek to employ (Ehrenberg and Smith (2006)). The process of self-selection in the labor market implies that the pensions that are uniformly offered by police departments must be valued by workers, both young and old, who are interested in police work. Consequently, I infer that young officers care quite a deal about their pensions, and I note this is consistent with anecdotal accounts.

IX. Professor Neumark and Mr. Brann Fail to Consider Interaction Effects, That Is, The Combined Effects of Multiple Downgrades to Compensation and Benefits

46. Neither Professor Neumark's nor Mr. Brann's reports consider the interaction effect of multiple downgrades to compensation and benefits. In fact, they concede that their opinions were based on considering pension reductions in isolation from other actual and proposed reductions in staffing and benefits that are part of the experience of current Stockton police officers. Professor Neumark emphasizes the idea that economic factors are not decisive in the migration decision, because individuals care about friend networks and local characteristics, for example. I agree with Professor Neumark that many people living in any given location in the U.S. will have an attachment to the location. That attachment may well include some of the non-economic factors that Professor Neumark mentions in his report. However, as a matter of economic theory and common sense, I conclude that every employee has a "trigger point," that is, a point at which downgrades to compensation and benefits will result in a decision to relocate as soon as possible.

47. Professor Neumark argues that because some officers have not yet left, they would not leave in response to further cuts (Neumark Report, p. 18). He argues that this follows from thinking of people as "heterogeneous," i.e., differently situated. I disagree that

thinking of people as differently situated implies that their continuous employment even following downgrading necessarily means that they would not depart for another job after further downgrading. If an officer has not departed despite downgrading, it is reasonable to infer either that (a) the economic downgrade was not sufficient to offset factors that tie the officer to the area and/or to the job, or that (b) the officer has not yet had enough time to effectuate an alternative plan. However, it does not at all follow that further downgrades would not affect departures. To demonstrate the flaw in Professor Neumark's logic, note that it implies officers would remain even if pension benefits were completely eliminated—a plainly erroneous conclusion. Not only do I disagree with Professor Neumark's claim in this regard, I reach the opposite conclusion. That is, for officers who have remained with the Department despite past downgrades, any further subsequent downgrades would increase the likelihood of their departures because the overall economic downgrade is closer to offsetting factors that tie the officer to the area and/or job. Economic theory and common sense are on the same page in this matter.

48. Mr. Brann similarly concedes in deposition that he fails to consider interaction effects (Brann Dep., pp. 104:23-105:3).

49. In summary, and contrary to the positions of Professor Neumark and Mr. Brann, in light of the ongoing departmental staffing crisis and the downgrades in compensation and benefits that have already occurred, I find it quite likely that further downgrades to compensation and benefits in the form of a modest pension reduction would push a significant number of officers to their trigger points, leading to a further acceleration of police department departures, including departures of some of the most able officers, and to worsened morale among those officers who remain with the Department. As discussed below, these departures would lead to increased crime in the City. Also as discussed below, it would be cost-effective (weighing the cost of crime) for Stockton to avoid taking measures such as reducing pensions that would exacerbate its retention and recruitment problem.

X. <u>Reductions in Police Generally Imply a Higher Risk of Crime, Particularly Violent Crime</u>

50. I take strong issue with Mr. Brann's statement that "it is a fallacy to attempt to establish a causal relationship between crime and police staffing levels" (Brann

Report, p. 7). An extensive, multi-decade body of research in both criminology and economics has examined the effect of police on crime, and has established that there *is* in fact a causal relationship between crime and police staffing levels. The more recent articles in this literature have been successful at producing estimates that are credible to a wide array of researchers within both economics and criminology. This recent literature points to a powerful effect of police numbers on crime. I am both well-versed in this literature as well as a contributor to it.[9] Mr. Brann appears unfamiliar with this literature.

51. Excellent reviews of this vast literature are provided in Cameron (1988), Nagin (1998), Eck and Maguire (2000), Skogan and Frydl (2004), Levitt and Miles (2006), or Lim, Lee and Cuvelier (2010). This literature has papers of varying quality. Early papers in this literature, in particular, are not methodologically sophisticated and reach incorrect conclusions regarding the effectiveness of police in fighting crime. Mr. Brann notes in his deposition (88:12-24), the ideal method for assessing the effect of one variable on another is an experiment. Mr. Brann is apparently not familiar with the best and most recent research on the effect of police on crime, which focus on "natural experiments," or situations arising naturally that happen to generate a control group and a treatment group that are *ex ante* comparable, akin to the groups emerging from a true randomized experiment. Papers in that category include, for example, Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005), Evans and Owens (2007), Draca, Machin, and Witt (2011), and Machin and Marie (2011), all of which have been published in leading journals within economics, indicating their fundamentally sound methodology. Relatedly, in my own recent work on this subject, we contend that our estimates are credible precisely because they relate differences in police force growth rates to differences in subsequent crime growth rates, and we document that the police force growth rates are unrelated to the usual suspects of confounders, including demographic factors, the local economy, city budgets, and social disorganization (see Chalfin and McCrary (2013) for a detailed discussion).

52. In summary, there is excellent literature documenting the effect of police on crime. The literature takes as its starting point that a credible study is one that

[9] See, for example, McCrary (2002), McCrary (2007), or Chalfin and McCrary (2013).

approximates a randomized experiment. The literature also concludes that increases in the number of police officers reduce crime and that decreases in the number of police officers increase crime.

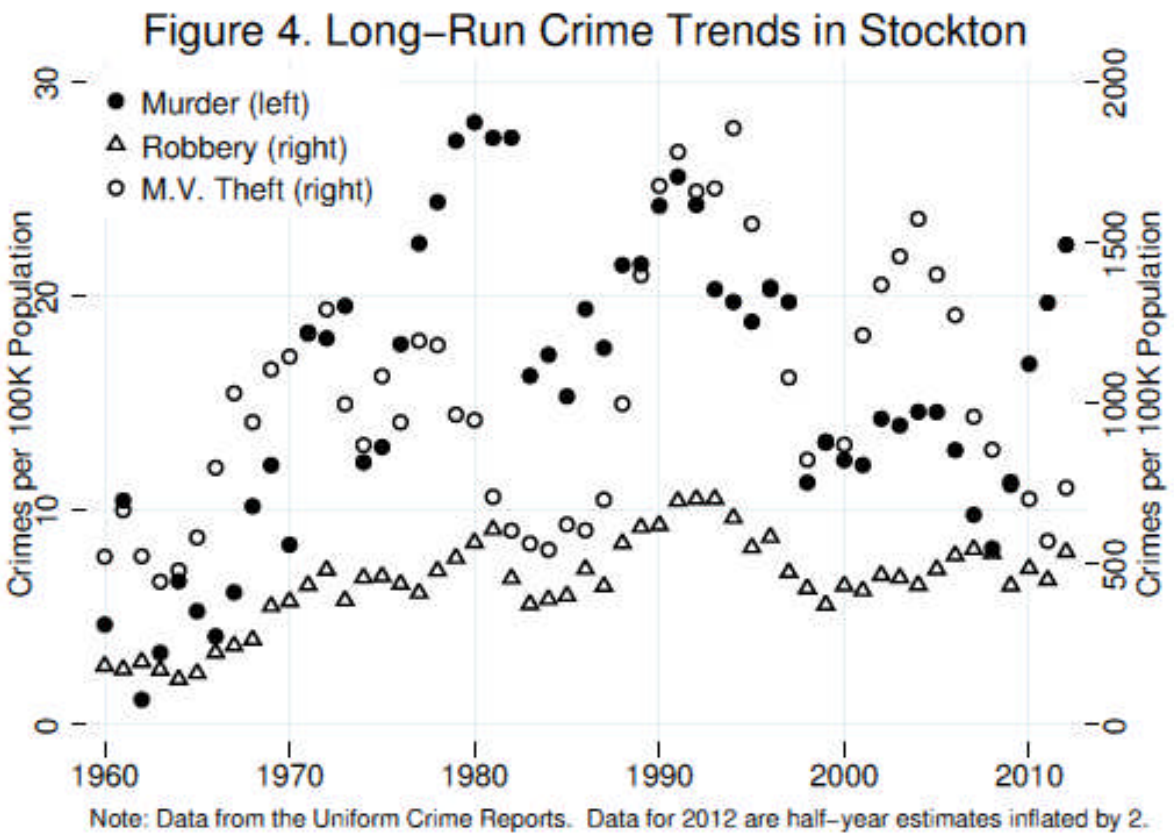
53. A specific finding of interest from that literature is that police have a powerful effect on violent crimes, perhaps even more so than on property crime, with surprisingly strong effects on murder specifically (e.g., see review in Levitt and Miles (2006)). As discussed in Chalfin and McCrary (2013), this result is surprising if police affect crime purely through a deterrence channel, but is entirely plausible if police affect crime not just through a deterrence channel but also through an incapacitation channel. Many people are initially unfamiliar with the incapacitation effect of police on crime, but recognize it upon definition. The incapacitation effect of police is that when police layoffs or departures occur, fewer arrests will occur as a result, and there will be crimes that will occur as a result of the failure of the criminal justice system to incapacitate the offender (McCrary 2010). Since police focus a great deal of energy on apprehending individuals who are in particularly heightened periods of criminal activity, it may well be that police are able to apprehend offenders in chaotic episodes during which they are at risk of spinning out of control and committing murder (Chalfin and McCrary (2013)). In sum, contrary to Mr. Brann's position, it is my position that it is decidedly not "a fallacy to attempt to establish a causal relationship between crime and police staffing levels" (Brann Report, p. 7).

XI. <u>Crime Rates in Stockton Are Dangerously High and Increasing</u>

54. Stockton has long struggled with high levels of crime. The most accurate information on crime for local jurisdictions in the U.S. is the UCR system. The three crimes measured most accurately in the UCR system are murder, robbery, and motor vehicle theft, with murder being the most accurate of the three (Zimring and Frase (1980), Blumstein 2000, and Tibbetts 2012).[10] Figure 4 presents time series data on these three crimes from the UCR

[10] It is widely appreciated within the crime literature that trends in the crime aggregates of violent and property crime are not particularly meaningful, because of differences in the scale of crime and the quality of measurement. For example, the most common violent crime is aggravated assault, which also happens to be the violent crime measured least well by the UCR system. Similarly, the most common property crime is larceny, which also happens to be the property crime measured least well by the UCR

system. The figure displays crime counts per 100,000 population over the period 1960-2012, the entire period over which UCR data are available electronically.[11] This 50 year period includes several powerful crime trends, including the famous crime wave of the late 1970s and the crack epidemic of the late 1980s and early 1990s.



55. These long-run comparisons are relevant for contextualizing claims in Mr. Brann's report that "the Part I crime rate [in Stockton] has been falling for the past several years" (Brann Report, p. 1). Without this context, his claims on Stockton's allegedly declining crime rate are highly misleading (Brann Report, p. 4-5).

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system. Consequently, I put little stock in the violent crime series presented by Professor Neumark in his Figure 2, except to the extent it resonates with the information in my Figure 4.

[11] Full data for 2012 have not been released by the FBI at this time, so I use crime counts from the semiannual release, pertaining to January through June, and double them. Comparing these approximate counts with Stockton's full January-December 2012 submissions to the UCR system indicates that this is a very accurate approximation. Stockton's data are in the electronic file *2012 15yrs of Stockton Crime.pdf*.

56. In his declaration, Mr. Brann makes the claim that crime in Stockton is not a particular problem currently, inasmuch as crime has been higher previously than it is currently and inasmuch as crime has been falling for the past several years. This is true as far as it goes, but presenting this fact without any context is highly misleading.

57. For all three crime categories, the situation was once more dire than it is now. Murder per capita was higher during the 1970s crime wave and the crack epidemic of the early 1990s, and robbery and motor vehicle theft were higher during the crack epidemic.

58. However, it is highly misleading to talk about crime in Stockton in isolation, as noted. Violent crime appears to be on the rise in Stockton since 2008, while during the same time period it is either flat or falling in comparable cities in the state, as noted in Figure 5A.

59. Relative to other cities in California, Stockton has recently seen a surprisingly high level of crime, particularly violent crimes such as murder and robbery. This is consistent with the recent declines in police staffing and the effect of police on crime just discussed. Figures 5A, 5B, and 5C plot the prevalence of murder, robbery, and motor vehicle theft in Stockton and for the two groups of comparison cities considered in Figures 2A and 2B, above. Murder in Stockton is disturbingly high relative to its own level in 2008 and high relative to the benchmark cities used here. The same is true for robbery. These observations are consistent with recent journalistic accounts regarding the brazen gold necklace thievery in Stockton (CBS 2012). Motor vehicle theft trends in Stockton are more in line with those of the comparison cities. This is consistent with the evidence from the police-crime literature, which documents a more muted effect of police on car theft.

Figure 5A. Relative Prevalence of Murder

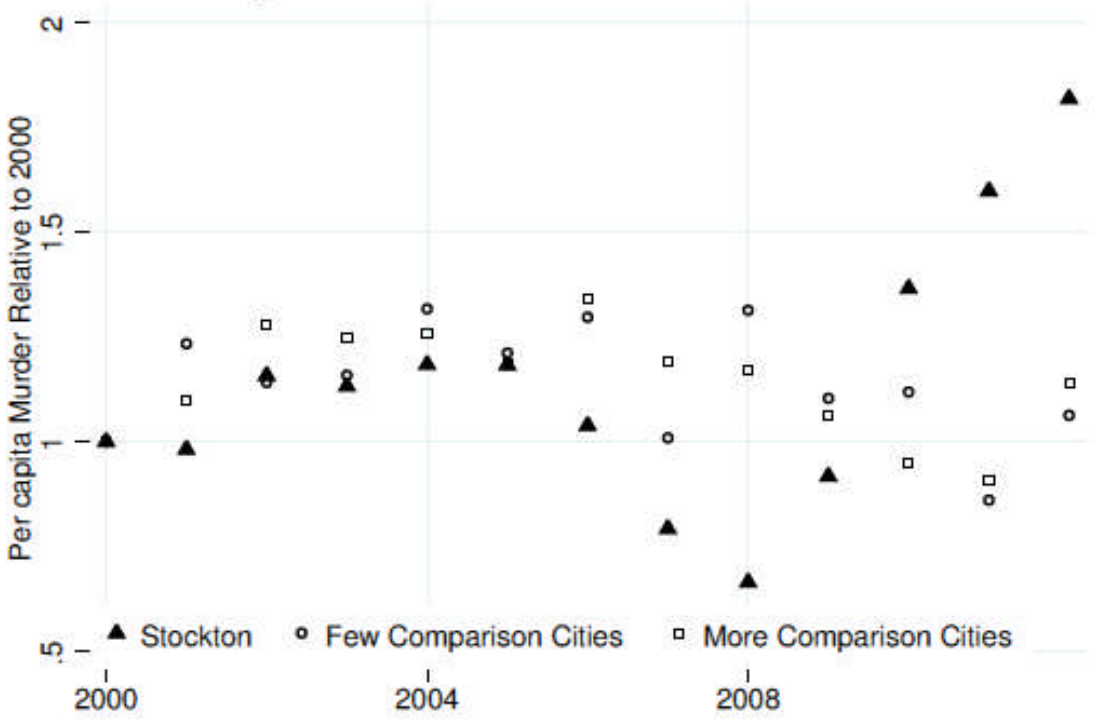
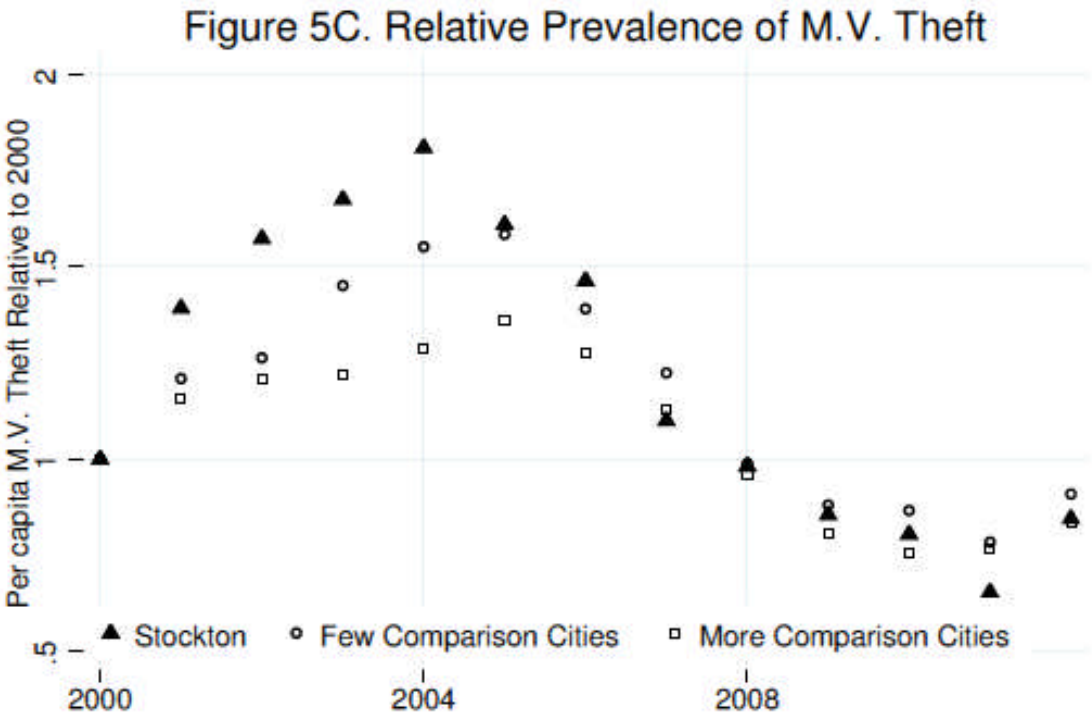


Figure 5B. Relative Prevalence of Robbery



Note: Data are from the Uniform Crime Reports. *Few comparison cities* are the Brann cities from Figure 2A. *More comparison cities* are the cities with 200–500K population from Figure 2B.

60. The analysis above attempts to convey the magnitude of the crime burden in Stockton by comparing Stockton to other cities, an issue Mr. Brann attempts to discount in his report (Brann Report, pp. 4-5). This analysis shows that Stockton is an unusually dangerous city and increasingly so since the size of the police force began to shrink dramatically.

XII. <u>Stockton Is Under-Policed And Cannot Afford To Lose More Officers</u>

61. Mr. Brann attempts to neutralize Stockton Police Chief Eric Jones' claims that Stockton is under-policed by arguing that Stockton does not have the lowest number of officers per capita among California cities, and that that metric is of limited, or perhaps no use without information on police officer utilization (Brann Report, pp. 5-8). Mr. Brann's statement that Stockton does not have the lowest number of officers per capita among California cities is true, but misleading. In his deposition and report, Mr. Brann made it clear that he believes any quantitative analysis must be contextual. However, in considering how the number of officers in the Department relates to the number of officers in other jurisdictions, his report is

surprisingly one-dimensional and non-contextual, considering only population. My analysis in Chalfin and McCrary (2013) is much more scientific and contextual.

62. I agree with Mr. Brann that police utilization is a key variable influencing crime. However, that utilization is important does not imply that manpower is unimportant. Indeed, as the Braga (2006) report notes, there may be a minimal level of staffing consistent with pro-active policing. Below a certain level of police staffing, police officers can only react to calls for service and are unable to take the necessary steps to reign in crime. Indeed, Mr. Brann himself acknowledges as much in his deposition, agreeing that every department as a "minimum number of officers that it needs to have on staff in order to fulfill the basic needs of policing the community" (Brann Dep., pp. 170:24-171:9).

63. Having studied the effects of police on the cost of crime in 242 U.S. cities, including Stockton, in a report I co-authored in 2013 (Chalfin and McCrary (2013)), I conclude that Mr. Brann erred in his attempt to neutralize Chief Jones' claims that Stockton is under-policed. Our findings in that paper indicated Stockton was the second-most under-policed city in California among cities with at least 200 thousand population ("medium to large California cities") as of 2010, given each additional dollar spent on policing would have saved Stockton $2.30 in crime costs (Chalfin and McCrary (2013), Supplemental Table, Row 193). As of 2011, however, Stockton became the single most under-policed city among medium to large California cities, surpassing Oakland, CA (Table 1)[12].

[12] Note that because I am here opting for a highly conservative cost of statistical life ("CSL") of $5 million, the numbers in Table 1 are slightly smaller than those in Chalfin and McCrary (2013). The numbers in Chalfin and McCrary (2013) are based on my best estimation of $7 million. A higher value of statistical life ("VSL") value implies a higher benefit-cost-ratio ("BCR").

Table 1. Recent Changes to the Estimated Benefit-Cost Ratio of Police Investments for Top 5 Most Under-Policed Cities in California With Population Above 200 Thousand

	2007		2008		2009	
Rank	City	BCR	City	BCR	City	BCR
1	Oakland	3.8	Oakland	3.6	Oakland	3.2
2	San Bernardino	3.1	San Bernardino	2.6	Stockton	2.6
3	Stockton	2.7	Stockton	2.4	Bakersfield	2.2
4	Sacramento	2.0	Bakersfield	2.1	Modesto	2.2
5	Fresno	2.0	Sacramento	2.0	Fresno	1.8

	2010		2011	
Rank	City	BCR	City	BCR
1	Stockton	3.5	Stockton	3.3
2	Oakland	3.4	Oakland	2.8
3	San Bernardino	2.5	San Bernardino	1.8
4	Bakersfield	2.3	Modesto	1.5
5	Fresno	1.9	Bakersfield	1.2

Note: Data from Uniform Crime Reports. BCR=benefit-cost ratio. Details of calculation and framework given in Chalfin and McCrary (2013) and summarized in report.

64. An alternative way to convey the magnitude of the crime burden in Stockton is to *monetize* the cost of crime. Monetizing the cost of crime is a highly accepted practice both by state governments and the federal governments[13] as well as the criminology and economics literature (see, e.g., Cohen and Piquero (2008)). See Appendix A for specific methods on how monetizing the cost of crime is done.

65. My methodology for finding Stockton is the most under-policed medium to large city in California is as follows: Using the generally accepted methods described in Appendix A, I calculated the cost of crime per capita for residents in Stockton to be approximately $15,000, or nearly three quarters of per capita income in Stockton.[14] This is the second-highest cost of crime per capita for a city in California with more than 200,000 population, behind only Oakland.

[13] See, for example, the U.S. Environmental Protection Agency website, http://yosemite.epa.gov/ee/epa/eed.nsf/pages/MortalityRiskValuation.html#pastvsl. Robinson (2007) reviews U.S. agencies and their safety/money tradeoffs. CalTrans (2012) outlines that they also utilize this type of approach.

[14] Income per capita taken from the American Community Survey 5-year estimates for 2006-2010.

66. Combining the figure for the per capita cost of crime with an estimate of the "fully-loaded" cost of hiring an additional sworn officer of $200,000 allows for a simple cost-benefit analysis of investments in police.[15] Chalfin and McCrary (2013, Section III) shows that a cost-benefit analysis is justified under a rich theoretical framework that contemplates heterogeneity (differences) in the population and the possibility that policing could potentially crowd out private investments in precaution. Here, I update that work to reflect police and crime data through 2011, with a particular focus on medium to large California cities.[16]

67. With the recent departures from the Department and the ensuing crime wave, Stockton is the city in medium to large California cities most in need of maintaining the size of its police force. Specifically, I estimate that as of 2011 every $1 less spent on police costs city residents *over $3 in crime costs*. These numbers will likely be more extreme when complete data for 2012 become more available, since crime in Stockton in 2012 was relatively high compared to other California cities.

68. Pursuant to the analysis above, it would be cost-effective for the City to avoid taking measures that would exacerbate its existing retention and recruitment problems such as reducing officer pensions.

APPENDIX A: MONETIZING THE COST OF CRIME

69. Monetizing the cost of crime is hard to think of from an *ex post* perspective—i.e., *after* victimization—because as any victim of a truly violent crime will likely explain, they would pay almost any amount of money to have avoided the victimization. Similarly, those who survive the murder victim would forgo many, many worldly possessions to have their loved one back. That is, *ex post*, the cost of serious crime is nearly incalculable. This is not a useful perspective for policy making, because it implies gross overinvestment in safety.

70. However, this approach makes quite a bit of sense from an *ex ante* perspective—i.e., before victimization. People routinely bear costs to lower their probability of

[15] I use the estimated police elasticities of crime cited in Chalfin and McCrary (2013), which are likely conservative estimates of the effect of police on crime, as discussed in that paper.
[16] I am not able to update these numbers through 2012, as police data for 2012 are not yet available through the UCR system.

victimization. Some of these costs are quite explicit, such as with the purchase of a car or house alarm. Other such costs are perhaps more subconscious, such as when a person chooses not to rent a large apartment in a dangerous neighborhood, preferring instead to rent a small apartment in a relatively safer neighborhood.[17] Yet other such costs are fully implicit, such as the costs associated with making a detour when walking to avoid a somewhat dangerous block or alley. In making these choices, people are trading off money and safety. They accept personal safety risk in order to save money.

71. The fact that people routinely confront this *ex ante* tradeoff between safety and money provides a natural way for governments to monetize safety: collecting information on a variety of choices make and asking government to value safety to the same extent that individuals themselves value safety. At the federal and state level, this is in fact exactly what governments do.[18] A large literature within economics and criminology collects information on these safety/money tradeoffs. A recent paper, Cohen and Piquero (2008), summarizes much of this literature and presents estimates in 2007 dollars of the cost of victimization for the crimes that are measured by the UCR system. These estimates rely in large part on civil jury awards to victims of crime, which likely reflect a combination of *ex ante* and *ex post* perspectives. Updating these costs to 2010 dollars using the Consumer Price Index, the Cohen and Piquero (2008) figures are roughly $4 million for murder, $142,000 for rape, $12,600 for robbery, $39,000 for assault, $2,000 for burglary, $500 for larceny, and $5,800 for motor vehicle theft. For the specific crime of murder, a more appropriate benchmark is probably that used by federal and state agencies for investments that reduce the chance of death. The literature on the value of a *statistical* life (VSL) has deeply informed this policy issue and points to a value closer to $7 million in 2010 dollars. To understand how the VSL concept gets used, suppose that the VSL is given by x and that installing a given technology would reduce the probability of death annually by 10 percent. Then government should be willing to pay $0.10x$ to

[17] For the sake of the hypothetical, assume that the only amenity that differs across the two neighborhoods is the level of safety.

[18] See, e.g., the U.S. Environmental Protection Agency website, http://yosemite.epa.gov/ee/epa/eed.nsf/pages/MortalityRiskValuation.html#pastvsl. Robinson (2007) reviews U.S. agencies and their safety/money tradeoffs. CalTrans (2012) outlines that they also utilize this type of approach.

put that technology in place. Suppose instead that the technology would reduce the probability of death annually by 5 percent. Then government should be willing to pay 0.05*x* to put that technology in place. As noted, currently most state and federal government agencies set *x* at roughly $7 million. However, since the California Department of Transportation uses a figure slightly below that, and to be conservative, I use a figure of $5 million (CalTrans 2012). This figure is conservative because a higher value for the cost of murder justifies higher expenses on police.

72. Combining the $5 million figure with the inflation adjusted numbers from Cohen and Piquero (2008) allows me to construct the overall cost of crime, which is the weighted sum of the number of crimes for the 7 major index crimes measured in the UCR system. This calculation shows that the cost of crime per capita for residents in Stockton is approximately $15,000, or nearly three quarters of per capita income in Stockton.[19] This is the second-highest cost of crime per capita for a city in California with more than 200,000 population, behind only Oakland.

APPENDIX B: MATERIALS REVIEWED

- All documents reviewed by Professor Neumark (Neumark Report, pp. 3-6)
- All documents reviewed by Mr. Brann (Brann Report, pp. 21-24)
- All documents referenced in Chalfin and McCrary (2013)
- Chalfin and McCrary (2013), "The Effect of Police on Crime: New Evidence from U.S. Cities, 1960-2010," NBER Working Paper # 18815, February 2013.
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[19] Income per capita taken from the American Community Survey 5-year estimates for 2006-2010.

- 2012 15yrs of Stockton Crime.pdf (electronic file with Stockton submissions to UCR system for last 15 years)
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- Table_78-California.xls (electronic file with UCR information on police officers in 2011)
- Table_4_Offenses_Reported_to_Law_Enforcement_by_State_Alabama_through_California_2012.xls (electronic file with UCR information on crimes known to police in first half of 2012)
- table_8_offenses_known_to_law_enforcement_by_state_by_city_2011.xls (electronic file with UCR information on crimes known to police in 2011)
- RETA01.y01.gz (ASCII text electronic file from FBI with UCR "Return A" data for 2001)
- RETA10.DAT.gz (analogous, but for 2010; other years omitted, but file names are similar and cover 1960-2010)
- UCR17301.DAT.gz (ASCII text electronic file from FBI with UCR "Police Employee" data for 2001)
- PE02.gz (analogous but for 2002; other years omitted, but file names are similar and cover 2000-2010)
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- 2011-12_Annual_Budget.pdf (electronic file, budget for Stockton for 2011-12; other years omitted, but file names are similar and cover 2007-2012)

- E1_2012_Internet_Version.xls (electronic file, population data from California Department of Finance)
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- Lim, Hyeyoung, Hoon Lee, and Steven J. Cuvelier, "The Impact of Police Levels on Crime Rates: A Systematic Analysis of Methods and Statistics in Existing Studies," Asia Pacific Journal of Police and Criminal Justice, 2010, 8 (1), 49–82.
- Menniti, Andrea, "Stockton Police Warn of 77 Gold Chain Robberies Since March," CBS Sacramento, June 11, 2012.

Exhibit C

NBER WORKING PAPER SERIES

THE EFFECT OF POLICE ON CRIME: NEW EVIDENCE FROM U.S. CITIES, 1960-2010

Aaron Chalfin
Justin McCrary

Working Paper 18815
http://www.nber.org/papers/w18815

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The Effect of Police on Crime: New Evidence from U.S. Cities, 1960-2010
Aaron Chalfin and Justin McCrary
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ABSTRACT

We argue that the key impediment to accurate measurement of the effect of police on crime is not necessarily simultaneity bias, but bias due to mismeasurement of police. Using a new panel data set on crime in medium to large U.S. cities over 1960- 2010, we obtain measurement error corrected estimates of the police elasticity of the cost-weighted sum of crimes of roughly -0.5. The estimates confirm a controversial finding from the previous literature that police reduce violent crime more so than property crime.

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I. Introduction

One of the most intuitive predictions of deterrence theory is that an increase in a typical offender's chance of being caught decreases crime. This prediction is a core part of Becker's (1968) account of deterrence theory and is also present in historical articulations of deterrence theory, such as 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), and Lee and McCrary (2009), among others.[1]

On the empirical side, one of the larger literatures in crime focuses on the effect of police on crime, where police are viewed as a primary factor influencing the chance of apprehension facing a potential offender.[2] This literature is ably summarized by Cameron (1988), Nagin (1998), Eck and Maguire (2000), Skogan and Frydl (2004), and Levitt and Miles (2006), all of whom provide extensive references.

Papers in this literature employ a wide variety of econometric approaches. Early empirical papers such as Ehrlich (1972) and Wilson and Boland (1978) focused on the cross-sectional association between police and crime. More recently, concern over the potential endogeneity of policing levels has led to a predominance of papers using panel data techniques such as first-differencing and, more recently, quasi-experimental techniques such as instrumental variables (IV) and differences-in-differences. Prominent panel data papers include Cornwell and Trumbull (1994), Marvell and Moody (1996), Witt, Clarke and Fielding (1999), Fajnzylber, Lederman and Loayza (2002), and Baltagi (2006). Some of the leading examples of quasi-experimental papers are Levitt (1997), Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005), Evans and Owens (2007), and Machin and Marie (2011).

Despite their extraordinary creativity, the quasi-experimental approaches pursued in the literature are typically limited in terms of their inferences by difficulties with precision. For example, a typical finding from this literature is that the police elasticity is larger in magnitude for violent crime than for property crime. This finding is often viewed skeptically however, as there is a common belief that violent crimes such as murder or rape are more apt to be crimes of passion than property crimes such as motor vehicle theft. However, the standard errors on the violent and property crime estimates from the previous literature have been large enough that it is unclear whether the difference in the point estimates is distinguishable from zero. Indeed, for many of the papers in the literature, estimated police elasticities for specific crimes are only

[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] A related literature considers the efficacy of adoption of "best practices" in policing. Declines in crime have been linked to the adoption of "hot spots" policing (Sherman and Rogan 1995, Sherman and Weisburd 1995, Braga 2001, Braga 2005, Weisburd 2005, Braga and Bond 2008, Berk and MacDonald 2010), "problem-oriented" policing (Braga, Weisburd, Waring, Mazerolle, Spelman and Gajewski 1999, Braga, Kennedy, Waring and Piehl 2001, Weisburd, Telep, Hinckle and Eck 2010) and a variety of similarly proactive approaches. In this paper, we address the effect of additional manpower, under the assumption that police departments operate according to "business-as-usual" practices. As a result, the estimates we report are likely an underestimate with respect to what is possible if additional officers are hired and utilized optimally.

statistically distinct from zero if additional pooling restrictions are imposed (e.g., equal effect sizes for all violent crime categories). Overall, the imprecision of the estimates from the quasi-experimental literature has led to substantial ambiguity regarding the substance of its findings.

Approaches based on natural variation lead to notably more precise estimates than do quasi-experimental approaches, but the former may be apt to bias because of confounding. This suggests there is merit in assessing the extent of confounding. We present evidence that confounding may be less of an issue than previously believed. In particular, using a new panel data set on crime, police, and a host of covariates for 242 large U.S. cities over the period 1960-2010, we demonstrate empirically that, conditional on standard controls, year-over-year changes in police have generally weak associations with the confounders mentioned in the literature, such as demographic factors, the local economy, city budgets, social disorganization, and recent changes in crime. This new dataset covers more cities than have been used and more years than have been used in most (but not all) of the previous literature.

The weakness of the correlations between police and confounders suggests that estimates of the effect of police on crime using natural variation in police may be only slightly biased, despite the *a priori* concerns raised in the literature. A potential problem, however, with using natural variation is that any measurement error in police could lead to bias of a different nature—measurement error bias. The "iron law of econometrics" is that, in a regression, the coefficient on a predicting variable will be too small in magnitude if it is measured with error, with the bias increasing in the amount of measurement error (Hausman 2001). Most natural experiment approaches, such as IV, do not suffer from the same bias (see, for example, Bound, Brown and Mathiowetz (2001)), at least under the hypotheses of the classical measurement error model. Measurement error bias thus has the potential to explain the larger magnitude of the estimates from the quasi-experimental literature, as compared to the traditional literature using natural variation, which has not addressed the issue of measurement errors in police. We show that there is a surprisingly high degree of measurement error in the basic dataset on police used in the U.S. literature, the Uniform Crime Reports (UCR).[3] Estimates from the older panel data literature that failed to account for measurement error bias were likely too small by a factor of 5.

The core of our paper is a series of measurement error corrected estimates of the effect of police on crime using natural variation in year-over-year changes in police at the city level in the U.S. in recent decades. Our estimated police elasticities are substantively large and, taken at face value, suggest that the social value of an additional dollar spent on police in 2010 is approximately $1.60. We introduce a conceptual framework articulating precise conditions under which such a cost-benefit test justifies hiring additional police. The

[3]The degree to which estimates of the total number of police nationally are compromised by measurement errors in the UCR data has been noted by Eck and Maguire (2000). However, they do not discuss the potential for measurement errors at the city level to bias estimates of the police elasticity derived from panel data.

results we introduce along these lines parallel the "sufficient statistic" results discussed in some of the recent public finance literature (e.g., Chetty 2009).

In addition to being significant in substantive terms, our estimated police elasticities are significant in statistical terms. The precision of our estimates allows us to confirm the common and somewhat surprising finding from the previous literature, alluded to above, that police have more of an influence on violent crime than on property crime.[4] However, prior literature has not been able to reject the null hypothesis that the violent crime elasticity is equal to the property crime elasticity, due to imprecise estimates. Our analysis is the first to demonstrate that this apparent finding is unlikely to be due to chance.

Essential to our empirical approach is the existence of two independent measures of police. We combine the standard UCR data on the number of police with data on the number of police from the Annual Survey of Government (ASG). Under the assumptions of the classical measurement error model, described below, IV using one measure as an instrument for the other is a consistent estimator for the results of least squares, were there to be no measurement error. The assumptions of the classical measurement error model are strong, but partially testable. We present the results of a battery of tests of the hypotheses of the classical measurement error model, finding little evidence in our data against them. The tests we utilize would appear to be new to the literature.

Since we focus on natural variation in policing, it is, of course, possible that our estimates are subject to simultaneity bias. It is typical in this literature to difference the data, thus removing between-city variation, and to control for national crime trends using year effects. As the quasi-experimental literature has emphasized, however, this approach may be compromised by confounders associated with growth rates in police and growth rates in crime. A particular concern is that changes in regional macroeconomic conditions, shocks to regional crime markets, or changes in state-level criminal justice policies may act as important confounders, thus biasing the results from standard panel data approaches. 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 might plausibly lead to both increases in police through increased block grants and passage of more punitive state sentencing policies. Such an attitude might be associated with harsher sentencing along both the intensive and extensive margin, changes in a state's capital punishment regime, decreases in the generosity of the state's welfare system or changes in the provision of other public services to low-income individuals.

We seek to address these potential sources of bias with the inclusion of state-by-year effects, an innovation

[4]The cross-crime pattern of the police elasticity estimates could reflect relative deterrence effects, relative incapacitation effects, or non-classical measurement error. The deterrence effect of police is that some crimes will not occur, because a person notes the increase in police presence and thereby is deterred from committing the offense. The incapacitation effect of police is that some crimes will not occur because additional police will result in arrests, pre-trial detention, and jail time for those who offend (McCrary 2009). The non-classical measurement error hypothesis we have in mind is that increases in police might increase reporting of crimes to police. See Levitt (1998) for discussion.

that has not, to date, been utilized in the literature. These state-by-year effects add roughly 1,500 parameters to each set of IV estimates and control for unobserved heterogeneity in city-level crime rates that is constant within the state. Inclusion of these variables increases the R^2 in crime regressions to nearly 60 percent for most crime categories. This is a remarkably high degree of explanatory power for a panel data model specified in growth rates. To the extent that omitted variables bias remains, we note that the previous literature has emphasized that simultaneity bias would lead regression estimates to be positively biased, i.e., to understate the magnitude of the police elasticity of crime (e.g., Nagin 1978, 1998). This reasoning would suggest that our estimates are conservative in magnitude.

The police elasticity of crime is obviously an important component of any public policy discussion regarding the wisdom of changes in police staffing. However, public investments in policing may crowd out private investments in precaution, making a social welfare evaluation of police more involved than it would at first appear. In Section II, we articulate precise conditions under which the police elasticity of crime can be used as a basis for social welfare analysis when private precautions are a first-order consideration. Our framework is related to recent work in public finance emphasizing the central role of policy elasticities in social welfare analysis (e.g., Chetty 2009).

After the social welfare analysis of Section II, Section III shows police hiring is only weakly related to the usual suspected confounders and discusses institutional aspects of police hiring that limit the scope for confounding. This section also provides some comments regarding interpretation. Next, in Section IV, we present direct evidence on the degree of measurement error in survey and administrative data on the number of police. We then outline our econometric methodology in Section V, discuss our primary data in Section VI, and report estimated police elasticities of crime in Section VII. In Section VIII, we compare our results to those from the previous literature. Section IX connects the social welfare analysis of Section II with the empirical findings of Sectio VII; produces a list of the 30 most overpoliced and 30 most underpoliced cities in our sample; and discusses the robustness of our policy conclusions to incapacitation effects of police. Finally, Section X concludes.

II. Conceptual Framework

Our paper provides an empirical examination of the magnitude of the police elasticity of crime. A natural question is whether the elasticity estimates we present are large or small. We now introduce a conceptual framework designed to adress this issue.[5] The framework will provide conditions under which comparing a police elasticity of crime to the ratio of taxes for supporting public policing to the expected cost of crime is a valid basis for welfare analysis (cf., Saez 2001, Chetty 2006, 2009). That is, this section answers the question: Supposing policing passes

[5]Our analysis holds fixed the punishment schedule facing offenders and asks only how to optimally set the probability of apprehension. This can be thought of as a social welfare analysis focused on the choice of policing facing a city having little influence on state sentencing policy.

a cost-benefit test, under what types of conditions is this sufficient to justify hiring additional police officers?

Here is the basic framework we consider. Suppose society consists of n individuals with linear utility over wealth. Each individual i faces a probability of victimization that depends on own precautions, X_i, the precautions of others, and policing, S. The probability of victimization is denoted $\phi_i \equiv \phi_i(X_1, X_2, \ldots, X_n, S)$ and ϕ_i is assumed continuous in all arguments and convex in X_i and in S. To finance policing, each individual pays a lump-sum tax, τ. We assume agents are in a Nash equilibrium, so that the beliefs of any one individual regarding the precautions of others is consistent with the beliefs of the others regarding the precaution of the one. For person i, we take expected utility to be given by

$$U_i \quad = \quad (y_i - k_i)\,\phi_i + y_i(1 - \phi_i) = y_i - k_i\phi_i \tag{1}$$

where k_i is the cost of crime, $y_i = A_i - \tau - p_i X_i$ is after-tax wealth net of expenditures on precautions, A_i is initial wealth, and p_i is the price of precaution. We assume any goods that must be purchased in order to obtain precaution are produced under conditions of perfect competition, implying that the only social value of precaution is in lowering crime.[6] Our definition of expected utility can either be thought of as implying that society is comprised exclusively of potential victims or as implying that the social planner refuses to dignify the perpetrator's increased utility, as in Stigler (1970).[7]

Our social planner faces two types of constraints. The *financing* constraint is that total tax receipts for policing, $n\tau$, must equal total expenditures, wS, where w is the cost of hiring an additional officer. The *liberty* constraint is that the social planner is either unwilling or unable to dictate an individual's investments in precaution. To motivate the liberty constraint, note that a person installing a burglar alarm would not be held liable in tort for the burglary of her neighbor, even if it could be shown that the cause of her neighbor's burglary was the installation of the alarm. The liberty constraint is thus one that actual governments respect. To clarify that our social planner calculations are different from an unrestricted social planner's calculations where precautions could conceivably be dictated, we refer to the constrained social planner as the *state*. We define the state's problem as the maximization of average expected utility, $\frac{1}{n}\sum_{i=1}^{n} U_i$, subject to the financing and liberty constraints. This problem can be thought of as (1) delegating to each individual the choice of precaution; and (2) maximizing the average indirect utility function over policing. To solve the state's problem, then, we begin by solving the individual's problem.

Individuals adjust precautions to maximize expected utility. The first order necessary condition for this problem, which is also sufficient under our assumptions, is $p_i = -k_i\phi_{ii}$, where the second subscript indicates a

[6] Precaution may or may not involve a market transaction. For example, it could entail circumnavigating a dangerous neighborhood at the expense of extra travel time, or it could also involve the purchase of a burglar alarm. In these examples, the price of precaution is either the cost of the additional travel time or the market price of the alarm.

[7] See Cameron (1989) for a valuable discussion of these conceptual issues and extensive references to the relevant literature.

partial derivative. We assume that precautions and policing are both protective against crime, or that $\phi_{ii} < 0$ and $\phi_{iS} < 0$. Solving the first order condition for X_i leads to a reaction function, $X_i(X_{-i}, S)$, specifying the privately optimal level of precaution as a function of the precaution of others and policing, where X_{-i} is the vector of precautions for all agents other than i.[8]

Under the assumptions above, each agent has a unique best strategy for any given set of beliefs regarding the actions of other agents, and we obtain a Nash equilibrium in pure strategies (Dasgupta and Maskin 1986, Theorems 1, 2). Figure 1 shows individual reaction functions for the $n = 2$ case under high and low policing.[9] The equilibrium requirement that beliefs be mutually consistent implies a set of restrictions. These restrictions lead to equilibrium demand functions, or the level of precaution demanded by person i as a function of policing, prices, taxes, and assets alone (i.e., not the precautions of others). Write equilibrium demand for precaution as $X_i(S)$. Substituting the equilibrium demand functions into the individual's utility function yields equilibrium maximized expected utility for the individual, or

$$V_i(S) \quad = \quad A_i - \tau - p_i X_i(S) - k_i \phi_i\Big(X_1(S), X_2(S), \ldots, X_n(S), S\Big) \tag{2}$$

The state maximizes the average $V_i(S)$ subject to the financing constraint. Define $\mathcal{V}(S) \equiv \frac{1}{n}\sum_i V_i(S)$ where $\tau = wS/n$. The first order necessary condition, which is also sufficient, is $0 = \mathcal{V}'(S) = \frac{1}{n}\sum_i(-w/n + V_i'(S))$. In this framework, police affect expected utility for individuals through five distinct mechanisms:

1. additional police lower utility by increasing the tax burden ($-w/n < 0$);
2. additional police increase utility by lowering expenditures on precaution ($-p_i X_i'(S) > 0$);
3. additional police lower utility by crowding out precaution, thereby increasing the probability of crime indirectly ($-k_i\phi_{ii}X_i'(S) < 0$);
4. additional police increase utility by reducing the probability of crime directly ($-k_i\phi_{iS} > 0$); and
5. additional police either lower or increase utility by crowding out precautions by persons $\ell \neq i$, either increasing or decreasing, respectively, the probability of crime externally (the sign of $-k_i\phi_{i\ell}X_\ell'(S)$ is ambiguous because the sign of $\phi_{i\ell}$ is ambiguous)

The first order condition for the state's problem reflects these different mechanisms. Multiplying the first order condition by S/C, where $C = \frac{1}{n}\sum_{i=1}^n k_i\phi_i$ is the *crime index*, or the average expected cost of crime, does not change the sign of the derivative and yields a convenient elasticity representation. We have

$$\mathcal{V}'(S)\frac{S}{C} \quad = \quad -\frac{wS}{nC} - \sum_{i=1}^n \omega_i\rho_i\eta_i - \sum_{i=1}^n \omega_i\varepsilon_{ii}\eta_i - \sum_{i=1}^n \omega_i\varepsilon_{iS} - \sum_{i=1}^n \omega_i \sum_{\ell\neq i} \varepsilon_{i\ell}\eta_\ell \tag{3}$$

where $wS/(nC) = \tau/C$ is the tax burden relative to the expected cost of crime, $\omega_i = k_i\phi_i/\sum_{i=1}^n k_i\phi_i$ is the fraction of the expected cost of crime borne by person i, $\rho_i = p_i X_i(S)/(k_i\phi_i) < 1$ is the ratio of precaution expenses to the expected cost of crime, $\varepsilon_{iS} = \phi_{iS}S/\phi_i < 0$ is the partial elasticity of the probability of crime

[8] We suppress the dependence of the reaction function on prices, taxes, and initial assets to maintain a simple presentation.

[9] The example assumes $-\ln\phi_i(X_1, X_2, S) = \alpha X_i + \beta X_{-i} + \gamma S$, with $\beta < \alpha$, which leads to linear reaction functions $X_i(X_{-i}, S) = (1/\alpha)\left(\ln(\alpha k_i/p_i) - \beta X_{-i} - \gamma S\right)$. This formulation thus echoes the traditional textbook treatment of Cournot duopoly with linear demand (e.g., Tirole 1988, Chapter 5).

for person i with respect to policing, $\varepsilon_{i\ell} = \phi_{i\ell} X_\ell(S)/\phi_i$ is the partial elasticity of the probability of crime for person i with respect to precaution for person ℓ, and $\eta_i = X_i'(S)S/X_i(S)$ is the elasticity of precaution for person i with respect to policing. The five terms in equation (3) correspond to the five different mechanisms described above. Note that if individuals are taking optimal precautions, then the second and third mechanisms exactly offset, i.e., $-\sum_i \omega_i \rho_i \eta_i - \sum_i \omega_i \varepsilon_{ii} \eta_i = 0$, or the envelope theorem.

We now turn to the task of connecting the state's optimality condition to observable quantities, in particular the police elasticity of crime. Estimates of the police elasticity of crime are of two types. The first type is a *total* police elasticity, so called because it reflects both the direct reduction in crime due to increasing police as well as the indirect increase in crime due to reductions to precautions that result from hiring police. The second type is a *partial* police elasticity, so called because it holds precautions fixed and thus reflects only the direct reduction in crime due to increased police. Since our study focuses on changes in crime associated with year-to-year fluctuations in policing, we believe that our study most likely identifies a partial police elasticity, at least if most precautions are fixed investments, such as deadbolts and burglar alarms, or if precautions take the form of habits of potential crime victims that are slow to evolve. Because this is plausible but not demonstrable, however, we provide empirical calibrations both under the assumption that our study identifies the partial elasticity and under the assumption that it identifies the total elasticity.

To make these ideas explicit, note that the total and partial elasticities are given by

$$\widetilde{\theta} = \left(\frac{1}{n} \sum_{i=1}^{n} k_i \left\{ \phi_{ii} X_i'(S) + \phi_{iS} + \sum_{\ell \neq i} \phi_{i\ell} X_\ell'(S) \right\} \right) \frac{S}{C} = \sum_{i=1}^{n} \omega_i \left(\varepsilon_{ii} \eta_i + \varepsilon_{iS} + \sum_{\ell \neq i} \varepsilon_{i\ell} \eta_\ell \right) \quad (4)$$

$$\text{and} \quad \theta = \left(\frac{1}{n} \sum_{i=1}^{n} k_i \phi_{iS} \right) \frac{S}{C} = \sum_{i=1}^{n} \omega_i \varepsilon_{iS} \ , \quad (5)$$

respectively. Next, combining equations (3), (4), and (5), we have

$$\mathcal{V}'(S)\frac{S}{C} = -\frac{wS}{nC} - \sum_{i=1}^{n} \omega_i \rho_i \eta_i - \widetilde{\theta} \equiv -\frac{wS}{nC} - r - \widetilde{\theta} \quad (6)$$

$$= -\frac{wS}{nC} - \sum_{i=1}^{n} \omega_i \sum_{\ell \neq i} \varepsilon_{i\ell} \eta_\ell - \theta \equiv -\frac{wS}{nC} - e - \theta \quad (7)$$

where $r = \sum_{i=1}^{n} \omega_i \rho_i \eta_i$ is the *crowdout effect*, or the weighted average product of the ratio of precaution expenses to the expected cost of crime (ρ_i) and the elasticity of precaution with respect to policing (η_i), and $e = \sum_i \omega_i \sum_{\ell \neq i} \varepsilon_{i\ell} \eta_\ell$ is the *externality effect*, or the weighted average change in the crime index that results from policing crowding out precautions and externally impacting crime (i.e., the fifth mechanism affecting expected utility described above). The weights in the weighted average (ω_i) correspond to the fraction of the total expected cost of crime borne by person i.

The signs of the crowdout and externality effects will be important for some of our reasoning. Consider first the crowdout effect. While we can imagine that a given individual might perversely increase precaution with increased policing,[10] we believe that this is rare. We assume that, at least on average in the population, policing crowds out precautions. Since ρ_i cannot be negative, this means we assume $r \leq 0$.

The sign of the externality effect is somewhat more ambiguous. On the one hand, if forced to guess we would say that most precautions have beggar-they-neighbor effects (i.e., for most i and ℓ, $\varepsilon_{i\ell} \geq 0$), implying a negative overall externality effect, or $e \leq 0$. On the other hand, there are of course precautions that have positive externalities, such as LoJack®. Finally, many precautions have aspects of *both* positive and negative externalities.[11] Consequently, although we have a prior view, we will calibrate our empirical analysis allowing for both positive and negative externality effects.

As noted, equations (6) and (7) are both proportional to the first order condition for the state's problem of maximizing $\mathcal{V}(S)$. Consequently, the state's solution can be recast in terms of the total and partial police elasticities, taxes relative to the expected cost of crime, the externality effect, and the crowdout effect.

Consider first the possibility that our empirical analysis identifies the total elasticity, $\widetilde{\theta}$, i.e., that precautions adjust quickly. Rearranging equation (6) shows that

$$\mathcal{V}'(S) > 0 \quad \Longleftrightarrow \quad \widetilde{\theta}\left(1 + r/\widetilde{\theta}\right) < -\frac{wS}{nC} \tag{8}$$

Suppose that increasing police is worthwhile in the provisional cost-benefit sense that

$$|\widetilde{\theta}|/\frac{wS}{nC} \equiv \widetilde{\kappa} \quad > \quad 1 \tag{9}$$

Since r and $\widetilde{\theta}$ share sign, the adjustment term $1 + r/\widetilde{\theta}$ is bigger than one, and if $\widetilde{\kappa} > 1$ then it is conservative to conclude that increasing police is welfare improving. Intuitively, this follows since increasing police under this scenario has two benefits for individuals—reduced crime and reduced expenditures on precaution—and only the first benefit is measured by the police elasticity.

Consider next the possibility that our empirical analysis identifies the partial elasticity, θ, i.e., that precautions are slow to adjust. Rearranging equation (7) shows that

$$\mathcal{V}'(S) > 0 \quad \Longleftrightarrow \quad \theta\left(1 + e/\theta\right) < -\frac{wS}{nC} \tag{10}$$

Suppose now that increasing police is worthwhile in the provisional cost-benefit sense that

[10] For example, we can imagine an individual who does not think installing a camera is worth it, because she does not believe there are enough police to follow up on any leads she might give them.

[11] For example, the Club® has a negative externality in that it may displace car theft to another car (Ayres and Levitt 1998). On the other hand, each additional car using the Club® raises search costs for the car thief and provides a marginal disincentive to car theft. As a second example, consider a business installing a security camera. The camera could have a negative externality in displacing a burglary to another business and a positive externality in deterring a sidewalk robbery.

$$|\theta| \Big/ \frac{wS}{nC} \equiv \kappa \quad > \quad 1 \tag{11}$$

An analysis like that above shows that if $e \leq 0$, i.e., if precautions have beggar-thy-neighbor effects on average, then it is conservative to conclude that increasing police is welfare improving. This makes sense because under this scenario a typical person's precaution imposes a negative externality on others which government can mitigate through police hiring. Suppose instead that $e > 0$, or that precautions have positive externalities on average. In this scenario, government has an incentive to restrict public policing somewhat, in order to encourage precaution. We assume that externalities play a smaller role than the direct effect of policing, or that $e < |\theta|$.[12] We then have the bounds $0 < 1 + e/\theta < 1$, and the conclusion that

$$\mathcal{V}'(S) > 0 \qquad \Longleftrightarrow \qquad \theta < -\frac{wS}{nC}\frac{1}{1+e/\theta} \qquad \Longleftrightarrow \qquad |\theta| \Big/ \frac{wS}{nC} = \kappa > \frac{1}{1-e/|\theta|} \tag{12}$$

Consequently, the provisional conclusion that increasing police is welfare improving remains correct if

$$\kappa > \frac{1}{1-e/|\theta|} \qquad \Longleftrightarrow \qquad \frac{e}{|\theta|} < \frac{\kappa - 1}{\kappa} \tag{13}$$

In words, if $\kappa > 1$, hiring police improves welfare as long as the externality effect is not too big relative to the partial elasticity. For example, if $\kappa = 2$, then additional police are socially valuable unless the externality effect is half as large as the partial elasticity, and if $\kappa = 1.5$, then additional police are socially valuable unless the externality effect is one-third as large as the partial elasticity.

This basic framework is readily extended in a variety of directions. One such direction pertains to multiple crime categories, which will be relevant for our empirical calibrations. For multiple crime categories, the crime index continues to be defined as the average expected cost of crime but no longer has the simple definition from above because there is more than one crime category. However, if we redefine the crime index as

$$C \quad = \quad \frac{1}{n}\sum_{i=1}^{n}\sum_{j=1}^{J} k_i^j \phi_i^j \tag{14}$$

we retain the core conclusions of the above analysis with analogous redefinition of terms and greater notational complexity. In connecting our empirical results with this normative framework, we draw on the the literature seeking to estimate the cost of various crimes (e.g., Cohen 2000, Cohen and Piquero 2008). This literature can be understood as seeking to estimate k_i^j for a "typical person". With these estimates, we can take $\widehat{C} = \sum_j k^j N^j / P$ as an approximation to the true crime index, where P is a measure of population, k^j is the cost of crime j, and N^j is the number of such crimes reported to police in a given jurisdiction in a given year, or an approximation for $\sum_i \phi_i^j$. These measurement considerations suggest that in empirical analysis one

[12] Since θ is negative and $wS/(nC)$ is positive, the second inequality in (10) cannot be satisfied if $e > |\theta|$. If $e > |\theta|$ regardless of the level of S, then $\mathcal{V}'(S) > 0$ is never satisfied, and the state is at a corner solution where it is optimal to have no police.

could either use the cost-weighted sum of crimes per capita as a dependent variable, or use the cost-weighted sum of crimes as a dependent variable provided there were population controls included as covariates. We follow the latter approach, as we detail below. Consequently, throughout our analysis, we will consider not just the effect of police on aggregate crime, as is typical of most crime papers, but also the effect of police on the *cost-weighted crime index*, or the weighted sum of crimes, where the weights are an estimate of the cost of the crime. We provide detail on these weights in Section VI, below.

III. Institutional Background and Identification Strategy

As noted above, the primary focus of much of the recent literature on police and crime has been the potential endogeneity of changes in police force strength. These concerns are rooted in the notion that a city ideally intertemporally adjusts its policing levels to smooth the marginal disutility of crime for the median voter, just as a consumer in a lifecycle model ideally intertemporally adjusts purchases to smooth the marginal utility of consumption. Such intertemporal adjustments to police would lead changes in police levels to be endogenous, i.e., to be correlated with unobserved determinants of changes in crime.

Our reading of the economics, political science, and public administration literatures is that the realities of city constraints and politics make intertemporal smoothing difficult, dampening the scope for endogeneity of this type. Cities labor under state- and city-level statutory and constitutional requirements that they balance their budgets annually,[13] they face tax and expenditure limitations,[14] they confront risks associated with hiring police due to legal and contractual obligations which encourages hiring as a means of solving long-term rather than short-term problems,[15] they may be operating under a consent decree or court order regarding racial, ethnic, or sex discrimination which may affect hiring decisions directly or indirectly and may affect retention,[16] they may suffer from inattention regarding staffing or may utilize staffing reductions as bargaining chips (e.g., bailout-seeking),[17] and cities may be hamstrung by unilateral changes to state and federal revenue sharing funds that are

[13] See Cope (1992), Lewis (1994), Rubin (1997), and City of Boston (2007).

[14] See Advisory Commission on Intergovernmental Relations (1977b, 1995), Joyce and Mullins (1991), Poterba and Rueben (1995), Shadbegian (1998, 1999).

[15] Regarding legal obligations, consider two examples: during 1972-1982, the federal government began pressuring departments to hire protected class group members with threat of withholding city and department revenues (Chicago Tribune 1972), and during 1972-1973, Massachusetts municipalities were unsure how to proceed with hiring in light of a constitutional challenge to a state statute allowing departments to favor city residents (Larkin 1973). Regarding contractual obligations, note that union contracts and state and local civil service ordinances may make it difficult to fire a police officer, even one who is substantially underperforming.

[16] For general background, see McCrary (2007).

[17] See, for example, LA Times (1966), Ireton (1976), or Recktenwald (1986a, 1986b). A common pattern is for police departments to have hired a large cohort of officers at some point. For some cities, this was after World War II, for other cities it was the late 1950s, and for other cities it was the 1960s crime wave. Combined with typical pension plans pegged to 20 years or 25 years of service, many departments face retirement waves roughly two decades after a hiring wave, setting the stage for a 20 to 25 year cycle unless the city exercises foresight. For example, in response to the famous Boston Police Strike of 1919, in which nearly three-quarters of the police department went on strike on September 9, then-governor Calvin Coolidge, having assumed control of the department on an emergency basis, refused to allow the strikers to return to work and replaced them all with veterans from World War I (Boston Police Department 1919, Russell 1975). This hiring burst, combined with the State-Boston

difficult to anticipate.[18] In addition, state and local civil service ordinances necessitate a lengthy and transparent hiring process making it difficult to adjust policing levels quickly or in great numbers.[19] Finally, cities may suffer from important principal-agent problems with elected officials having potentially quite different objectives from those of the median voter.[20] In short, if the city is analogous to a lifecycle consumer, it is most akin to one confronting liquidity constraints, limited information, inattention, and perhaps even self-commitment problems.

To amplify these points, consider the case of Chicago over the last five decades. Figure 2 presents an annotated time series of the number of sworn officers in the Chicago Police Department. In 1961, there were just over 10,000 sworn officers in Chicago. Crime and, in particular, the inadequacy of law enforcement was a major theme of the 1964 presidential election (Dodd 1964, Pearson 1964). As riots broke out in many U.S. cities between 1965 and 1968 (National Advisory Commission on Civil Disorders 1968), federal revenue sharing dollars made their way into Chicago budgets and the number of police increased rapidly (Varon 1975). By 1971, the number of sworn officers had risen to just over 13,000. A 1970 suit filed by the Afro-American Patrolmen's League against Chicago alleging *inter alia* discrimination in violation of 42 U.S.C. §1981, the modern legacy of §1 of the Civil Rights Act of 1866, was later joined by the Department of Justice in 1973 after the 1972 amendments to the 1964 Civil Rights Act expanded coverage of the Act to government employers.[21] Eventually, Judge Prentice H. Marshall, a self-described activist judge, reached a now-famous standoff with Mayor Richard Daley (Dardick 2004). Marshall ordered the department to use a quota system for future hiring in order to remedy discrimination in past hiring practices. Daley insisted that under such conditions, he did not intend to hire many officers. After impressive brinkmanship, Daley yielded when it became clear that failing to follow the court order would mean the loss of $100 million dollars in federal funds (Enstad 1976). Thereafter, the city faced a serious budget crisis (O'Shea 1981). The early 1980s saw the initiation of a long-term hiring freeze (Davis 1985), and with attrition the number of sworn officers fell from 12,916 in February 1983 to 11,945 in May 1986.

By summer 1986, the city faced a tidal wave of upcoming retirements. The department had added a large number of officers in the late 1950s, and those officers were nearing retirement. As of early 1987, fully 4,000

Retirement System which provides for a defined benefit pension after 10 years if over 55 and after 20 years if of any age, led to a highly persistent "lumpiness" in the tenure distribution of the department (Boston Police Department 1940, Table VI).

[18]Relevant federal programs over this time period include the Law Enforcement Assistance Administration (1968-1982), the Edward Byrne Memorial State and Local Law Enforcement Assistance programs (1988-2006), the Local Law Enforcement Block Grant program (1996-2006), the Justice Assistance Grant (2006-present), and the Community Oriented Policing Services (1994-present). For background on federal programs, see Varon (1975), Hevesi (2005), Richman (2006), and James (2008). At its peak in the late 1970s, LEAA funding accounted for roughly 5 percent of state and local criminal justice expenditures (Advisory Commission on Intergovernmental Relations 1977a). Background on state programs, which are ubiquitous, is much more scarce, but see Richardson (1980).

[19]See, for example, Greisinger, Slovak and Molkup (1979) and Koper, Maguire and Moore (2001).

[20]This perspective is particularly emphasized in the political science literature; see Banfield and Wilson (1963), Salanick and Pfeffer (1977), Schwochau, Feuille and Delaney (1988), and Clingermayer and Feiock (2001).

[21]For background on this litigation, see McCrary (2007) generally and more specifically *Robinson v. Conlisk*, 385 F. Supp. 529 (N.D. Ill. 1974), *United States v. City of Chicago*, 385 F. Supp. 543 (N.D. Ill. 1974), and *United States v. City of Chicago*, 411 F. Supp. 218 (N.D. Ill. 1976).

officers were eligible for retirement. The city tried to get ahead of the predictable decline in manpower, but it could not hire quickly enough to replace departing officers (Recktenwald 1986a, 1986b). Consequently, the department began shrinking again from 12,809 in April 1987 to 12,055 in November 1989 as the crack epidemic was roughly three years old.[22] The department managed to return to 12,919 sworn officers by January 1992, however, and policing levels were roughly stable until the beginning of the Community Oriented Policing Services (COPS) program. Between COPS funds and improving city revenues from the strong economy, the number of sworn police officers approached 14,000, reaching a peak of 13,927 in December 1996. The numbers were then stable during the crime decline of the 2000s, but in the wake of the 2008 financial crisis, the number of officers declined to 12,244, eroding nearly all the gains in police strength since 1990. Recently released data from the UCR program suggest that the number of sworn officers fell to 12,092 in 2011, and recent city payroll data indicate that the number of officers in October 2012 stood at 11,937.[23]

Overall, the key takeaways from Figure 2 are that: (1) Chicago's police strength has fluctuated a great deal over the past five decades, with swings of 10 percent being rather common, and (2) these fluctuations seem to respond to perceptions of lawlessness, but are also the product of political haggling, budgetary mismanagement, gamesmanship, and a seeming lack of attention on the part of city planners. By our reading, these cycles are not limited to Chicago, but are a pervasive feature of police hiring in cities across the United States (cf., Wilson and Grammich 2009).

Sometimes, these cycles are driven by fiscal crisis and bad luck. For example, in 1981, Boston confronted a sluggish to recessionary economy, Proposition $2\frac{1}{2}$, and a major Massachusetts Supreme Court decision that led to large reductions in Boston's property tax revenue.[24] Massachusetts, like other states, requires municipalities to balance their budgets annually.[25] Forced to balance its budget, the city reduced the police department budget by over 27 percent, eliminated all police capital expenditures, closed many police stations, and reduced the number of sworn officers by 24 percent (Boston Police Department 1982).

Other times, these cycles are driven by mayoral objectives that are unrelated to crime. For example, in the mid 1970s, Mayor Coleman Young sought to aggressively hire officers under an affirmative action plan (Deslippe 2004). The department hired 1,245 officers under the plan in 1977, increasing the size of the police force by some 20 percent, and the next year, a further 227 officers were hired under the plan. After Detroit

[22] Based on our own readings, Chicago newspapers begin mentioning the crack epidemic in 1986, and this is also the date identified more quantitatively by Evans, Garthwaite and Moore (2012).

[23] See `http://www.fbi.gov/about-us/cjis/ucr/` and `http://data.cityofchicago.org`, both accessed on October 24, 2012. The financial crisis led to force reductions in many cities, most famously Camden, which laid off 45 percent of its sworn officers in early 2011 (Katz and Simon 2011).

[24] *Tregor v. Assessors of Boston*, 377 Mass. 602, *cert. denied* 44 U.S. 841 (1979). For background on Proposition $2\frac{1}{2}$, see Massachusetts Department of Revenue (2007).

[25] *General Laws of Massachusetts*, Chapter 59, Section 23. Note that these cuts were partially offset by intervention from state government. See in this regard footnote 27 and Figure 3D, below.

hired those officers, the city confronted a serious budget crisis, forcing the city 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, but a new round of cuts in 1983 undid this effort, and 224 officers were again laid off. In 1984, 135 of those officers were recalled.[26] These sharp changes indicate liquidity problems or perhaps bargaining.

These anecdotal considerations suggest that short-run changes in police are, to a great extent, idiosyncratic. That case is strengthened by establishing that changes in police are only weakly related to changes in observable variables.[27] We now present statistical evidence on the exogeneity of changes in police to several key social, economic and demographic factors, conditional on some basic controls. Each column of Table 1 presents coefficients from 13 separate regressions of the growth rate in the UCR or ASG measure of police on the growth rate in a potential confounder, conditional on the growth rate in city population and either year effects or state-by-year effects, and weighted by 2010 city population.[28] We motivate and describe in greater detail these controls below. For now, it is sufficient to understand that these are the key covariates we will condition on later in the paper, where we model crime growth rates as a function of police growth rates and other covariates. Standard errors, in parentheses, are robust to heteroskedasticity.[29]

The table is divided into three panels, each of which addresses a different class of potential confounders. Panel A explores the relationship between police and the local economy, as measured by personal income, adjusted gross income, wage and salary income, county-level total employment, and the city's municipal expenditures exclusive of police. There are four sets of models, corresponding to the UCR or ASG measure of police and to year effects or state-by-year effects. The estimates in Panel A give little indication that police hiring is strongly related to local economic conditions. While the estimates based on year effects are all positive, they are generally small in magnitude. For example, the largest estimated elasticity is that of police with respect to total county

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

[27] Note that we are *not* arguing that police levels fail to respond to crime in the medium- to long-run. Over a longer time horizon, cities may be able to overcome transaction costs and reoptimize, particularly when confronting severe crises. For example, cities facing a difficult crime problem may be able to obtain "emergency" funding from the state or federal government. Describing the situation in Washington, D.C., around 1994, Harriston and Flaherty (1994) note that the "hiring spree [in police] 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." As another example, in response to the 1980-1981 Boston police staffing crisis, "the Massachusetts Legislature enacted the Tregor Act [in 1982]... [providing] the city of Boston with new revenues... This legislative action terminated all layoffs and greatly diminished the risk that future layoffs might take place." *Boston Firefighters Union Local 718 v. Boston Chapter NAACP, Inc.*, 468 U.S. 1206, 1207 (1984). To the extent that even short-run fluctuations in police are partly responding to crime, it is likely that our estimates understate the effect of police on crime.

[28] As discussed in greater detail below, we include two separate measures of city population growth in these regressions to mitigate measurement error bias associated with errors in measuring city population. The UCR and ASG measures of police are described in Section VI, below. For details on the other variables used in this table, see the Data Appendix. We prefer not to control for these variables directly in our main analyses because they are missing for many years. However, after presenting our main results, we conduct a robustness analysis for the 1970-2002 subsample. During that time period, we can control for most of the potential confounders. These results, given in Table 7, show that our main effects are essentially unaffected by the inclusion of further covariates.

[29] For this and all other tables in the paper, we have additionally computed standard errors that are clustered at the level of the city. These are scarcely different from, and often smaller than, those based on Huber-Eicker-White techniques. The similarity in the standard errors suggests small intra-city residual correlations.

employment for the UCR measure given city population growth and year effects, where the elasticity estimate is 0.10. This would imply that a large 10 percent increase in total employment would result in a 1 percent increase in police. The estimates based on state-by-year effects are slightly smaller in magnitude and of varying signs. In addition to being economically small, these estimates are generally statistically indistinguishable from zero at conventional significance levels, or just barely on the cusp of statistical significance.

In Panel B of Table 1 we relate police hiring to several measures of social disorganization and a measure of the population at risk of arrest. The measures of social disorganization are the fraction of births where the mother is a teenager, for all babies as well as for African American babies, the fraction of births where the baby is low birthweight (less than 2500 grams, or about 5.5 pounds), and a proxy for the 12th grade dropout rate.[30] The first three measures are only available at the county level, while the fourth is measured at the city level. While not directly linked to crime, all four of these variables capture changes in local conditions which may correlate with a need for increased police. Notably, a casual examination of the city-specific time series for each of these variables shows that they are often strongly related to the onset of the "crack epidemic" that swept through U.S. cities during the late 1980s and early 1990s.[31] The results for these four variables are similar to those in Panel A, with little indication of a strong relationship between police and social disorganization, at least conditional on controls. The majority of the elasticities are negative, indicating that more social disorganization is associated with less policing rather than more, and are extremely small in magnitude. The elasticity of largest magnitude, that pertaining to the fraction of births where the mother is a teenager, among black births, for the ASG measure with year effects, is just -0.04.

The last variable in Panel B shows the relationship between police and a proxy for the number of persons at risk for arrest. This variable is constructed using Census data on county population for 16 detailed age-race-gender groups, weighted by the 2009 share of each of these groups among arrestees nationally.[32] For the UCR and ASG series, we estimate elasticities pertaining to the at-risk population of 0.11-0.12 and 0.05-0.10, respectively. As with the other estimates we have seen so far, controlling for year effects or even state-by-year effects matters little. These estimates suggest that a 10 percent increase in the at-risk population leads to a 1 to 1.3 percent increase in police, controlling for the covariates described. We note that a 10 percent increase in the population at risk of arrest is an extreme hypothetical representing 2.6 standard deviations for this variable. Overall, while there is some evidence that police hiring is responsive to demographic changes, we would characterize the relationship as fairly weak.

[30] The dropout rate proxy in year t is one minus the number of 12th graders in the city in year t relative to the number of 11th graders in the city in year $t-1$. See Data Appendix for discussion.

[31] The degree to which the "crack epidemic" represents an exogenous shock to local crime markets has been subject to considerable debate in the literature on the effect of state-level abortion policy on crime.

[32] Arrest data from the FBI were available through 2009 at the time of this writing.

Finally, Panel C presents elasticities of police with respect to three lagged crime aggregates: violent crimes, property crimes and a cost-weighted crime index which weights the prevalence of each crime by an estimate of the social damages associated with that crime (we describe these weights in detail in Section VI, below). These elasticities range from 0.004-0.012 for violent crimes to 0.008-0.019 for property crimes, suggesting that a 10 percent increase in crime would lead to no more than a 0.2 percent increase in police.[33] While the weak association reported here is somewhat at odds with the sense one gets from the existing academic literature, it is consistent with the limited available reports from interviews with police chiefs.[34]

Taken as a whole, the estimates in Table 1 suggest a more limited link between police hiring and potential confounders than the literature has presupposed, at least conditional on our preferred controls. While the results do not indicate a large discrepancy between models controlling for year effects and those controlling for state-by-year effects, in the remainder of the paper, we focus on models for crime that include state-by-year effects. These models are robust to any possible confounder that varies over time at the state level. This includes, for example, state-level policies that may affect crime, such as poor support, education policy, or penal policy. Most papers in the literature focus on models for year-over-year growth rates in crimes at the city level include year effects. Evans and Owens (2007) are unusual in focusing on a more flexible model involving group-specific year effects, where the groups are defined according to population and pre-COPS program crime trends. However, as far as we know, no paper in the literature has used state-by-year effects and thereby completely isolated the effect of police from the effect of state-level policies.

Before closing this section, we would like to address one final issue pertaining to interpretation. After reviewing the literature on police staffing fluctuations, including a non-random sampling of newspaper coverage for specific cities in specific years, we have the impression that policing increases are sometimes associated with the city council or mayor being pleased with the direction of the department. This may mean that the number of police partially proxies for changes in what police are doing, as well. For example, it is possible that increases in police are associated with the hiring of a popular new police chief or with the department being willing to transition to a community policing model. This might mean that our estimates, and those in the previous literature utilizing natural variation, capture an effect of police that is somewhat broader than just the effect of police manpower, *per se*.

[33] We note that a limitation of our analysis of possible confounders is that there are few variables which are collected systematically for a large number of cities for a long period of time. One suspected confounder in particular–calls for service–is sometimes reported in police department annual reports, but is not collected on a systematic basis by any organization. Consequently, we are unable to completely address the issue of possible confounders. On the other hand, calls for service likely does correlate strongly with other measures we do observe, such as crime.

[34] For example, Police Executive Research Forum (2005b) discusses the results of a focus group with four police chiefs (Largo, Scottsdale, Omaha, and Baltimore County) and a deputy chief (Charlotte-Mecklenburg). "Participants pointed out that the crime rate is usually not a major factor in budget success." (p. 18).

IV. Evidence on the Extent of Measurement Error in the Number of Police

A. Direct Evidence

We begin our discussion of the nature and extent of measurement errors in police personnel data using the case of New York City in 2003. The UCR data reports that the New York Police Department employed 28,614 sworn police officers in 2003.[35] 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 3 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.[36] These data confirm that the 2003 measure is in error and additionally suggest that the 1999 measure may be in error.[37]

Administrative data on the number of officers are difficult to obtain. More readily available are numbers from departmental annual reports. However, even these are not easy to obtain; annual reports are largely internal municipal documents and historically did not circulate widely. In recent years, many departments have begun a practice of posting annual reports online, but only a few cities post historical annual reports. Moreover, the annual report may or may not report the number of officers employed by the police department.[38]

Nonetheless, we have been able to obtain scattered observations on the number of sworn officers from annual reports for selected years for selected other cities: 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 (1997).[39] The time series of sworn officers for these cities is given in Figure 3 in panels B through E. Treating the administrative and annual report data as the true measure, it seems that 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. While the series are highly correlated in levels or logs, the correlation is notably lower after taking first differences (results unreported). This is important because most of the recent literature analyzes the data in first-differences or with city effects.

Many people are suprised that there are errors in measuring the number of police officers. Errors could

[35] As discussed below, the UCR measurement protocol is a snapshot of the stock of officers as of October 31 of the survey year.

[36] See the Data Appendix for details on these data. Special thanks to Frank Zimring for pointing us towards public domain information on New York police staffing based on his work on the New York City crime drop (Zimring 2011).

[37] We have discussed the 2003 measure of police with other scholars of crime and police, both in economics and in criminology. To date, we have not heard a plausible account for this number, other than that it is a data entry error.

[38] For example, the annual reports for the Boston Police Department are available online beginning in 1885, but the reports stop detailing the number of officers between 1972 and 1981, when the number of officers fell by 40 percent. See `http://www.bpl.org/online/govdocs/bpd_reports.htm`.

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

arise due to (1) transitory movements within the year in the number of police, (2) conceptual confusion, or (3) data entry errors. Regarding the first source of error, Figure 4 gives information on transitory movements in police for Chicago for the period 1979-1997. The figure displays the monthly count of the number of sworn officers, with the count for October superimposed as horizontal lines.[40,41] October is chosen because this is the reference month for the UCR data on police used in the literature. The figure demonstrates that there is a great deal of within-year volatility in the number of sworn officers. Overall, the series is characterized by hiring bursts followed by the gradual decline associated with losses due to retention or retirement. Transitory movements in police officers are relevant because surveys typically ask for a point-in-time measure, and the snapshot date differs across surveys. Among those we have been able to examine, internal police department documents use different reporting conventions, typically corresponding to the end of the municipal fiscal year, which varies across municipalities and over time. Perhaps responding to the ambiguities of point-in-time measures, the New York City Police Department uses average daily strength in internal documents.

In addition to transitory movements, there may also be conceptual ambiguity over who counts as a sworn police officer. First, there may be confusion between the number of total employees, which includes civilians, and the number of sworn officers. Second, newly hired sworn 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, due to frictions associated with the hiring process, there is often a discrepancy between the number of officers the department has authority from city government to employ ("authorized strength") and the number of officers currently employed ("deployed strength").[42] 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.[43]

Finally, the UCR measure of sworn police has errors (e.g., New York in 2003) that are inconsistent with transitory movements within the year in the number of sworn police officers and inconsistent with conceptual

[40] We are not aware of any public-use data sets containing information on within-year fluctuations in police. During the period 1979-1997, a unique non-public dataset on sworn officers in Chicago is available to the authors, however, that allows the construction of monthly counts. These data are discussed in Siskin and Griffin (1997) and were previously used in McCrary (2007). See the Data Appendix for details.

[41] A natural question is whether there is seasonality to police hiring, particularly since summer months are typically high crime months. A regression of log police on an exhaustive set of year and month dummies over the period 1979 to 1997, where monthly data are available, yield an R-square of 0.95. This regression gives little indication of seasonality. While the set of 18 year dummies have an F-statistic of over 193, the set of 11 month dummies have an F-statistic of 1.08, with a p-value of 0.38.

[42] Typical steps include a written examination, a drug test, a background check, an interview, and a series of physical and psychological tests, among others (Police Executive Research Forum 2005a, Wilson and Grammich 2009).

[43] 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. The LEMAS data also allow us to discount the possibility that there is error due to ambiguities among sworn officers, full-time sworn officers, or full-time-equivalent sworn officers, as only 1 to 2 percent of officers appear to work part-time.

confusion. For such errors, we have no other explanation than typographical or data entry error.[44]

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 the cities in our main analysis sample. We define this sample and give background on the ASG data in Section VI, below.

Figure 5 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 ("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 0.99. Panel B makes it clear that differencing the data substantially reduces the association between the two series; the slope coefficient for the data in growth rates is just 0.22. This much smaller relationship is the expected pattern when the true number of officers changes slowly (**?**, Section 26.2.5).

To appreciate the implications of these findings for quantification of the police elasticity of crime, we turn to a simple statistical model. Suppose the two observed series on police are related to true police as

$$S_i = S_i^* + u_i \tag{15}$$

$$Z_i = S_i^* + v_i \tag{16}$$

and suppose the outcome of interest, Y_i, is related to the true number of police and covariates X_i as

$$Y_i = \theta S_i^* + \gamma' X_i + \epsilon_i \tag{17}$$

Here, S_i is the log UCR measure in a given city and year, Z_i is the log ASG measure, S_i^* is the "true" log police or *signal*, X_i are other covariates measured without error, u_i and v_i are mean zero measurement errors that are mutually uncorrelated and uncorrelated with ϵ_i, S_i^*, and X_i, and ϵ_i is mean zero and uncorrelated with S_i^*, X_i, u_i, and v_i. Equations (15) through (17) and the stochastic restrictions just named constitute what is known as the classical measurement error model (Fuller 1987).

A famous result from the econometric literature on measurement errors (see, for example, Wooldridge

[44]It is worth noting that the crime data are the focus of the UCR system, with notably less attention paid to the police numbers. It is common to see a discussion of UCR crime figures in the local news and for local politicians to be under fire for any spikes in those numbers. However, neither of us have ever seen a local news discussion of the UCR measure of the number of officers. Perhaps because the release cycle used by the FBI for the UCR system involves releasing the numbers for police well after they release the numbers for crimes, reporters seem to ask cities directly for figures on police. This suggests that any lack of care in preparing the UCR police numbers would usually go unnoticed.

(2002, Section 4.4.2)) relates the probability limit of the least squares regression estimate of θ, based on using covariates S_i and X_i, to the scope of measurement errors and the relationship between the signal and the included covariates, under the assumptions of the classical measurement error model:

$$\begin{aligned}
\text{plim}_{n\to\infty}\ \widehat{\theta}_{\text{OLS}} &= \frac{\mathbb{C}[\mathcal{M}S_i, \mathcal{M}Y_i]}{\mathbb{V}[\mathcal{M}S_i]} = \frac{\mathbb{C}[\mathcal{M}S_i^* + \mathcal{M}u_i, \mathcal{M}Y_i]}{\mathbb{V}[\mathcal{M}S_i]} = \frac{\mathbb{C}[\mathcal{M}S_i^*, \mathcal{M}Y_i]}{\mathbb{V}[\mathcal{M}S_i]} && (18)\\
&= \theta\ \frac{\mathbb{V}[\mathcal{M}S_i^*]}{\mathbb{V}[\mathcal{M}S_i]} = \theta\ \frac{\mathbb{V}[\eta_i]}{\mathbb{V}[\eta_i + \mathcal{M}u_i]} = \theta\ \frac{\sigma_\eta^2}{\sigma_\eta^2 + \sigma_u^2} && (19)\\
&= \theta\ \frac{\sigma_*^2(1-R^2)}{\sigma_*^2(1-R^2) + \sigma_u^2} && (20)
\end{aligned}$$

where for a random variable A_i, we define $\mathcal{M}A_i = A_i - \mathbb{V}[X_i]^{-1}\mathbb{C}[X_i, A_i]$, i.e., applying $\mathcal{M}$ purges a random variable of its linear association with X_i, η_i is the associated linear projection residual, σ_η^2 is the variance of η_i, σ_u^2 is the variance of u_i from equation (15), and R^2 is the population R-squared from a regression of the signal S_i^* on the covariates X_i.[45]

This formula stands for three ideas. First, since $\sigma_u^2 > 0$, OLS will be too small in magnitude, or attenuated. Second, while it is a staple of empirical work to see whether a regression estimate is robust to the inclusion of various control variables, equation (20) indicates that the cure of additional covariates may be worse than the disease of omitted variables bias. Adding more controls increases the R^2, exacerbating any attenuation bias. Third, since the estimates of θ and γ will generally covary, the bias in the estimate of θ will spill over to result in bias in the estimate of γ.

Now return to equation (17) and suppose that X_i is measured without any errors. Under the models in equations (15) and (16) and the associated assumptions on u_i and v_i, it is straightforward to estimate the reliability ratio. The probability limit of the coefficient on Z_i in a regression of S_i on Z_i and X_i is

$$\frac{\mathbb{C}[\mathcal{M}S_i, \mathcal{M}Z_i]}{\mathbb{V}[\mathcal{M}S_i]} = \frac{\mathbb{C}[\mathcal{M}S_i^* + \mathcal{M}u_i, \mathcal{M}S_i^* + \mathcal{M}v_i]}{\mathbb{V}[\mathcal{M}S_i]} = \frac{\mathbb{V}[\mathcal{M}S_i^*]}{\mathbb{V}[\mathcal{M}S_i]} \equiv \pi \qquad (21)$$

This implies that the ratio of the least squares estimate of the police elasticity of crime, relative to the estimate of π, is consistent for θ, suggesting a role for IV. This also implies that, in the context of the discussion of Figure 5, a regression of log crime on log police will not be importantly compromised by measurement errors in police, because in logs the reliability ratio is 0.99. However, a regression of growth rates in crime on growth rates in police and other covariates will be compromised, because in growth rates the reliability ratio is 0.22. Indeed, as we show below, once population growth rates and state-year effects are included in the model, the reliability ratio falls to 0.16. Consequently, even setting aside problems with simultaneity bias of the type discussed in the literature, measurement errors in police suggest that least squares estimates of the police

[45] Recall that $R^2 = 1 - \sigma_\eta^2/\sigma_*^2$, so that $\sigma_\eta^2 = \sigma_*^2(1-R^2)$. The formula for $\mathcal{M}$ assumes X_i is a vector with no linear dependencies. More generally, $\mathcal{M}A_i$ is A_i less the linear projection onto the column space of X_i.

elasticity in the literature are too small by a factor of 5 or more.

V. Econometric Approach

The three equation model introduced in Section IV.B leads naturally to a simultaneous equations model. Substituting equation (15) into equation (17) and linearly projecting S_i onto Z_i and X_i yields

$$Y_i = \theta S_i + \gamma' X_i + \varepsilon_i \tag{22}$$
$$S_i = \pi Z_i + \phi' X_i + \nu_i \tag{23}$$

where we now interpret Y_i as the year-over-year change in log crime in a given city and year, S_i as the year-over-year difference in observed log police, and X_i as a vector of control variables such as log revenues per capita, log population, the demographic structure of the population, all measured in first differences, as well as year effects or state-by-year effects. In this model, $\varepsilon_i = \epsilon_i - \theta u_i$, and ν_i is a linear projection error. This is then a standard simultaneous equations model where Z_i is potentially an instrument for S_i.

Estimation of the parameters in equations (22) and (23) proceeds straightforwardly by IV since the model is just-identified, and 2010 city population is used as a weight to obtain a police elasticity estimate representative of the typical person. Sufficient conditions for excluding Z_i from equation (22) are

$$(A1) \qquad \mathbb{C}[u_i, \epsilon_i] = \mathbb{C}[v_i, \epsilon_i] = 0$$
$$(A2) \qquad \mathbb{C}[u_i, (S_i^*, X_i')'] = \mathbb{C}[v_i, (S_i^*, X_i')'] = 0$$
$$(A3) \qquad \mathbb{C}[u_i, v_i] = 0$$
$$(A4) \qquad \mathbb{C}[\epsilon_i, (S_i^*, X_i')'] = 0$$

where u_i and v_i are the measurement errors from equations (15) and (16) and ϵ_i is the structural error term from equation (17).[46]

Assumptions $(A1)$ through $(A3)$ assert that the measurement error in the UCR and ASG measures of police are not associated with the structural error term in equation (17), and are not associated with the true growth rate in police and the covariate vector X_i, and that the UCR and ASG measurement errors are mutually uncorrelated, respectively. We discuss empirical implications of assumptions $(A1)$ through $(A3)$ below. Assumption $(A4)$ is innocent if we maintain that we would be interested in running a regression of crime growth rates on police growth rates and controls X_i, were police growth rates observed without error. On the other hand, $(A4)$ may reasonably be called into question. In particular, city population growth rates are measured with error. City population growth is a sufficiently important confounder that we feel the (infeasible) regression model implied by equation (17) and assumption $(A4)$ would not be of interest unless X_i

[46] Assumptions $(A1)$ through $(A4)$ together imply that $\mathbb{E}[Z_i\epsilon_i] = \mathbb{E}[Z_i u_i] = 0$, which implies that $\mathbb{E}[Z_i\varepsilon_i] = 0$. Assumptions $(A2)$ and $(A4)$ imply that $\mathbb{E}[X_i\varepsilon_i] = 0$. Of course, $\mathbb{E}[(Z_i, X_i')'\varepsilon_i] = 0$ is one of the two familiar conditions for consistency of IV using Z_i as an excluded instrument and X_i as an included instrument. The other familiar condition, that the excluded instrument predict the endogenous regressor, i.e., that $\pi \neq 0$, is unremarkable in this context.

included it.[47] We discuss the challenges of mismeasurement of city population growth in greater detail below.

Under the classical measurement error, the exact same steps we used to motivate the simultaneous equations model in equations (22) and (23) can be used to motivate a second simultaneous model with the roles of S_i and Z_i reversed and identical parameters in equation (22).[48] We refer to IV models that use the ASG measure of police as an instrument for the UCR measure as *forward* IV estimates and to models that use the UCR measure of police as an instrument for the ASG measure as *reflected*. As noted, both IV estimates are consistent for the police elasticity of crime. This raises the possibility of pooling the estimates to increase efficiency. To do so, we stack the orthogonality conditions for the forward and reflected IV programs into the broader set of moments

$$g_i(\beta) = W_i \begin{pmatrix} Z_i(Y_i - \theta_1 S_i - \gamma_1' X_i) \\ X_i(Y_i - \theta_1 S_i - \gamma_1' X_i) \\ S_i(Y_i - \theta_2 Z_i - \gamma_2' X_i) \\ X_i(Y_i - \theta_2 Z_i - \gamma_2' X_i) \end{pmatrix} \tag{24}$$

where W_i is 2010 city population in levels and all other variables are as defined before, and we estimate the parameters using generalized method of moments (GMM). When the parameters θ_1 and θ_2 and γ_1 and γ_2 are allowed to differ, estimating those same parameters by GMM is equivalent to estimating them separately by IV and correcting the standard errors for the common dependent variable. We can also estimate the system imposing the restriction $\theta_1 = \theta_2 = \theta$.[49] This leads to an implicit averaging of the unrestricted IV estimates and potentially to efficiency gains.[50] An omnibus test of the classical measurement error model is also then available as the standard GMM test of overidentifying restrictions. Since these models are overidentified, there is a

[47] In times of population growth, police force size and crime both grow mechanically. For our sample, a population-weighted regression of the growth rate in a typical crime category on the growth rates of population as measured in the UCR and ASG yields a sum of population elasticities of roughly one or even higher. Replacing the dependent variable by the growth rate in police yields a sum of the population elasticities of roughly four-fifths. The resulting positive bias in the estimated police elasticity for specifications that omit population growth is quite large economically.

[48] Some well-known papers utilizing IV strategies to address measurement error have focused on the estimated return to education among samples of twins (see Card (1999) for a review of this literature). The set of econometric issues raised in those papers is slightly different than in our context, simply because twin number is randomly assigned in those studies. In our context, the labels "UCR" and "ASG" carry substantive meaning in a way that the twin labels do not.

[49] A somewhat technical issue arises if we additionally seek to impose the restriction that $\gamma_1 = \gamma_2 = \gamma$: redundancy of moments. When we do not impose any restrictions, we have a just-identified system with $2K$ parameters and $2K$ moments, all of which are linearly independent, where X_i has $K-1$ elements. However, once the restrictions $\theta_1 = \theta_2 = \theta$ and $\gamma_1 = \gamma_2 = \gamma$ are imposed, we have K parameters and only $K+1 < 2K$ linearly independent moments. This suggests two obvious approaches to estimation: (1) impose only the restriction $\theta_1 = \theta_2 = \theta$, in which case there is no moment redundancy; or (2) impose both sets of restrictions and drop $K-1$ moments, in which case GMM will embarassingly differ depending on which set of $K-1$ moments are dropped. An involved solution to the difficulty posed by the second approach is to estimate the models by empirical likelihood (EL; see Imbens 1993, Qin and Lawless 1994, and Imbens 2002 for an introduction and references to the literature), in which case estimates are invariant to the set of moments used to identify the model. EL may also be of interest for the first approach, as the model is (slightly) overidentified. We have used both approaches, using both GMM and EL for the sake of completeness, and there is hardly any difference across the four total possibilities. In our discussion, we focus on the first approach using GMM to maintain a simple presentation and additionally report EL estimates for the sake of completeness. We note that EL computation—a thorny issue—in our application was facilitated greatly by suggestions in Guggenberger and Hahn (2005).

[50] Indeed, a very good approximation to the GMM estimate is the weighted average of the forward and reflected IV estimates, with weights of the inverse squared standard errors. In most software packages, this average will be far easier to compute than GMM. However, the standard errors for GMM are notably larger than the square root of the sum of the weights, so for inference purposes the GMM computation may be necessary.

priori merit in considering empirical likelihood (EL) estimation as well. For overidentified models, EL has been shown to have smaller higher order bias than GMM (Newey and Smith 2004) and to enjoy other advantages as well (see, for example, Imbens, Spady and Johnson 1998). However, in our data, EL estimates and standard errors are nearly identical to two-step GMM estimates, as discussed below, and we focus on GMM.

A challenge we face in implementing the above ideas is that population growth is an important confounder, yet is also likely measured with error. As discussed above, measurement error bias may not have the attenuation bias form if more than one covariate is measured with error. Measurement errors in the population variable in the UCR data are, to the best of our knowledge, not discussed in the literature, but they are likely at least as bad as the measurement errors in police. As with police, any such problems will be particularly serious when the data are measured in growth rates. A potential solution to the measurement problems with city population growth is to again use the UCR measure as an instrument for the ASG measure since both surveys report city population. However, because the two measures are measured very similarly—both are essentially forecasts based on counts from the Census—there are good reasons to believe that the errors in the two measures are not independent of one another. Accordingly, we follow an approach suggested by Lubotsky and Wittenberg (2006) and include both the UCR and the ASG population measures in our main equation of interest. We argue below, based on an empirical comparison to models including data on alternative population controls, that this procedure is sufficient to control for the confounding influence of city population growth.

VI. Data

In this section, we introduce our sample of cities and describe the main sources of information for our data. Our sample of 242 cities is drawn from all cities with more than 50,000 population each year from 1960-2010.[51] In Figure 6 we present a map of the United States highlighting the location of our sampled cities. The shading of states provides information on the number of sampled cities in each state. Our sample contains at least one city in 45 of 51 U.S. states, inclusive of the District of Columbia.[52] In addition, there are 10 states for which our sample contains only a single city. This feature of our data will become relevant in understanding parameter estimates that condition on state-by-year effects.[53]

For each city in our sample, we collect information from public data sources on a variety of different measures. We obtain data on crimes and sworn police officers from the UCR. We collect information on sworn police officers from the ASG and from another survey, described below, that is available for selected years since 1987. These three types of data are the core of our analysis, but we also collect auxiliary data on city

[51] We exclude approximately 30 cities due to extensive missing data and various data quality issues. See Data Appendix for details.
[52] Alaska, Idaho, North Dakota, Vermont and Wyoming are unrepresented in our sample.
[53] States with only a single sampled city are dropped from the analysis when unrestricted state-by-year effects are included.

revenues, police payroll, and police operating budget from the finance files of the ASG; city demographic structure from the Census Bureau; county-level economic data from the Bureau of Economic Analysis; and proxies for social disorganization from the Centers for Disease Control and the National Center for Educational Statistics. Finally, we obtain data on city population from the UCR and ASG which we supplement with data from the National Cancer Institute's Surveillance Epidemiology and End Results (SEER) dataset and some limited information on city births from the National Center for Health Statistics. We now provide more detail regarding each of these data sources. We focus our discussion on our measures of crimes, police, and population, and provide more information regarding our auxiliary data in the Data Appendix.

The UCR crime data we collect are the standard measure used in the empirical literature. These data are collected annually by the FBI. Crime measures represent the total number of offenses known to police to have occurred during the calendar year and are part of the "Return A" collection. The offenses recorded in this system are limited to the so-called index offenses—murder, forcible rape ("rape"), robbery, aggravated assault ("assault"), burglary, larceny exclusive of motor vehicle theft ("larceny"), and motor vehicle theft. Time series for each of the crime rates utilized for each of our cities are shown in Web Appendix Figure 1.

Sworn police are included in both the Law Enforcement Officers Killed or Assaulted (LEOKA) collection and the Police Employees (PE) collection and represent a snapshot as of October 31st of the given year. Because of the late date of the measurement of the number of police, it is typical to measure police in year t using the measure from year $t-1$ (cf., Levitt 1997), and we follow that convention here. Consequently, although we have data on levels 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 employment files of the ASG. The ASG is 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.[54] As with the UCR system, the ASG reports a point-in-time measure of police. For 1960-1995 the reference date is November 1 and for 1997-2010 the reference date is June 30.[55]

The UCR data provide the number of full-time sworn police officers and the total number of police officers

[54]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.

[55]No annual ASG survey was conducted in 1996. We impute data for 1996 using the average of the 1995 and 1997 levels. Other than this one missing year and occasional missing data, information on police is available in both the UCR data and ASG data for each of these cities for the entire study period.

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 (FTE) police personnel, without differentiating between sworn officers and civilian employees. In order to extend the series, we use the UCR data to generate a city- and year-specific estimate of the proportion of police personnel who are sworn officers. This was accomplished by regressing the proportion of police personnel who are sworn on city and year indicators using the 1960-1977 sample and generating a predicted value for the sworn percentage in each city-year.[56] The ASG FTE numbers before 1977 were then multiplied by the estimated proportion.

For selected analyses we also draw upon a third measure of police. This measure is drawn from two additional sources: the Law Enforcement Management and Administrative Statistics (LEMAS) series and the Census of State and Local Law Enforcement Agencies. These data, which we refer to as the LEMAS series, have been collected at regular intervals from 1987-2008. For additional details, see the Data Appendix.

The measure of city population used in the majority of crime research is from the FBI's Return A file. While this series contains observations for nearly all city-years, it is potentially contaminated by measurement error, particularly in the years immediately prior to each decennial Census. The population entries are contemporaneous; while the FBI could retroactively correct any of the population figures used in the files, it does not. We augment the city population measure from the UCR with the city population measure from the ASG, as noted. As with the UCR, the ASG population measure is noisy and often not smooth across Census year thresholds. Because of the clear errors around Census years, we smooth both series using local linear regression with a bandwidth of 5 years and the triangle kernel (Fan and Gijbels 1996).[57,58,59] Intuitively, this is akin to taking a moving average of the underlying series.

In Section IX, we use data on the cost of police and the cost of crime to derive approximate benefit-cost

[56] Time series plots of the number of full-time sworn officers according to the UCR and ASG measures for each city are provided in Web Appendix Figure 2.

[57] Appendix Figures 1A and 1B provide evidence of the importance of smoothing the raw population measures. These figures present scatterplots of the growth rate in violent and property crimes against the growth rate in the the raw and smoothed population measures from both the UCR and the ASG file. In panel A of Appendix Figure 1A, we see that a 10 percent increase in the population growth rate is associated with a 2.5 percent increase in the number of violent and property crimes. While the crime-population elasticity need not equal 1, this population elasticity is surprisingly small. Panel B plots the crime growth rate against the smoothed UCR population measure. Here, the regression slopes for violent and property crime are 0.94 and 0.84, respectively, neither of which is statistically significantly different from 1. Appendix Figure 1B reports similar results for the ASG population measure. We interpret these findings as evidence that the smoothed population measures more accurately reflect changes in city population.

[58] Below, we test empirically our notion that using both the UCR and the ASG population series adequately controls for population growth using the number of births in a city-year. This can be thought of as a proxy for the size of the population. These data are available from the National Center for Health Statistics (NCHS) for the years 1960-1993 for all 242 cities in our sample and for the years 1960-2003 for 147 of the larger cities. These data are not available electronically, but are available as a series of scanned PDF files at an NCHS website. See `http://www.cdc.gov/nchs/products/vsus.htm`. We had the data on the number of births entered by workers from Amazon's Mechanical Turk service and reviewed them for accuracy. For an introduction to this service, see `http://www.mturk.com/mturk/welcome`. We note that the manner in which we had the data entered and the screening process we undertook together persuade us that there are no data entry errors on the part of the Mechanical Turk workers.

[59] Our population imputations, as well as the raw data underneath them, are shown for each city in the sample in Web Appendix Figures 4A and 4B.

ratios, both nationally and for specific cities. We pause here to describe these data briefly, with further detail provided in the Data Appendix. Data on the cost of police are taken from the ASG Finance and ASG Employment files from 2003-2010 and are used in conjunction with other public data to estimate a fully-loaded cost of hiring an additional police officer. As noted we also make use of the crime index, or the cost-weighted sum of crimes. The correct weight to use for connecting our empirical estimates to the framework of Section II is a measure of the *ex ante* cost of crime—i.e., the dollar amount a potential crime victim would pay to reduce their probability of victimization, relative to the change in the probability. While this is in principle a person-specific concept, we follow the literature in using an estimate for a representative person. Unfortunately, estimates of the *ex ante* cost of crime are not available except for the crime of murder, where we can take advantage of the rich literature on the value of a statistical life (VSL). For other crimes, we use estimates of the *ex post* costs of crime, which are typically derived from civil jury awards. The value of these civil jury awards captures both direct costs to crime victims arising from injuries sustained during the commission of the crime, as well as losses arising from reductions in a victim's quality of life.

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 index offenses. We additionally report summary statistics for the aggregated crime categories of violent and property crime, which simply add together the relevant corresponding individual crime categories, respectively, and for the cost-weighted crime index.

Descriptive statistics are reported for a sample of 10,589 observations, the universe of data for which measures of crime, police and population are nonmissing. 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 250 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 across cities, with between city variation accounting for nearly 90 percent 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, the vast majority (91 percent) of crimes are property crimes with the most serious crimes (murder and rape) comprising less than 1 percent 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 nearly sixty percent of all property crimes. This is particularly problematic since these are the two crime categories that are generally believed to be the least comparable

across jurisdictions and time periods. Third, and turning to the growth rates, perhaps the most relevant feature of the data is that taking first differences of the series comes close to eliminating 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. Moreover, in results not shown, the first difference of a log per capita measure exhibits essentially no cross-sectional heterogeneity.

Because of the prominence of the growth rate in police for our analysis, it is of interest to examine the marginal distribution of the growth rate in police for the UCR data and the ASG data separately. Both series exhibit a mass point at zero. In the UCR data, roughly 3.9 percent of the population-weighted observations have a growth rate of zero. The corresponding figure in the ASG data is 6.1 percent. Figure 7 presents estimates of the conditional density function for the growth rate in police, conditional on not being zero.[60] The figure indicates that the growth rate in police is roughly symmetric with a range of approximately -8 to 12 percent for both series. Compared to the UCR series, the ASG data has a greater prevalence of zero growth rates and a greater prevalence of extreme growth rates. For reference, the figure also shows normal density curves. These are generally close to the local linear density estimates.

Figure 8 highlights long-run trends in crime and police for our sample of 242 cities as well as for all cities in the United States, 1960-2010. The dotted lines in Panels A present the time series for total violent crimes per 100 thousand persons while the solid lines present the time series for cost of violent crimes per person.[61] Panel B presents the same time series evidence for property crimes while Panel C presents the time series for total sworn officers. Focusing on the trends among our sample of cities, we see that regardless of whether crimes are cost-weighted, 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. For violent crime, costs in 2010 dollars per person rose from $500 in 1960 to $2,000 in 1990 before falling to less than $1,000 in 2010. For property crime, costs in 2010 dollars per person rose from less than $50 in 1960 to nearly $150 in 1990 before falling to just above $50 in 2010. Notably, our sample of cities, which covers approximately one third of the U.S. population over the 1960-2010 time period, closely parallels national trends.

Trends in policing in our sample of cities also closely track trends in policing nationally. The 1960s is a decade of strong gains in police strength, from roughly 160 officers per capita to just over 250 officers per capita, with some acceleration evident after the wave of riots in the period 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

[60] The conditional density function estimates are based on local linear density estimation (Fan and Gijbels 1996) and use a binsize of $b = 0.005$, a bandwidth of $h = 0.025$, and the Epanechnikov kernel. See McCrary (2008) for discussion of this density estimation technique and an application.

[61] This is simply the cost-weighted sum of crimes, computed for the subset of violent crimes, relative to the number of persons and is presented in units of dollars per person.

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 5).

VII. Results

A. Main Results

To estimate the police elasticity of crime correcting for measurement error, we utilize IV estimates where one noisy measure of police is an instrument for another noisy measure. The logical starting point for this analysis is then an examination of the extent to which the UCR and ASG measures of the growth rate in police are related. The first two columns of Table 3 present coefficients and standard errors from models in which the growth rate in the UCR measure is regressed on the growth rate in the ASG measure. These models correspond to what we term our forward IV regressions, in which the UCR measure is the endogenous regressor and the ASG measure is the instrument. The final two columns correspond to what we term our reflected regressions, in which the roles are reversed, with the UCR measure as the endogenous regressor and the ASG measure as the instrument.

Column (1) presents a regression of the growth rate in the UCR measure on the growth rate in the ASG measure, conditional on two measures of the growth rate in the city's population (one from the UCR file and one from the ASG file) as well as a vector of year effects. In the interest of simplicity, we refer to including both population measures as "controlling for population" throughout the paper. In column (2), we condition on state-by-year effects. These capture the effect of any potential covariate that varies over time at the state-level, such as state welfare policy, penal policy, or education policy.[62]

Consistent with the scatterplots presented in Figure 5, the coefficients reported in Table 3 are relatively small in magnitude, indicating that both the UCR measure and the ASG measure contain a great deal of noise once measured in growth rates. Referring for example, to column (1) of Table 3, we observe that, conditional on the growth rate in population, a 10 percent increase in the ASG measure is associated with only a 1.8 percent increase in the UCR measure. Column (2) shows that this result is robust to the inclusion of the full set of state-by-year effects with the coefficient value falling by roughly 10 percent from 0.18 to 0.16.

Turning to columns (3) and (4), which present the results from the reflected first stage regressions, we see that these coefficients are substantially larger in magnitude than the coefficients in columns (1) and (2). These differing magnitudes are expected since the UCR measure of police growth rates exhibits less variance than the ASG measure, and since the first stage coefficient is the covariance between the two measures, relative to

[62] In Table 3, and in subsequent tables, we report Huber-Eicker-White standard errors that are robust to heteroskedasticity. We note that the heteroskedasticity robust standard errors are extremely similar in magnitude to robust standard errors, clustered at the city level. We favor the robust standard errors as they are generally slightly larger in magnitude.

the variance of the predicting variable. As with the forward first stage regressions, results differ only slightly when the state-by-year effects are added.[63]

The F-statistic on the excluded police measure is reported below the coefficient estimates. Since the sample size only affects the scaled distribution of the IV estimator through its impact on the F-statistic, it is often said that the F-statistic is the "effective sample size" of the IV estimator (Rothenberg 1984, Section 6). Since the F-statistics we report are all above 140, standard asymptotic approximations will be highly accurate in the context of our application (cf., Bound, Jaeger and Baker 1995). That is, weak instruments are not a concern in this context.

In Table 4, we present estimates of the police elasticity. The first four columns correspond to least squares models in which we regress the growth rate in crime on the growth rate in police, conditioning on population growth and either year or state-by-year effects. The final four columns of Table 4 correspond to IV regressions that are robust to measurement errors in either of the two police series. Elasticities are estimated for each of the seven index crimes as well as three crime aggregates—violent crimes, property crimes and the cost-weighted crime index.[64]

Turning to column (1) of Table 4, we see that using the UCR measure of police officers, the police elasticity of crime is largest for murder (-0.27), motor vehicle theft (-0.19) and robbery (-0.18). All three elasticities are statistically significant at conventional significance levels. Overall, the elasticity is greater for violent crime (-0.12) than for property crime (-0.07).[65] Reflecting the large weight on murder, the cost-weighted crime elasticity is -0.21 indicating that a ten percent increase in police is associated with a two percent decline in the cost of crime to victims. Referring to column (2), the estimated elasticities are largely similar when the full set of state-by-year effects are included in the model. Here, the elasticities are generally smaller though of

[63] First stage results are extremely similar when we condition additionally on a large number of local-level control variables.

[64] An alternative to using one measure as an instrument for the other is to try to restrict attention to observations that do not contain obvious errors. For example, out of our primary sample of 10,589 observations, roughly 1,000 are either zero (potentially consistent with simply filling out the survey with a copy of the numbers for last year) or are consistent with a growth rate in excess of 20 percent in absolute value (potentially consistent with a gross error such as New York in 2003). This approach is only somewhat successful in our application. For example, the OLS regression of the growth rate of murder on the UCR measure of the growth rate in police is -0.204 in the primary sample and -0.359 in the restricted sample of 9,616 observations where the UCR measure is neither zero nor larger than 0.2 in magnitude. The IV estimate using the ASG as an instrument is -0.889, or more than twice as large as the estimate from the restricted sample (all three estimates control for two measures of population growth rates and state-by-year effects). If we perform the same analysis with the ASG measure as the endogenous regressor, the analogous three estimates are -0.143, -0.171, and -0.572.

[65] In a recent working paper, Solon, Haider and Wooldridge (2012) note that using weighted least squares will not necessarily estimate the average partial effect in the presence of unmodeled heterogeneous effects. They suggest an alternate procedure whereby population is interacted with the main effect of interest. As a robustness check, we re-estimate the population-weighted estimates in Table 4 using this formulation, centering population around the population of the city in which a typical individual lives in our sample, which we write as $\overline{w}$, and including the population weight as an additional regressor. Under a linear approximation to the heterogeneity, i.e., $\theta(W_i) = \theta(\overline{w}) + (W_i - \overline{w})\theta'(\overline{w})$, where the prime indicates differentiation, the coefficient on the growth rate in police represents the average partial effect. The estimates we obtain are similar to those reported in Table 4, but slightly less negative for the forward estimates and somewhat more negative for the reflected estimates. For example, for violent crime, we obtain forward and reflected estimates (standard errors) of -0.123 (0.042) and -0.092 (0.037) for violent crime and -0.049 (0.030) and -0.030 (0.026) for property crime, respectively. The degree of similarity between these results and those in columns (2) and (4) of Table 4 provide little evidence in favor of important unmodeled heterogeneity in our primary models, and since the effects are opposite for forward and reflected models, this does not change our pooled estimates importantly.

the same order of magnitude. Conditioning on the state-by-year effects, the largest elasticities are for murder (-0.20), robbery (-0.20), and motor vehicle theft (-0.13). Elasticities for the aggregates are -0.12 for violent crimes, -0.06 for property crimes, and -0.14 for the cost-weighted crime index.

Columns (3) and (4) report results for models in which the growth rate in crimes is regressed on the growth rate in the ASG measure of police. To our knowledge, this is the first time a city-level panel data regression of crime on the ASG measure of police has been run.[66] While the coefficients in columns (3) and (4) are smaller in magnitude, they are also more precisely estimated with significant coefficients for murder (-0.15), motor vehicle theft (-0.11), and robbery (-0.09). While the violent crime elasticity (-0.05) remains significant, the property crime elasticity (-0.03) is no longer significant. Note that the smaller magnitude of the reduced form coefficient in columns (6)-(10) is expected; returning to equation (20), we recall that the degree of attenuation is greater when the reliability ratio is smaller, and the reliability ratio of the ASG measure is worse than that of the UCR measure. These elasticities are largely similar when the full set of state-by-year effects are included in column (4) with the exception of motor vehicle theft which falls by roughly half.

Taken as a whole, least squares estimates of the elasticity of crime with respect to police point to a persistent but modest relationship between changes in police and criminal activity. Regardless of whether we rely on the UCR or ASG measure, a 10 percent increase in the size of a city's police force (which would correpond to a 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.

In the final four columns of Table 4 we report IV estimates of each crime elasticity that correct for measurement error. These estimates are typically five times larger in magnitude than those estimated via least squares.[67] Referring to column (5), the largest elasticites are those for murder (-0.80), motor vehicle theft (-0.59), robbery (-0.46) and burglary (-0.22). In addition, we report elasticities for each of the two crime aggregates of -0.29 for violent crimes and -0.15 for property crimes, though the latter is not precisely estimated. The elasticity with respect to cost-weighted crimes is -0.61. The elasticities arising from the reflected IV regressions reported in column (7) exhibit a similar pattern with elasticities for murder, motor vehicle theft and robbery of -0.74, -0.51 and -0.49, respectively. Elasticities for the crime aggregates are -0.32 for violent crimes and -0.20 for property crimes.

Finally, in columns (6) and (8), we present IV results that condition on state-by-year effects. Here we report a violent crime elasticity that is approximately -0.35 and a property crime elasticity that is approximately -0.17. Depending on whether the forward or reflected estimates are used, the cost-weighted crime elasticity is between

[66] Marvell and Moody (1996) use the ASG police measure in regressions of the growth rate in crime on the growth rate in police at the state level.

[67] A familiar result is that the IV estimate can be recovered by dividing the "reduced form" estimate of the police elasticites in Table 4 by the first stage estimate presented in Table 3. In this context, to recover the forward IV coefficients presented in columns (5) and (6) of Table 4, we would divide the reflected least squares coefficients in columns (3) and (4) by the relevant first stage coefficient.

-0.40 and -0.61. With regard to the individual crimes, elasticities are largest for murder (between -0.57 and -0.89), robbery (between -0.52 and -0.57), motor vehicle theft (between -0.30 and -0.37) and burglary (between -0.17 and -0.34). While the coefficient on robbery does not change appreciably when conditioning on state-by-year effects, coefficients on motor vehicle theft are approximately 30 to 50 percent smaller with the inclusion of the unrestricted state-by-year effects as compared to the standard first differencing specification. We interpret this as evidence in favor of the presence of substantial time-varying unobserved heterogeneity at the state-level.

In Table 5, we present GMM and EL estimates of the elasticity of crime with respect to police. These estimates combine the information from the forward and reflected IV estimates presented in Table 4. For each crime type, the table reports an elasticity conditional on population growth and state-by-year effects. As before, robust standard errors are presented in parentheses.

The table shows that two-step GMM is more precise than, but hardly differs from, one-step GMM, and that EL and GMM are nearly indistinguishable. The two-step GMM estimates are -0.67 for murder, -0.56 for robbery, -0.34 for motor vehicle theft and -0.23 for burglary. With regard to the crime aggregates, we report an elasticity of -0.34 for violent crimes, -0.17 for property crimes and -0.47 for the cost-weighted crime index. These estimates represent our best guess regarding the police elasticity and are our preferred estimates.[68]

In the bottom panel of Table 5, we report Hansen's J-test of overidentifying restrictions, which provides a measure of the discrepancy between the two parameter estimates.[69] Under the null hypothesis of classical measurement error, the test statistic has a χ^2 distribution with one degree of freedom, which has a 95 percent critical value of 3.84. Table 5 reveals that we fail to reject the null hypothesis of classical measurement errors in each of ten tests. In fact, the largest of these test statistics is just 1.86. We thus interpret the differences in

[68] An alternative approach is to specify a distributional assumption for S_i^* and the errors in the model. Under normality and mutual independence of S_i^*, u_i, v_i, and ϵ_i, imposing zero means for u_i, v_i, and ϵ_i, but allowing a non-zero mean of μ_* for S_i^*, we obtain a log likelihood function of the form

$$\begin{aligned} L_i(\beta) &= \frac{1}{2}\ln(\omega_Y) + \frac{1}{2}\ln(\omega_S) + \frac{1}{2}\ln(\omega_Z) + \frac{1}{2}\ln(\omega_*) - 2\ln(2\pi) - \frac{1}{2}\ln\left(\theta^2\omega_Y + \omega_S + \omega_Z + \omega_*\right) \\ &- \frac{1}{2}S_i^2\omega_S - \frac{1}{2}Z_i^2\omega_Z - \frac{1}{2}\mu_*^2\omega_* - \frac{1}{2}(Y_i - \gamma' X_i)^2\omega_Y + \frac{1}{2}\frac{\{\theta(Y_i - \gamma' X_i)\omega_Y + S_i\omega_S + Z_i\omega_Z + \mu_*\omega_*\}^2}{\theta^2\omega_Y + \omega_S + \omega_Z + \omega_*} \end{aligned}$$

where $\omega_j = 1/\sigma_j^2$ for $j \in \{Y, S, Z, *\}$, corresponding to ϵ_i, u_i, v_i, and S_i^*, respectively, and where $\beta = (\theta, \gamma, \omega_Y, \omega_S, \omega_Z, \omega_*, \mu_*)$. The normal likelihood approach implicitly forms an estimate of S_i^*, given by a linear combination (call it μ_i) of S_i, Z_i, and $Y_i - \gamma' X_i$, and imposes orthogonality conditions akin to those for a regression of Y_i on μ_i and X_i, but adjusted for the fact that μ_i is a generated regressor. To economize on computing time, we apply the MLE to data de-meaned by state-year, just as with EL. This approach yields point estimates (standard errors) for the 10 crime categories in Table 5 of -0.614 (0.225), -0.233 (0.212), -0.530 (0.111), -0.101 (0.122), -0.207 (0.085), -0.079 (0.064), -0.331 (0.097), -0.327 (0.085), -0.166 (0.059), and -0.433 (0.166). Between the MLE estimates and the EL estimates, we favor the EL estimates because they are consistent under a weaker set of assumptions. Between the EL and GMM estimates, we observe small enough differences that in this application the distinction seems academic.

[69] Here, the test statistic is computed via two-step GMM. The results are nearly identical when the test is computed using an EL approach. Because we are unwilling to assert that the variance matrix of the errors is spherical, the two-step GMM estimator is no longer the efficient estimator in its class, which implies that the test of over-identifying restrictions is not equal to the minimized value of the objective function. However, the proper test statistic can nonetheless be constructed; see Newey (1985) for a discussion and the proper formula for this case.

the IV coefficients reported in columns (6) and (8) of Table 4 as providing little evidence against the classical measurement error hypothesis.

Generally speaking, Hansen's J is an omnibus test. Some insight into what aspects of the classical measurement error model are being tested by Hansen's J can be obtained by examining the null hypothesis in more detail. Abstracting from covariates and using the indirect least squares interpretation of IV, the null hypothesis for Hansen's J in this context is that the two IV estimators share a probability limit, or

$$\frac{\mathbb{C}[Y_i, Z_i]}{\mathbb{C}[S_i, Z_i]} = \frac{\mathbb{C}[Y_i, S_i]}{\mathbb{C}[Z_i, S_i]} \tag{25}$$

Since the denominators for the two ratios in equation (25) are the same, the ratios can only be equal if the numerators are. Hansen's J-test is thus a convenient way to test equality of covariances, which is implied by the classical measurement error model since it implies that both covariances simplify to $\mathbb{C}[Y_i, S_i^*]$.

A second characterization of Hansen's J suggests other testing possibilities as well. Write the null hypothesis for Hansen's J as

$$0 = \frac{\mathbb{C}[Y_i, Z_i]}{\mathbb{C}[S_i, Z_i]} - \frac{\mathbb{C}[Y_i, S_i]}{\mathbb{C}[Z_i, S_i]} = \frac{\mathbb{C}[Y_i, Z_i - S_i]}{\mathbb{C}[S_i, Z_i]} \iff 0 = \frac{\mathbb{C}[Z_i - S_i, Y_i]}{\mathbb{V}[Y_i]} \tag{26}$$

where the logical equivalence follows since the ratio can only be zero if the numerator is zero. This latter characterization emphasizes that the null hypothesis for Hansen's J-test can also be understood as the requirement that the outcome not predict the difference in measures. This is implied by the classical measurement error model because the difference in measures is supposed to reflect only the difference in measurement errors, and each measurement error is supposed to be uncorrelated with the signal and the controls, which is $(A2)$, and with the structural error term, which is $(A1)$.

This is a helpful characterization because it clarifies what aspects of the classical measurement error model can and cannot be tested using Hansen's J-test. Hansen's J evidently does not test the validity of $(A3)$. This makes sense, because if $(A1)$ and $(A2)$ hold, but $(A3)$ does not, both IV estimators measure the incorrect quantity of $\mathbb{C}[Y_i, S_i^*] / (\mathbb{V}[S_i^*] + \mathbb{C}[u_i, v_i])$.

However, the analysis above suggests a natural method for testing $(A3)$ that takes advantage of the fact that for some years we possess a third measure of police from the LEMAS survey. Specifically, with 3 measures of police, we can see whether the difference between any two measures is predictable using a third measure. Since a third measure, say $\widetilde{Z}_i$, can be written as $\widetilde{Z}_i = S_i^* + \widetilde{v}_i$, where the same properties are asserted to hold for $\widetilde{v}_i$ as for u_i and v_i, $S_i - Z_i$ should be unrelated to $\widetilde{Z}_i$, $S_i - \widetilde{Z}_i$ should be unrelated to Z_i, and $Z_i - \widetilde{Z}_i$ should be unrelated to S_i. This method of testing $(A3)$ is really a joint test of $(A2)$ and $(A3)$, since each measure reflects both the signal and the measurement error. Access to a fourth measure would of course

make such an approach even more powerful, but that is infeasible in our application.

Tests along these lines are presented in Table 6. These tests partially take advantage of the fact that, for selected years, we have three measures of police taken from the UCR, ASG, and LEMAS measurement systems, as discussed in Section VI, above.[70] Each column of Table 6 pertains to regressions of the the difference between the growth rates for two police measures. Column (1) pertains to the difference between the growth rate in the UCR and ASG measures for the full 1960-2010 sample. Columns (2)-(4) use only the subsample of years for which the LEMAS measure is available, with column (2) pertaining to the UCR and ASG series, column (3) pertaining to the UCR and LEMAS series, and column (4) pertaining to the LEMAS and ASG series. Each column of Table 6 presents coefficients from a regression of the growth rate in the measurement error gap on three categories of covariates: the growth rate in each of the seven index crimes (Panel A), the growth rate in the remaining police measure (Panel B) and the growth rate in each of our two population measures (Panel C).

Referring to Panel A, using the full sample in column (1), we find little evidence of a relationship between the growth rate in the measurement errors and the growth rate in crime for any of the seven index crimes. Of the seven t-ratios, only one is above 1 in magnitude. Columns (2)-(4) provide twenty-one tests of this hypothesis using only the subsample for which the LEMAS measure was collected. Each of these three columns uses a particular difference in measures as the dependent variable: $S_i - Z_i$, $S_i - \widetilde{Z}_i$, and $Z_i - \widetilde{Z}_i$. None of the 21 t-ratios in these columns in Panel A give evidence against the restrictions of the classical measurement error model. As noted, these t-ratio tests amount to joint tests of Assumptions $(A1)$ and $(A2)$, because crime growth rates reflect both the structural error ϵ_i and the signal S_i^*.

Panel B of Table 6 presents coefficients and standard errors from a regression of a difference in police measures on the police measure not involved in the difference (e.g., $S_i - Z_i$ being regressed on $\widetilde{Z}_i$). These are tests of Assumptions $(A2)$ and $(A3)$, because under the classical measurement error model, $S_i - Z_i$ is simply a difference in measurement errors, and the third measure reflects both the signal and a third measurement error. The results in this panel may contain some slight evidence against the classical measurement error model. Specifically, one of the three tests (UCR-LEMAS) rejects at the 1 percent level and this may be consistent with mean-reverting measurement error. On the other hand, the other two tests in Panel B provide little evidence against the classical measurement error model at the 5 percent level. More broadly, the magnitude of the covariance seems to be quite small—a 10 percent increase in the growth rate of a given police measure is associated with only a 0.8 percent change in the measurement error.

In Panel C, we present results from a series of regressions of the growth rate in the measurement errors on the growth rate of each of our two population measures. In all cases, we find little evidence of a systematic

[70] For a more detailed discussion of the LEMAS data, see the Data Appendix.

relationship between measurement errors and population growth rates.

Finally, in the bottom panel of Table 6, we present p-values from a series of F-tests on the joint significance of all of the variables in predicting the growth rate in the measurement errors. For the full sample, we fail to reject (p-value = 0.83) that the measurement errors are unrelated to crime, police, and population. For the LEMAS subsample, we fail to reject the null hypothesis in all three cases (p-values = 0.25, 0.17, and 0.07).

Overall, we interpret the evidence in Table 6 as furnishing little evidence against the assumptions of the classical measurement error model. There are 39 total tests presented in Table 6; only one of these tests rejects at the 5 percent level, and no joint test is significant at the 5 percent level.[71]

However, since these tests are not commonly used in the literature, there is a question regarding how powerful these tests are at detecting violations of the classical measurement error model. To address this point, we conducted a small simulation study pegged to our sample. We generate simulated data $(Y_i, S_i, Z_i, \widetilde{Z}_i)$ as

$$Y_i = \theta S_i^* + \epsilon_i \tag{27}$$
$$S_i = \lambda_1 S_i^* + u_i \tag{28}$$
$$Z_i = \lambda_2 S_i^* + v_i \tag{29}$$
$$\widetilde{Z}_i = \lambda_3 S_i^* + \widetilde{v}_i \tag{30}$$

where the vector $(S_i^*, \epsilon_i, u_i, v_i, \widetilde{v}_i)$ is distributed jointly normal with zero mean and standard deviations calibrated to match key features of our data.[72] In the simulations, we allow five parameters of the data generating process (DGP) to vary: ρ_1, λ_1, λ_2, λ_3, and ρ_3, where ρ_1 is the (constant) correlation between u_i and ϵ_i, between v_i and ϵ_i, and between $\widetilde{v}_i$ and ϵ_i, and where ρ_3 is the (constant) correlation between u_i and v_i, between u_i and $\widetilde{v}_i$, and between v_i and $\widetilde{v}_i$. These parameters control the covariances among the elements of the vector $(S_i^*, \epsilon_i, u_i, v_i, \widetilde{v}_i)$.

Note that when $\rho_1 = \rho_3 = 0$ and $\lambda_1 = \lambda_2 = \lambda_3 = 1$, the DGP is consistent with the classical measurement error hypothesis. The parameter ρ_1 indexes the extent to which Assumption $(A1)$ is violated; λ_1, λ_2, and λ_3 index the extent to which Assumption $(A2)$ is violated; and ρ_3 indexes the extent to which Assumption $(A3)$ is violated.[73] We maintain Assumption $(A4)$ throughout. For each of 10,000 simulated data sets, we construct the tests performed in Table 6 and record whether the null hypothesis was rejected.[74] This allows

[71] While these tests are not independent, we note that a plot of the quantiles of the 39 t-ratios in Table 6 against the standard normal quantiles indicates similar distributions. Relatedly, the one-sample Kolmogorov-Smirnov test statistic (versus the standard normal distribution) has a p-value of 0.18.

[72] We set $\sigma_* = 0.044$, $\sigma_\epsilon = 0.260$, $\sigma_u = 0.047$, and $\sigma_v = 0.070$, and $\sigma_{\widetilde{v}} = 0.055$. This roughly matches the root mean squared error from IV models for the cost-weighted sum of crimes corresponding to Table 4, the first stage coefficients in Table 3 that condition on state-by-year effects, and the standard deviations of the various police measures after demeaning by state-year.

[73] Throughout, we maintain zero correlation between $(u_i, v_i, \widetilde{v}_i)$ and S_i^*. The parameters λ_j control the extent to which a composite error such as $u_i + (\lambda_1 - 1)S_i^*$ is correlated with S_i^*, where $S_i \equiv S_i^* + u_i + (\lambda_1 - 1)S_i^*$ and analogously for Z_i and $\widetilde{Z}_i$.

[74] To match our tests from Table 6, tests corresponding to column 1 are based on simulated data sets of size $n = 10,589$ and tests corresponding to columns 2 through 4 are based on $n = 1,752$.

us to examine the power of these tests against specific alternatives.

The results of this analysis are shown in Figure 9, which contains four panels. Each panel shows the impact of a departure from the classical measurement error model on the rejection rate for two tests ("Test A" and "Test B"). Test A is a t-ratio test in a bivariate regression of either $S_i - Z_i$, $S_i - \widetilde{Z}_i$, or $Z_i - \widetilde{Z}_i$ on an outcome Y_i (i.e., a test of the type discussed in Table 6, Panel A), and Test B is a t-ratio test where the covariate is not Y_i but a third measure of police (i.e., a test of the type discussed in Table 6, Panel B). The four panels in Figure 9 vary ρ_1, λ_1 and λ_2, and ρ_3, relative to the baseline of the classical measurement error model. The curves displayed are power curves corresponding to the tests which have power against the alternative being displayed. For reference, each panel also shows the average of the simulated GMM estimates. The true parameter in all scenarios is -0.5.

The figure shows that these tests have generally good power. For example, turning to Panel A, if the correlation between a measurement error and the structural error is 0.05, the rejection probability for Test A is roughly 30 percent. This is important, because even a small degree of correlation between a measurement error and the structural error leads to bias. The power of Tests A is very good for column 1, where we have our full sample size, but it is notably lower for columns 2 through 4. Our sense is that the measurement errors are unlikely to be correlated with the structural error, because we did not observe any rejections in any of the 28 tests in Panel A of Table 6, even those in column 1 where this test has quite good power.

Turning to the results in Panels B and C, we see that mean-reverting measurement error is quite likely to be detected as λ_1 or λ_2 depart from 1. Importantly, both Test A and Test B may detect mean-reverting measurement error. The curve labeled "A-any" is the power of a test which rejects at the 5 percent level if and only if one or more of the four Tests A reject at the $0.05/4 = 0.0125$ level. For a single crime outcome, this test has power approaching 20 percent for λ_1 or λ_2 equal to 0.7. We suspect that mean-reverting measurement errors in our data would thus be detected more decisively, either by rejections in columns 2, 3, and 4 of Table 6, or by at least threshold rejections for one or more crime categories.

Finally, in Panel D, we examine the power of Test B against alternatives rooted in correlated measurement errors. It is conceivable that the same core (mismeasured) information informs both the ASG and UCR measures of police. We suspect this happens rarely, as the UCR forms are filled out by employees of the police department and signed by the police chief, whereas the ASG forms are filled out by the mayor's office or city manager's office. However, it is of course true that the mayor could contact the police department for the information, in which case any measurement errors would be positively correlated. Nonetheless, Test B has power to detect correlated measurement errors. We note that to the extent the measurement errors in police are positively associated, we would understate the true effect of police on crime (cf., the expectation of the GMM estimates presented in Panel D).[75]

[75] Of course, as with any specification test, there will be a lack of power in specific directions. We have examined the power to

B. Robustness

Before turning to a discussion of the results presented above, we consider several robustness checks. The estimates in Tables 4 and 5 assume the exogeneity of police conditional on population growth and state-by-year effects. While state-by-year effects soak up important time-varying state-level variation, results will nevertheless be inconsistent if there are time-varying covariates measured at the city-level which are correlated with both growth in police and crime. In Table 1, above, we presented evidence that the growth rate in police is correlated with the growth rate in a number of city- and county-level covariates to only a limited degree. In Table 7, we explore the extent to which elasticities reported in Table 4 are robust to the inclusion of city-level covariates directly. The cost of this more direct analysis is that we are required for data availability reasons to restrict attention to the 1970-2002 subsample. The first six columns refer to estimates using the forward models while the last six refer to estimates from the reflected models.

We begin in column (1) by replicating the coefficients presented in column (6) of Table 4 for the 1970-2002 subsample of our data. These estimates condition on population growth and state-by-year effects. For the 1970-2002 subsample, the violent crime elasticity is -0.29 and the property crime elasticity is -0.26. The largest elasticites are for murder, robbery, and burglary (-1.1, -0.55, and -0.41, respectively). The elasticity for the cost-weighted crime index is -0.79. In column (2) we add a series of economic covariates that capture the growth rate in personal income and total employment as well as revenue and employment in four leading industrial sector (construction, manufacturing, wholesale trade and retail trade). We also include a variable that captures each city's public expenditures exclusive of police to capture the impact of all other municipal spending. In column (3), we include the lags of each of these variables to capture a potentially lagged response of crime to local macroeconomic conditions. In column (4), we capture changes in a city's demographic composition by adding control variables for the population share of sixteen age-gender-race groups within each city. In order to control flexibly for the effect of changes in a city's composition, in column (5) we add polynomials (to the second degree) and interactions for each of the demographic subgroups. Finally, in column (6) we add city-specific linear time trends that would capture long-standing crime trends that are independent of growth in police.

Referring to the forward models, it is apparent that the estimated elasticites change very little with the

detect local departures from the classical measurement error model, but one could instead imagine joint departures, and our tests will have little power against some of these joint alternatives. For example, if $(A2)$ is violated, but the λ_j parameters differ from 1 by the exact same amount, then the rejection rate for both Test A and Test B will be 5 percent. Similarly, if $(A1)$ is violated, but the measurement errors have the exact same covariance with the structural error, then the rejection rate for Test A will be 5 percent regardless of how large is the covariance with the structural error. This underscores, in our minds, the importance of validation studies based on administrative, rather than survey, data. We note that progress in labor supply and in the return to education, for example, occurred after several decades of hard work spent documenting fundamental properties of the measurement errors in the standard data sources on hours, earnings, and education. For a review of some of this literature, see Bound et al. (2001). Much more research along these lines is needed to obtain a clear picture of the proper inferences to be drawn from the crime literature.

inclusion of the controls. Referring, for example, to the cost-weighted crime index, the estimated elasticity moves from -0.79 when conditioning only on population and state-by-year effects to 0.76 when economic covariates are included. Conditioning also on the lags of the economic covariates brings the estimated elasticity up to -0.82 while controlling extensively for demographics brings the elasticity back to -0.79. When time trends are included, the elasticity increase to -0.82, just 2.5 percent higher than the original elasticity. A similar pattern holds for each of the other crime types with the largest change from column (1) to column (6) occuring for assault and larceny, both of which are imprecisely estimated. Referring to columns (7)-(12), the reflected estimates follow a similar pattern with the exception of murder which appears to be somewhat sensitive to the inclusion of lagegd economic covariates, a result which drives the difference between columns (7) and (12) for violent crimes and for the cost-weighted crime index. While the estimated murder coefficient changes with the inclusion of controls, it is nevertheless similar in magnitude to the estimate that conditions only on the state-by-year effects.[76]

As discussed, the elasticities reported in the paper condition on two measures of city population growth, taken from the UCR and ASG data systems, respectively. The motivation for including both measures is that we are persuaded there is measurement error in each series individually. As discussed in Section VI, above, it is necessary to smooth both series to circumvent clear measurement problems around Census years. However, even after smoothing, it may not be the case that true population growth rates can be represented as a linear combination of the growth rates of the UCR and ASG series. To assess the extent to which measurement error in population represents a source of bias for the estimated police elasticities, we take advantage of two additional proxies for a city's population: (1) population data from the Surveillance Epidemiology and End Results (SEER) dataset which has been compiled by the National Cancer Institute to track disease incidence and (2) the number of births in a city, drawn from the National Center for Health Statistics at the Centers for Disease Control. Births are correlated with population because, other things equal, the more individuals who are living in a city, the more children will be born. Moreover, unlike the UCR and ASG series on population, there is close to no measurement error in the number of births in a city, since births are estimated from no worse than 50 percent samples of birth certificates over the sample period and since birth certificates cover an estimated 99 percent of births in the U.S. over this time period.[77] Consequently, including the growth rate in births as a covariate should pick up on any remaining association between true population growth rates and crime growth rates.

Appendix Table 1 shows the sensitivity of our resulting estimates to the inclusion of SEER population

[76] We also consider whether the estimates are robust to the exclusion of the two largest cities in the sample–New York and Los Angeles–as well as whether the results are robust to the exclusion of cities with various data problems, namely those cities which have merged with their respective counties (e.g., Jacksonville, Nashville, Charlotte and Louisville) and cities which have been recently found to have misreported data to the FBI's Uniform Crime Reporting System (e.g., Milwaukee). When these cities are excluded from the sample, the estimates are nearly identical to those reported in Table 5.

[77] In the early 1970s, the NCHS transitioned to 100 percent samples of birth certificates.

data and the births data. For the years and cities for which data on births are available, the estimates change very little when the growth rate in births is added to the model. For example, referring to Panel A for which data on all cities in the sample are available for the 1960-1993 subsample, we see that pooled estimates of each of the crime elasticites are extremely similar with and without the inclusion of the births measure. For example, the violent crime elasticity moves from -0.16 to -0.18 while the property crime elasticity changes only in the third decimal place.

Panel B which provides estimates for a sample of 147 of our cities using the 1960-2003 window. For that sample, the largest impacts are on murder and motor vehicle theft where the elasticity changes from -0.548 to -0.565 and from -0.346 to -0.369, respectively, when births are included. Finally, in Panel C, we test the sensitivity of the pooled elasticities to inclusion of the SEER population data over the 1970-2008 time period. Again, the estimates are extremely similar when the SEER population measure is added, with motor vehicle theft showing the largest change (-0.320 versus -0.340). We interpret these findings as indicating that our estimates are not importantly compromised by measurement error in population. Indeed, to the extent that our estimates do change when additional population controls are added, they tend to get larger in magnitude with additional population controls, suggesting that our full sample estimates may be conservative.[78]

VIII. Discussion

The 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. Pooled estimates in Table 5 represent our best guess regarding crime-specific police elasticities. Pooling via GMM or EL, we obtain precisely estimated elasticities of -0.34 for violent crimes and -0.17 for property crimes, with especially large elasticities for murder (-0.67), robbery (-0.56), motor vehicle theft (-0.34) and burglary (-0.23).

In this section, we contextualize these findings by comparing our reported elasticities to those in the prior literature. Table 8 presents selected police elasticities from eight recent papers that use U.S. data. Each of the papers explicitly seeks to correct for simultaneity bias, for which our estimates do not adjust. While these papers do not discuss the possibility of measurement error in police or in population, an IV estimator using exogenous instruments will correct for both simultaneity bias and measurement error bias under the classical measurement error hypothesis.

[78] A final issue that is worth mentioning is the possibility of displacement—an increase in policing in one jurisdiction might displace crime to a nearby jurisdiction. If this is the case, then our approach will tend to overestimate the social value of policing, since part of the apparent crime reduction associated with increasing policing would stem from a simple reshuffling of criminal activity. Appendix Table 2 addresses this concern. In this table, we contrast the GMM estimates presented in Table 5 with estimates based on aggregating up to the MSA level. The estimates in Table 2 indicate that there is not enough statistical power in these data to distinguish the estimates at the city-level from those at the MSA-level. If anything, the estimates at the MSA are larger, rather than smaller, than the estimates at the city level, which is the opposite of the expected pattern if displacement were a first-order phenomenon in these data.

Looking across the estimates from these papers in Table 8, four tendencies are evident. First, the estimates are generally negative. Some of the estimates are zero (e.g., Levitt (1997) for property crime), but virtually none are positive.[79] Second, the general tendency of these estimates is similar to, or perhaps slightly larger than, that of our own estimates. For example, the average of the murder elasticities is 1.18 in magnitude.[80] This is similar to the magnitude of our own estimated murder elasticity (roughly 1) when we replace our preferred state-by-year effects specification with a simpler year effects specification, which is more similar to most of the research designs employed in the previous literature. Similarly, the average of the elasticities for robbery, burglary, and auto theft is approximately 0.79, 0.35, and 0.77 in magnitude, respectively. Controlling for year effects, our estimates of the same quantities are roughly 0.50, 0.17, and 0.50, respectively. The differences between these estimates would likely not rise to the level of statistical significance, but the general tendency is for our estimates to be slightly smaller in magnitude. Some of this discrepancy stems from utilization of different time periods. For example, when we restrict our analysis to the years analyzed by Evans and Owens (2007), namely 1990 to 2001, our estimated elasticities are -0.83 and -0.31, for violent and property crimes, respectively. These are extremely close in magnitude to those in Evans and Owens (2007) (-0.99 for violent crimes and -0.26 for property crimes). Given the magnitude of the standard errors, the differences in estimates are likely consistent with the hypothesis of sampling volatility.

Third, there is a general tendency to find that police have a larger protective effect on violent crimes than on property crimes. This is a surprising finding if we conceive of the estimated effect of police on crime as being about deterrence. However, as noted in the introduction, the effect of police on crime operates through both a deterrence and an incapacitation channel. Moreover, police departments actively focus their resources on the incapacitation of individuals posing the greatest risk to society, which may make the incapacitation channel particularly important.

Fourth, the estimated elasticities tend to be quite imprecise, with estimated standard errors ranging from 0.2 to 0.7 for violent crimes and 0.2 to 0.9 for property crimes. As a result, it is often the case that even large elasticites (on the order of 1) cannot be rejected as being different from zero. Similarly, the cross-crime pattern of the elasticities is difficult to discern. For example, one of the more precise studies is that of Evans and Owens (2007). In that study, the magnitude of the estimated elasticities and standard errors suggest that it would be difficult to reject tests of the equality of various crime-specific elasticities. As a result, though

[79] The pooled estimates in Levitt (1997) are in error due to a mistake in the use of weights (McCrary 2002). The numbers in Table 8 listed as Levitt (1997) are actually the corrected numbers reported in McCrary (2002) that use Levitt's mayoral election year series. The numbers in Table 8 listed as McCrary (2002) are the numbers reported in McCrary (2002) that use McCrary's mayoral election year series.

[80] To avoid double-counting research designs, we count the average of the estimates from Levitt (1997) and McCrary (2002) as a single entry.

the general pattern of the elasticities is suggestive, it is difficult to draw inferences about even the most basic policy questions such as the relative effectiveness of police in reducing violent versus property crimes.

The elasticities we report in this research are estimated with considerably greater precision, with standard errors that are between one-quarter and one-half the size of those reported by Evans and Owens (2007) and up to an order of magnitude smaller than those reported in other papers. The result is that we are able to generate considerably stronger inferences regarding the cross-crime pattern of the elasticities.

In Table 9, we formalize this idea and test the equality of all pairs of individual crime elasticities. The table reports p-values from each of these tests, operationalized by stacking up crime categories into a broader GMM system. For a given row, a given column reports the p-value associated with a test of the equality of the coefficient for the crime category on the row and the coefficient for the crime category on the column. The pattern of the resulting p-values suggests that we can be confident that police reduce murder to a greater extent than assault and larceny and perhaps burglary. Likewise, the effect of police on robbery is greater than it is for assault, burglary and larceny and the effect of police on motor vehicle theft is greater than the effect of police on larceny. Referring to the aggregates, the elasticities for murder and robbery are greater than the property crime elasticity. We can also reject, at the 10 percent level, the equality of the violent and property crime elasticities. Despite a dominant pattern in the literature that suggests that the effect of police on crime is most concentrated among violent crimes, to our knowledge, this is the first paper that offers more than suggestive evidence on this point.

Whether our estimates are similar to or different from those in the preceding literature is important for getting the magnitude of police elasticities right, but is also interesting because it speaks to the broader issue of whether simple regression techniques are compromised by simultaneity bias. If our estimates are deemed to be similar to those reported in prior research, then our research implies a smaller role for simultaneity than has been suggested by prior studies.

Overall, our suspicion is that the estimates we have presented here are compromised somewhat by simultaneity bias, despite our best efforts to control for unobserved heterogeneity. The sign of the bias, as criminologists and economists have argued for several decades now, is likely positive, leading our approach to underestimate of the magnitude of the policing elasticity. Thus, the correct magnitude is likely at least as large as what our results indicate. As we turn in the next section to connecting our estimates to the state's optimal level of policing, these considerations should be kept in mind, as they suggest that our policy conclusions may well be conservative.

IX. Cost-Benefit Analysis

A. National Estimates

The results presented in Table 5 represent our best estimate of the elasticity of each type of crime with respect to police. These elasticities allow us to predict the change in reported crimes expected to arise from a given percent increase in the size of a city's police force. However, in allocating scarce resources among a large number of critical public services a potentially more relevant parameter is the ratio of the benefits to the costs of hiring additional police personnel. In Section II, we established that even in the presence of investment in precautions with externalities, the state's optimal choice of policing can be characterized by the the parameter θ, which represents the elasticity of the cost of crime with respect to police, holding precautions fixed. In particular, the rule-of-thumb outlined in Section II is that hiring police improves welfare when

$$|\theta| / \frac{wS}{nC} \equiv \kappa > 1 \tag{31}$$

In this section, we use the GMM approach described above to estimate the ratio of the benefits (as proxied by averted costs to potential victims) to the costs of police.

For a VSL of $7 million, we estimate a police elasticity of the cost of crime of -0.47 (standard error = 0.17). This elasticity estimate is based on a model including state-by-year effects and two controls for population, analogous to our preferred specification in Table 5. Scaling this elasticity estimate by the ratio of mean victimization costs to mean police expenditures produces an estimate of the 2010 social dollars saved from increasing spending on police by one dollar, or the benefit-cost ratio (BCR).[81] Varying the VSL from $1 to $28 million, our GMM estimate of the police elasticity of the cost of crime ranges from -0.32 (standard error = 0.09) to -0.55 (standard error = 0.26).

An unfortunate feature of these types of estimates is that benefit-cost calculations are often extremely sensitive to the monetized value of an averted murder.[82] Figure 10 provides a visual presentation of the findings from this analysis. The figure plots the BCR that follows from this GMM procedure on the vertical axis against possible VSL estimates on the horizontal axis. The change in the BCR is linear with respect to the VSL employed since the VSL is simply the factor by which murders are scaled in the analysis. The

[81] To obtain the cost of increasing policing, we take the average of the UCR and ASG counts and scale it by $130,000, an estimate of the fully-loaded cost of a police officer in 2010. As discussed in detail in Section VI, above, this estimate is based on data on the operating budget per officer, i.e., the ratio of the operating budget for the police department, in 2010 dollars, to the number of sworn officers. These figures are taken from the ASG Finance and ASG Employment files for 2003-2010. We use multiple years to get a clear picture of the finances for a department. These figures fluctuate a good deal from year to year. We use the city-specific median over time, and then compute a 2010 population weighted average of the city-specific medians; this weighted average is about $130,000. Our estimate of the cost of hiring additional officers is notably higher than those used in some of the literature (e.g., Evans and Owens (2007) use $55,000).

[82] In the literature, it is not uncommon for the results of a benefit-cost analysis of a given policy to depend on the researcher's choice between two reasonable alternative values of the cost of a murder.

BCR ranges from approximately 0.4 at a VSL of $1 million to approximately 6.0 at a VSL of $28 million. To further narrow down these estimates, we superimpose a kernel density estimate of the density of the 64 VSL estimates for the U.S. While the estimates vary considerably, approximately 80 percent of the data lies below $10 million which is associated with an approximate BCR of 2. At $7 million, the mean value of the VSL, the resulting BCR is 1.63, indicating that, in a typical U.S. city, an additional dollar allocated towards policing is predicted to save $1.63 in costs to crime victims. This would be consistent with classical notions of the underprovision of public goods (Samuelson 1954). On the other hand, as noted there is substantial ambiguity regarding VSL estimates. The estimated VSL from Ashenfelter and Greenstone (2004) implies a BCR of roughly $0.80, indicating substantial overpolicing.[83] If we revert to estimates from Cohen and Piquero (2008), the BCR is just below 1, suggesting that the political process may have arrived at the social optimum.

B. City-specific Estimates

The national benefit-cost ratios reported above answer the question: For a typical U.S. city in a typical year in our sample, what is the dollar value of crime reduction obtained by increasing spending on police by one dollar? A somewhat different question pertains to specific U.S. cities in 2010. For example, for many years Oakland, California, has had fewer police per capita than other cities, despite a relatively high crime rate. Journalists often note this fact and query whether Oakland should hire additional police (e.g., McKinley 2009). We now seek to answer the question: For specific U.S. cities in 2010, given that the value of a statistical life is $7 million, what is the dollar value of crime reduction obtained by increasing spending on police by one dollar? Despite great interest in such issues, we emphasize that investigations along these lines are necessarily somewhat speculative, as data for individual cities are less reliable than data for a few hundred cities, taken as a whole. Indeed, such an analysis may be heroic, as it involves assuming that the police elasticity of crime is constant across cities, across time, and across possible adjustments to the size of the police force.

These limitations aside, we believe it is nonetheless of interest to characterize the heterogeneity across cities in a benefit-cost ratio, as a function of the prevalence of crime, the number of officers, and the cost to the city of hiring officers. There is extraordinary heterogeneity across cities in the prevalence of crime. For example, the cost-weighted sum of crimes per capita in the most dangerous city in our sample (Gary, Indiana), is nearly 40 times that of the safest city in our sample (Waltham, Massachusetts). Similarly, cities vary quite a lot in terms of the number of officers and the expense per officer. We suspect that our approach, while flawed, captures much of the variation from city to city in the true benefit-cost ratio.

[83] In fact, while estimates of the VSL arising from the study of individuals' labor market behavior tend to yield large values (on average, $9.5 million), estimates of the VSL arising from the study of non-labor market behaviors tend to yield much smaller values (on average, $4 million).

Table 10 presents the bottom and top 30 cities in our sample, based on our estimated benefit-cost ratio. The benefit-cost ratio, reported in the final column (column 9) of the table, is rooted in our overall estimated police elasticity of the crime index for all cities and all years, but is scaled by the mean cost of crime in the city from 2003-2010 relative to the product of the average number of officers from 2003-2010 and the estimated cost of hiring an additional officer (see Section VI and the Data Appendix for discussion of the construction of this variable). The mean cost of crime is reported in per capita terms in the table (column 6), as is the estimated cost per officer (column 8). Column 5 of the table reports per capita income in the city, and column 7 reports the cost of crime relative to income per capita ("fraction income at risk"). This last column encourages thinking of crime as a tax on the populace. For reference, we additionally report city population as of 2010 and the city's poverty rate.

The 30 cities listed in the top half of the table have the lowest benefit-cost ratios among our 242 cities, while the 30 cities listed in the bottom half have the highest benefit-cost ratios. For example, for Sunnyvale, California, we estimate that every dollar spent on policing yields only 20 cents in benefits in terms of crime reduction. In contrast, we estimate that every dollar spent on policing in Gary, Indiana, yields $14 in benefits in terms of crime reduction. The population weighted average of the city-specific benefit-cost ratio is about $1.78, or slightly higher than our estimate for the overall sample reported above.

Scanning down the table, we see several interesting patterns. Cities with low benefit-cost ratios are small, low-poverty cities, with low levels of crime and low to moderate levels of policing. Police officers in these cities often enjoy high salaries and benefits, leading to high employer costs per officer. Sunnyvale and Berkeley, for example, both have costs per officer of roughly $280,000.

Cities with high benefit-cost ratios are suprisingly representative of our broader sample in some regards. For example, cities with high benefit-cost ratios include both low and high population cities. Also, these cities sometimes have low policing levels (e.g., Oakland and Richmond, California, have 180 and 160 sworn officers per 100,000 population, respectively) and sometimes have high policing levels (e.g., Baltimore and Camden have 480 and 510 sworn officers per 100,000 population). On the other hand, cities with high benefit-cost ratios have high poverty rates and extraordinarily high crime rates—generally an order of magnitude higher than cities with low benefit-cost ratios. Crime costs residents in these cities anywhere from 5 percent of their annual income (Mobile, Alabama) to 34 percent of their annual income (Camden). In contrast, for cities with low benefit-cost ratios, crime costs resident at most 1 percent of their annual income.

Another interesting pattern is that California cities are prevalent among the lowest benefit-cost ratio cities, with 13 out of 30 spots, but also are represented among the highest benefit-cost ratio cities (Oakland, Richmond, and San Bernadino). The estimated cost per officer is very high among California cities generally. High costs per officer keeps several high-crime California cities from being among the highest benefit-cost

ratio cities (Sacramento, Vallejo). Richmond's high estimated costs ($240,000) are particularly remarkable given its high poverty rate and low per capita income; wealthy Palo Alto's estimated cost per officer is slightly lower than that for Richmond. Factoring in base salary, overtime, and lump-sum payments, a police officer in Richmond makes an average of $148,000, or six times what a city resident makes.[84] It is worth noting that the estimated cost per officer does not include unfunded pension liabilities for the city, which is an ongoing issue for many cities that may lead the figures for officer expense to be understated (Gralla 2012).

As noted above, there is substantial ambiguity regarding some of the inputs to the city-specific benefit-cost ratios. However, we note that in many cases the benefit-cost ratios are sufficiently extreme that only gross errors in the inputs would alter the conclusion that the benefit-cost ratio was on the wrong side of 1. Our sense is that cities with benefit-cost ratios between 0.5 and 1.5 may well be near the optimal level of policing, but that the many cities outside this band are unlikely to be.

C. Police Incapacitation Effects and the Benefit-Cost Ratio

The estimates in the preceeding sub-sections are valid under the assumption that either (i) the decline in crime resulting from increased police is entirely due to deterrence or that (ii) the cost of incarcerating offenders is fixed in the short run so that the downstream cost of incapacitating offenders need not be counted as a cost of increased police personnel. Here, we re-frame the national benefit-cost analysis, treating the expected increase in incarceration resulting from more police as an additional cost of hiring a new officer. Because we are interested in the short-run costs and benefits of new police hiring, we count only the costs of incarceration that are borne in the first year. We begin with an estimate of the number of arrests per officer. Using our sample of 242 cities, an average officer made between 18.7 and 20.2 arrests in 2010, depending on whether the UCR or ASG officer count is employed.[85] Next, we employ an estimate of the conditional probability of a conviction given an arrest. In 2010, there were 13,120,947 arrests made by police officers in the United States while there were 1,132,290 convictions in state courts and another 81,934 convictions in federal courts. Dividing convictions by arrests yields an estimated conditional probability of a conviction of 9.3 percent. Of defendants sentenced in state courts, 40 percent were sentenced to state prison (with a mean sentence length of 4 years and 11 months), 28 percent were sentenced to a term in local jail (with a mean sentence of 6 months) and the remaining 32 percent were sentenced to a term of probation or an alternate penalty that did not involve incarceration. On average, offenders serve approximately 55 percent of their sentence. Thus, in steady state, a typical officer is associated

[84] Average take-home pay for officers based off of data pulled from the *San Jose Mercury News*. See footnote **??**.

[85] The working assumption here is that a new officer's productivity, and the lost productivity associated with laying off an officer, can be approximated using the productivity of an average officer.

with 20 new arrests, 1.85 new convictions and 0.87 incarceration-years.[86] At an incarceration cost of $25,000 per year, each new officer is thus associated with $21,738 in additional costs. Augmenting the salary figure with this estimate yields a benefit-cost estimate of $1.40 using the $7 million estimate of the value of a statistical life.

X. Conclusion

In this paper, we have presented estimates of the elasticity of crime with respect to police for index offenses: murder, rape, robbery, aggravated assault, burglary, larceny excluding motor vehicle theft, and motor vehicle theft. These estimates are based on annual data on year-over-year growth rates in crime and police in a panel data set of 242 cities observed from 1960-2010. Our primary specifications model growth rates in crime as a function of the growth rate in the number of sworn officers, population growth and a full set of state-by-year effects which render our estimates robust to arbitrary changes in state policy, such as penal policy, and other factors affecting cities in the same state similarly. In auxiliary regressions we show that our results are also robust to a wide array of local-level confounders.

We argue that a central problem in estimating the police elasticity of crime is measurement error in the number of police. These measurement errors are unimportant for specifications involving the level or log of police, but are first-order for specifications involving growth rates or city fixed effects. Problems with measurement errors in police have gone unaddressed in the crime literature, but add to a long list of literatures where measurement errors have been shown to be important (Ashenfelter and Krueger 1994, Bound, Brown, Duncan and Rodgers 1994, Kim and Solon 2005, Bollinger 2003, Black and Smith 2006, Edlin and Karaca-Mandic 2006, Nunn 2008).

A recent literature has focused on quasi-experimental estimates of the police elasticity of crime, and many of these estimates solve both for problems with measurement errors in police and for simultaneity bias (for a review, see Levitt and Miles 2006). We add to this literature by addressing the measurement error bias directly, utilizing independent measurements of the number of police departments collected annually by the Census Bureau. Correcting for measurement error increases least squares estimates of the police elasticity of crime by roughly a factor of 5 and results in estimates just slightly smaller than those estimated in the previous quasi-experimental literature. This may suggest a smaller role for simultaneity bias than has previously been emphasized. An advantage of bracketing the issue of simultaneity bias, and focusing instead on correcting for measurement errors, is that we obtain estimates that are demonstrably more precise than those from the previous literature and arguably conservative in magnitude. The received wisdom in this literature, going back to Nagin (1978) and before, is that police departments hire officers during and perhaps even in anticipation

[86] Regarding pre-trial detention, since the average length of time between arrest and sentence is approximately 5 months, we operate under the assumption that if an arrestee does not receive bail, their expected sentence amounts to time served.

of crime waves, leading the police elasticity of crime to be too small in magnitude.

Our best guess regarding the police elasticity of crime is -0.34 for violent crime and -0.17 for property crime. Crime categories where police seem to be most effective are murder, robbery, burglary, and motor vehicle theft, with estimated elasticities of -0.67, -0.56, -0.23, and -0.34, with standard errors of roughly 0.2 for murder and 0.1 for other crimes. The elasticity of the cost-weighted crime index is -0.47 with a standard error of 0.17.

To assess whether these magnitudes are small or large, we introduced a framework for assessing whether policing levels are socially desirable. This framework delivers a rule-of-thumb for optimal policing that pertains to a social planner unwilling to monitor precautions undertaken by individuals, where the social planner pays for policing using lump-sum taxes. This analysis, together with our empirical estimates of the police elasticity of the cost of crime, is suggestive of substantial underpolicing. However, as with many analyses pertaining to public investments and safety, our normative conclusions turn to a great extent on the price society is willing to pay for reductions in the probability of fatalities, or the value of a statistical life (VSL).

Despite the ambiguity regarding the appropriate quantity for the VSL, we note that federal and state regulatory authorities frequently undertake investments where the same tradeoff is confronted. Pegging policing investments to the typical federal standard suggests that society would receive approximately $1.60 in benefits from an additional 2010 dollar spent on policing. This estimate is likely conservative if simultaneity bias in the police elasticity of crime is important.

This policy conclusion is most strongly justified if externalities in precautions are unimportant or if there is little scope for policing crowding out precautions. Precautions may however be important. Nonetheless, the framework we have introduced allows us to be clear about the assumptions supporting our policy conclusions in such a scenario. If precautions have negative externalities on average, with one individual's precaution displacing criminal activity to her neighbors, then the policy conclusion is conservative. If precautions have positive externalities on average, with one individual's precaution protecting her neighbors against criminal activity, then this channel has to be fully one-third as large as the direct effect of police on crime in order to overturn the policy conclusion.

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Figure 1. Private Precaution Reaction Functions: Two Person Case, Low and High Public Policing

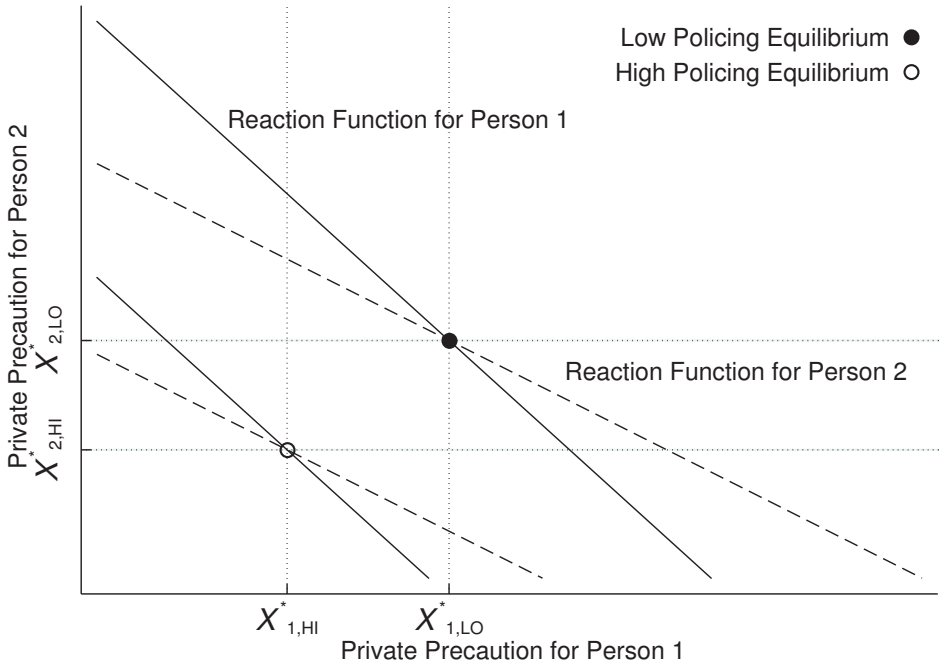
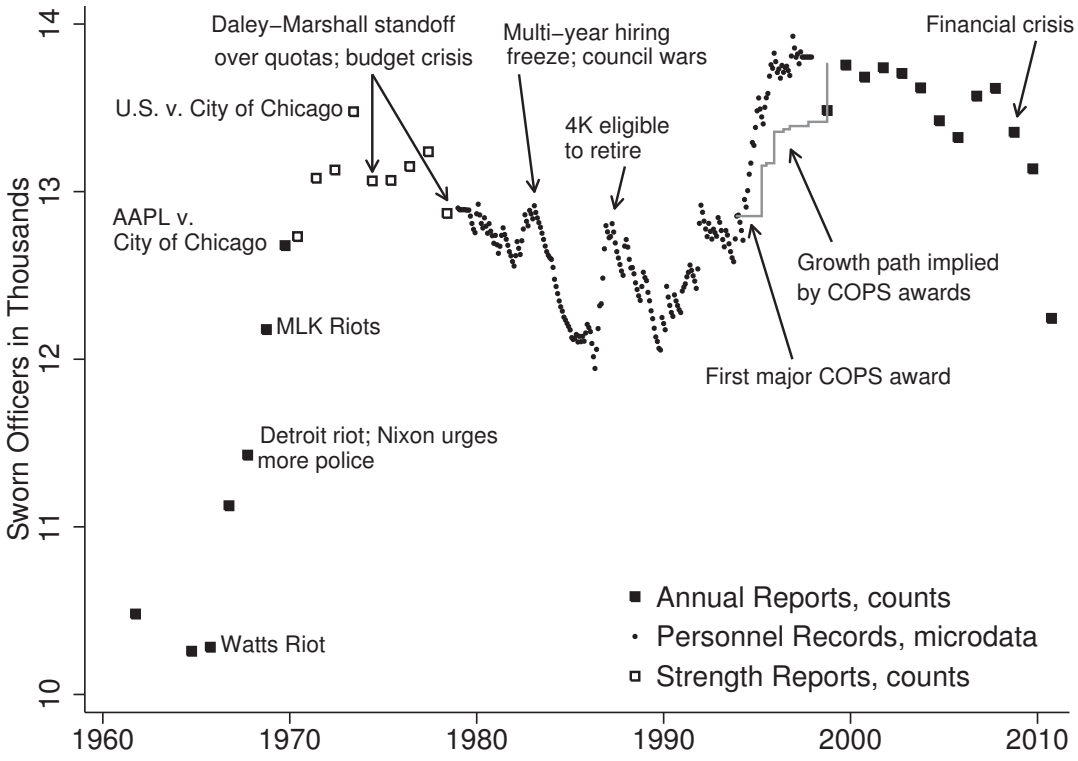
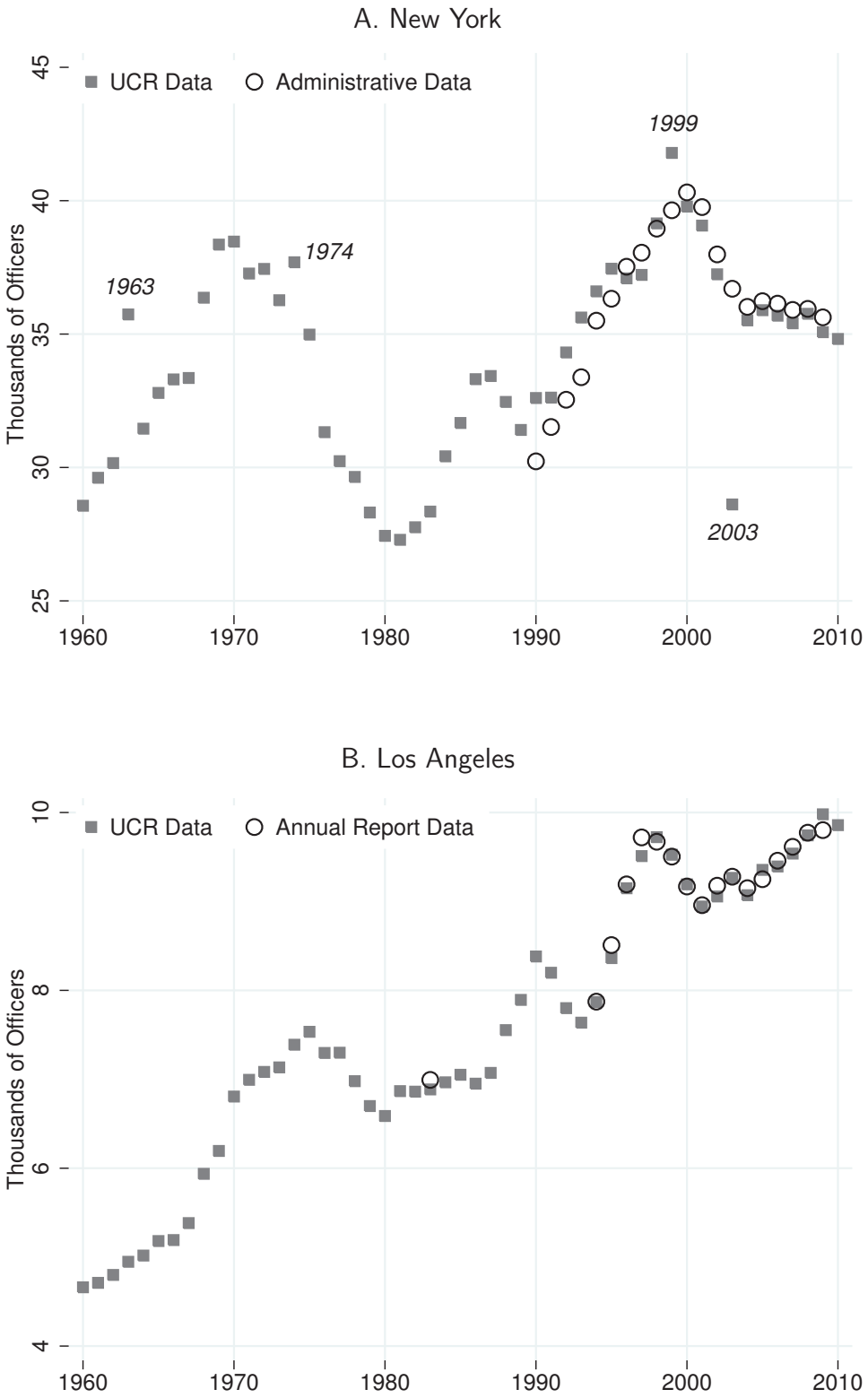


Figure 2. Police Hiring in Chicago, 1960-2010

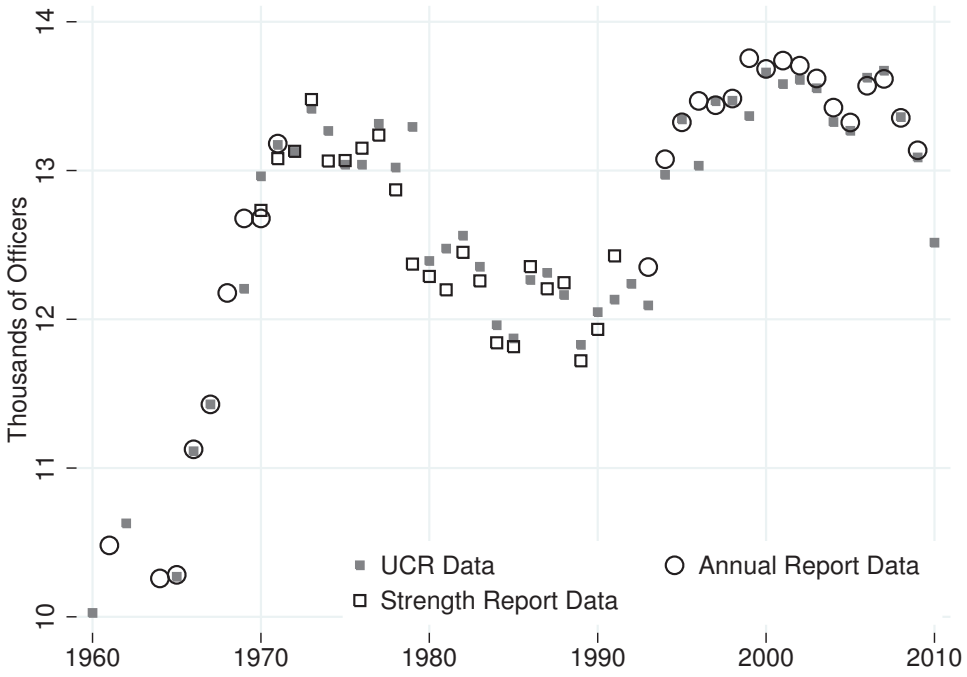


Note: See text for details.

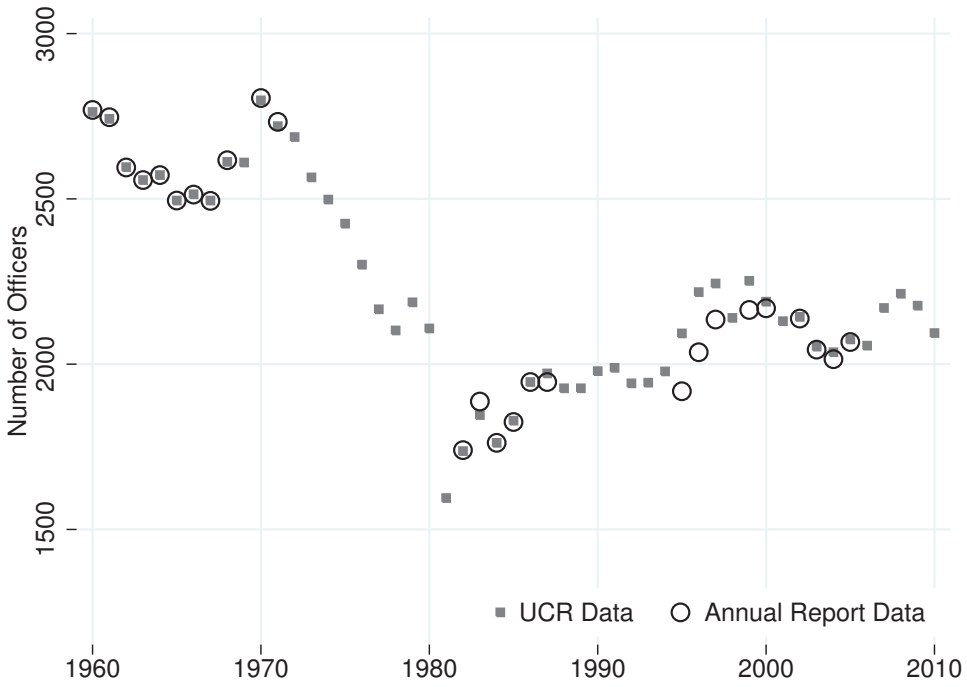
Figure 3. Sworn Officers in Five Cities:
the Uniform Crime Reports and Direct Measures from Departments

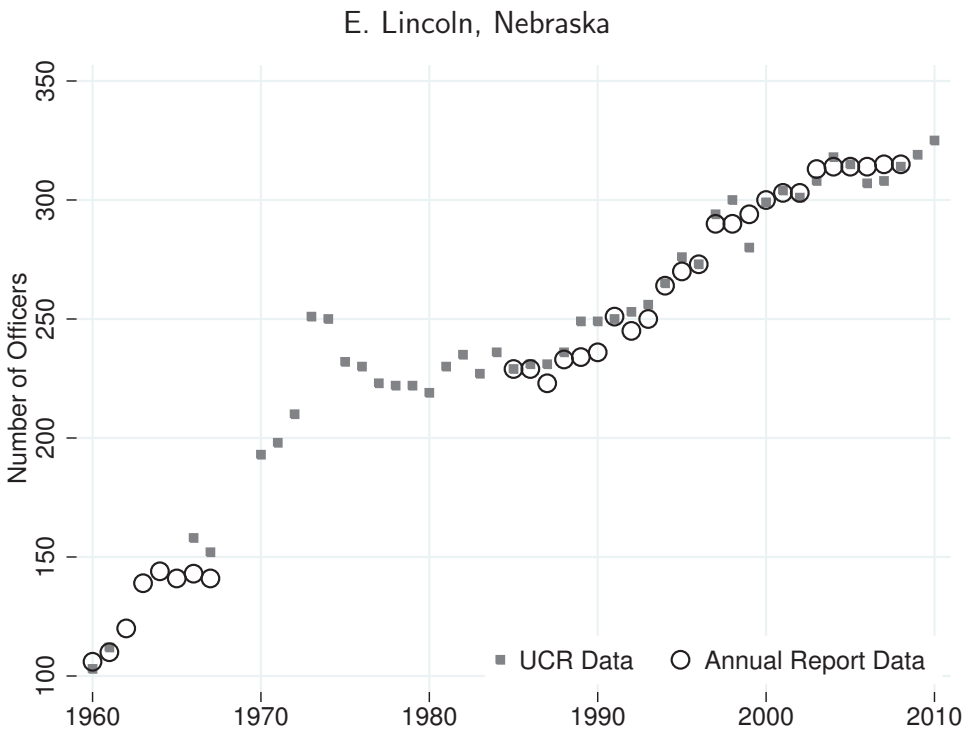


C. Chicago



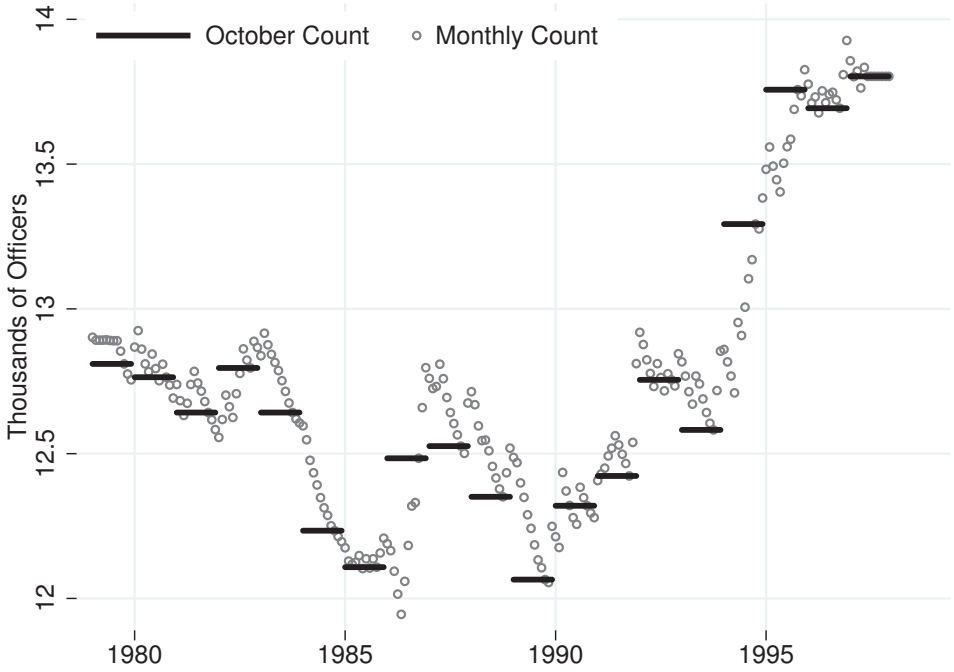
D. Boston





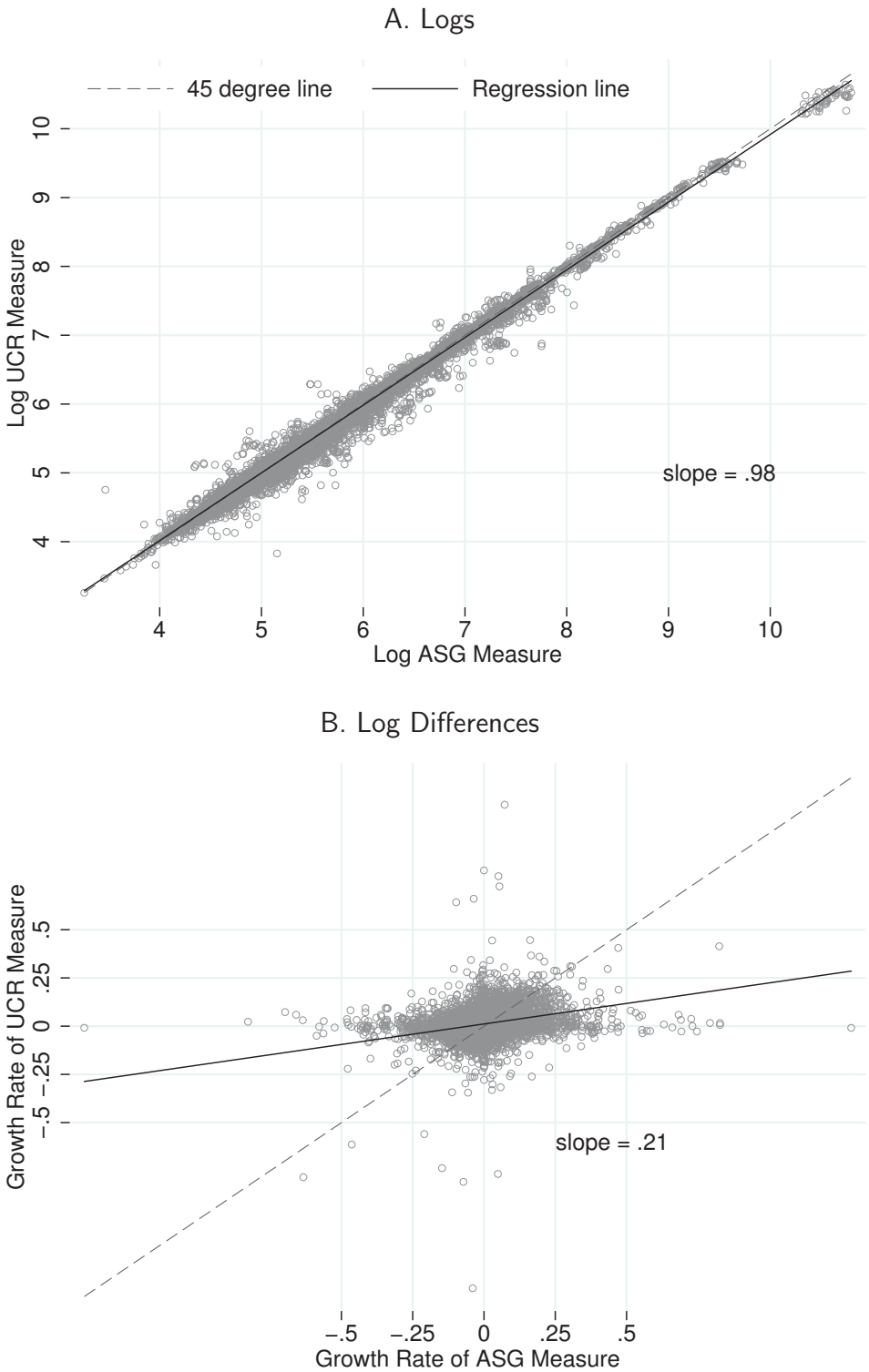
Note: In panel A, numbers for 1960-1994 are adjusted to account for the 1995 merger of NYPD with housing and transit police. See Data Appendix for details.

FIGURE 4. SWORN OFFICERS IN CHICAGO 1979-1997, BY MONTH



Note: See text for details.

Figure 5. 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 6. Location of Cities in Sample

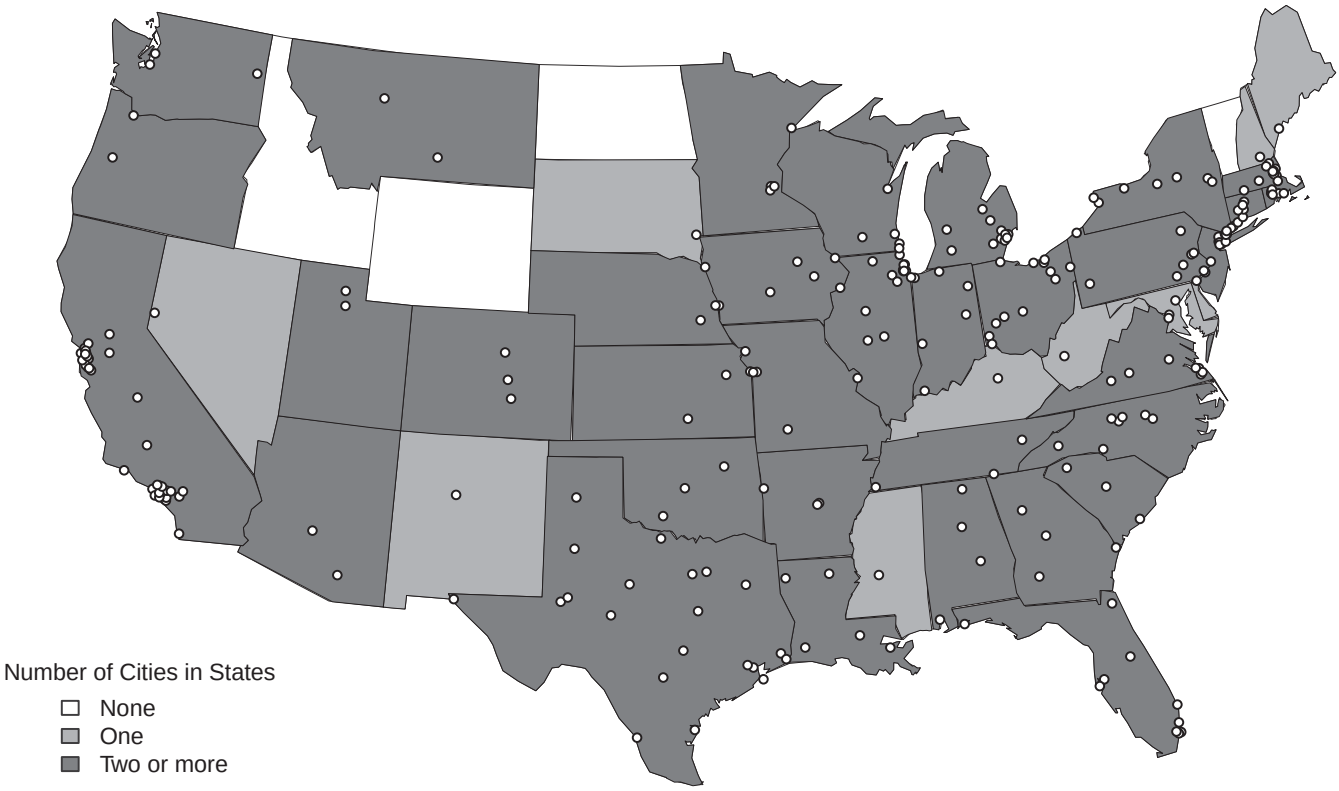
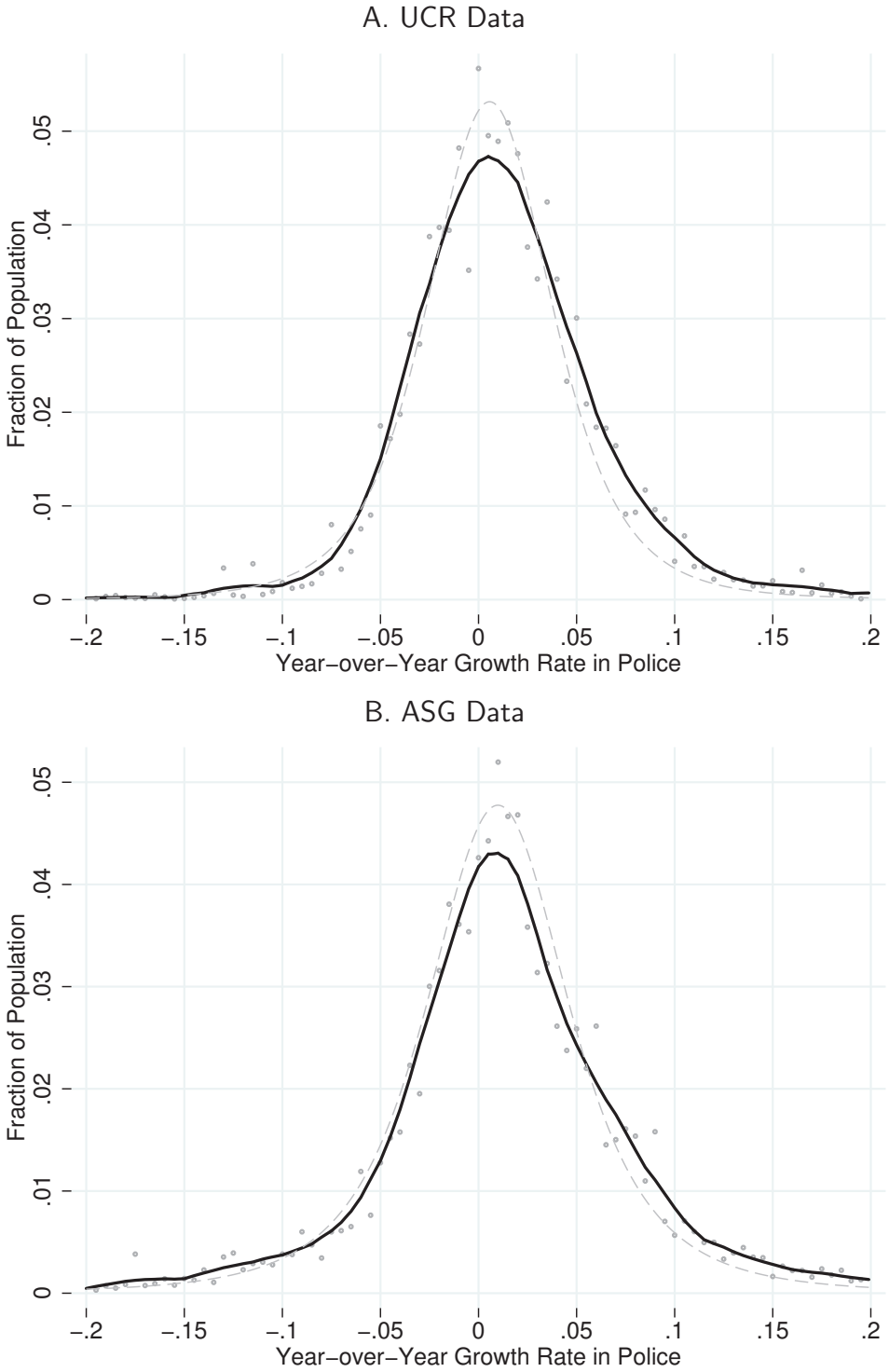


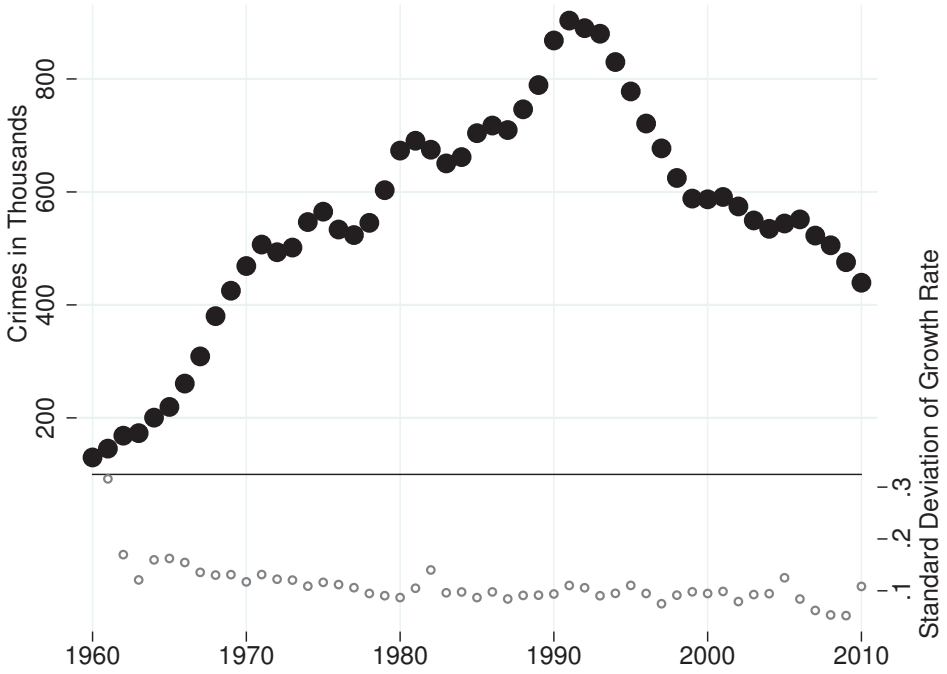
Figure 7. Distribution of Growth Rates in Police



Note: Curves are proportional to the population weighted conditional density function of growth rates in police, conditional on not having been equal to zero. In the UCR (ASG) data, 3.8 (6.1) percent of person-weighted city-years have exactly zero growth rate. Gray circles are undersmoothed histogram heights. The gray dashed line is represents the normal density plot. The scale for the y-axis is percent of person-weighted city-years. See text for details of density estimation.

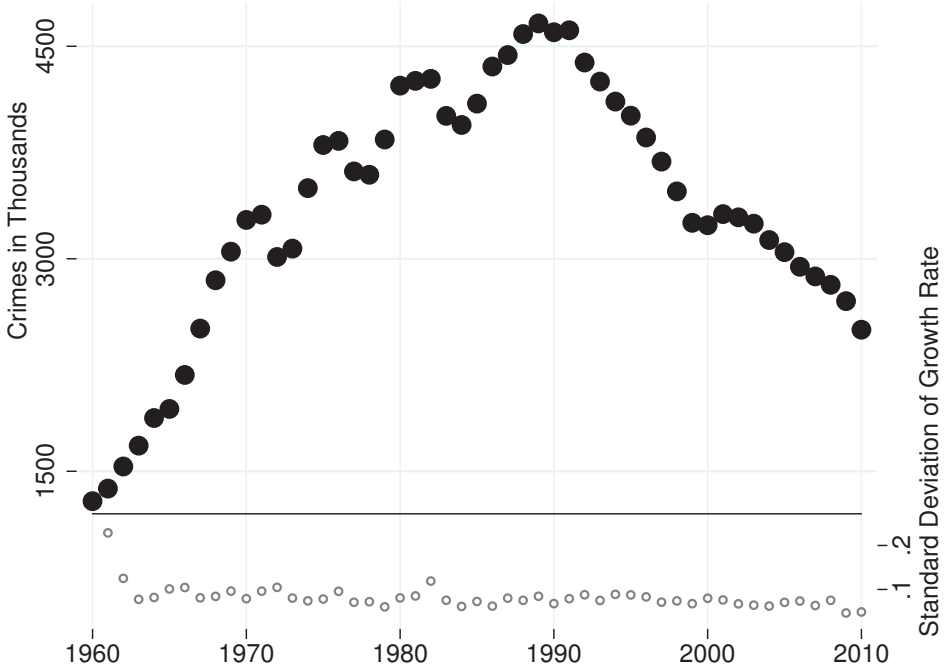
Figure 8. Aggregate Trends in Violent and Property Crime and Police: Evidence from the Uniform Crime Reports

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



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.

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



C. Sworn Police

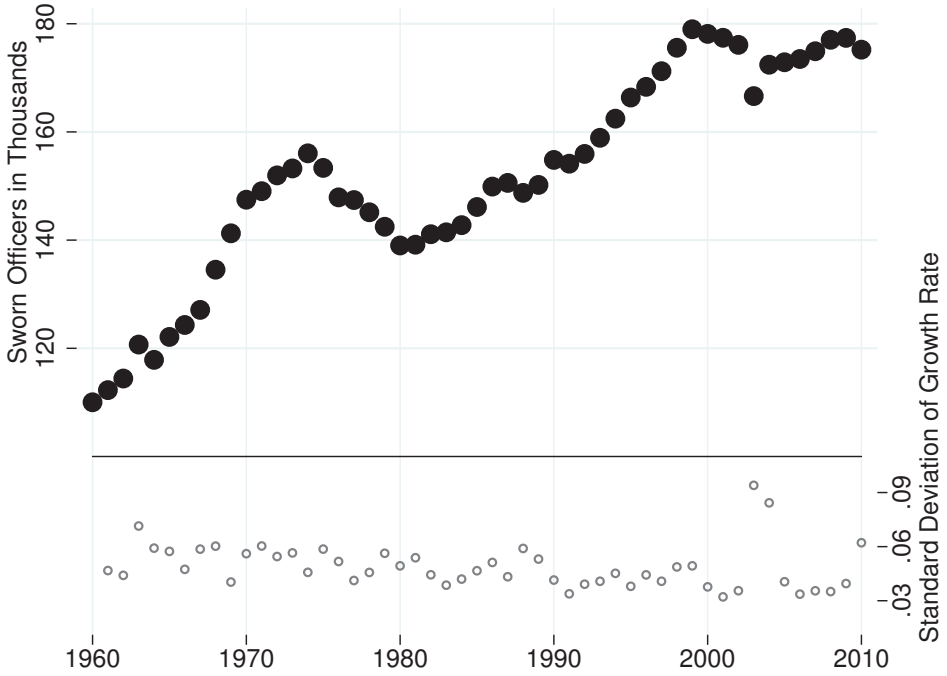
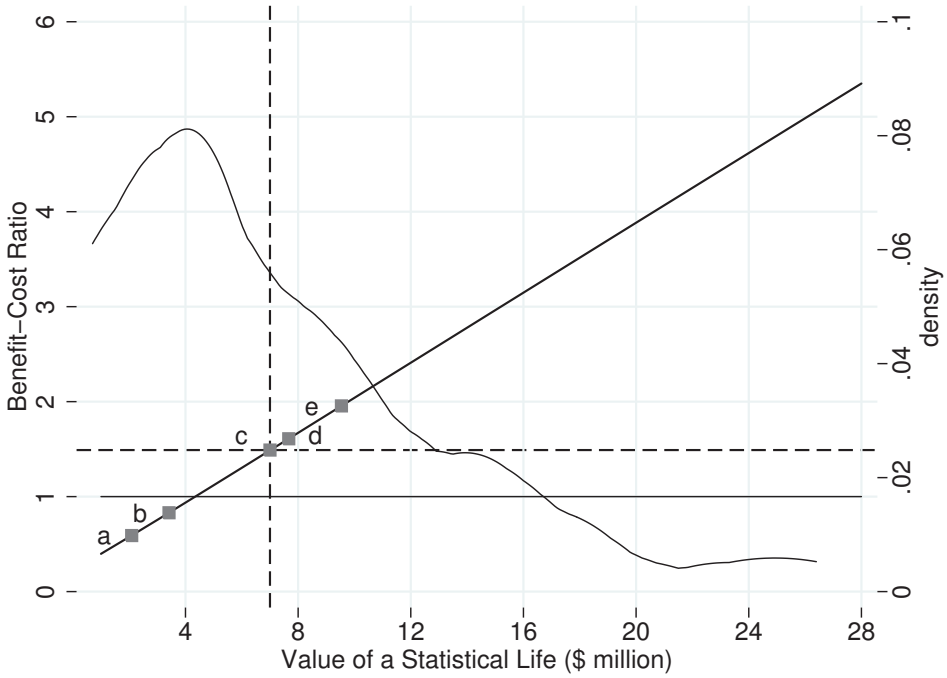


Figure 9. Cost-Benefit Analysis
Benefit-Cost Ratio as a Function of the Value of a Statistical Life



Note: The table plots the value of the benefit-cost ratio calculated using the two-step GMM procedure that pools the "forward" and "reflected" IV regressions 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, conditional on both the UCR and the ASG measure of the growth rate in the population size and a vector of unrestricted state-by-year dummies. For each measure of police, expenditures on personnel are estimated by multiplying the number of personnel by $130,000, an estimate of the "fully-loaded" annual salary of a police officer. Victimization costs for rape, robbery, assault, burglary, larceny and motor vehicle theft are drawn from Cohen (2000). As there is a great deal of variation in extant estimates of the value of a statistical life, the cost of murder is allowed to vary. Using the solid black line, we plot the benefit-cost ratio on the vertical axis as a function of the value of a statistical life, plotted on the horizontal axis in millions of dollars. The horizontal line correpsonds to a benefit-cost ratio of 1. In addition, we superimpose a kernel density function that plots the distribution of the extant estimates of the value of a statistical life. Key estimates include the $2.1 million VSL estimated by Ashenfelter and Greenstone (2004) ("a"), $3.4, the mean VSL among studies of non-labor market behavior ("b"), $7 million, the mean VSL in our sample ("c"), $7.7 million, the mean VSL used by various federal agencies for the 2004-2010 period ("d") and $9.5 million, the mean VSL among studies of U.S. labor market behavior ("e"). The dotted lines show the BCR ($1.49) at the mean value of a statistical life ($7 million). The majority of these estimates are drawn from Viscusi and Aldy (2003). We supplement these estimates with several that are drawn from the more recent literaure.

Table 1. Elasticity of the Growth Rate in Police
With Respect to the Growth Rate in Selected Covariates

Variable	Source	Years	UCR measure	ASG measure
A. Economic Characteristics				
Personal income	Bureau of Economic Analysis	1969-2010	0.056 (0.035) [0.040]	0.008 (0.050) [0.050]
Adjusted Gross Income	Internal Revenue Service	1990-2009	-0.024 (0.028) [0.026]	-0.041 (0.048) [0.035]
Wage and Salary Income	Internal Revenue Service	1990-2009	-0.019 (0.031) [0.028]	-0.046 (0.054) [0.051]
Total employment	Bureau of Economic Analysis	1969-2010	0.094 (0.047) [0.048]	-0.031 (0.073) [0.077]
Municipal budget cycle	Annual Survey of Government	1960-2010	0.036 (0.029) [0.029]	0.020 (0.025) [0.027]
B. Demographics and Social Disorganization				
Share of births to teenage mothers	Centers for Disease Control	1968-2002	0.009 (0.010) [0.008]	0.007 (0.014) [0.015]
Share of black births to teenage mothers	Centers for Disease Control	1968-2002	0.010 (0.009) [0.008]	0.020 (0.014) [0.015]
Share of births that are low birthweight	Centers for Disease Control	1968-2002	-0.015 (0.008) [0.008]	-0.000 (0.014) [0.015]
High school retention rate	National Center for Educational Statistics	1986-2008	0.005 (0.011) [0.012]	0.014 (0.016) [0.017]
Arrest-weighted demographic composition	U.S. Census	1970-2010	0.077 (0.036) [0.039]	0.077 (0.051) [0.042]
C. Lagged Crimes				
Violent Crimes	Federal Bureau of Investigation	1960-2010	0.010 (0.004) [0.004]	0.008 (0.005) [0.006]
Property Crimes	Federal Bureau of Investigation	1960-2010	0.016 (0.007) [0.006]	0.002 (0.009) [0.009]
Cost-Weighted Crimes	Federal Bureau of Investigation	1960-2010	0.007 (0.002) [0.003]	0.002 (0.003) [0.004]

Note: Each column reports results of a least squares regression of the growth rate in a given measurement of the number of police officers on the the growth rate in a key covariate, measured at either the city or the the county level. The first column pertains to models that employ the UCR measure of police while the second column pertains to models that employ the ASG police measure. All models condition on an unrestricted set of state-by-year effects as well as two measures of the growth rate in city population. Models are estimated using 2010 city population weights. Two sets of standard errors are reported below the parameter estimates. Huber-Eicker-White standard errors that are robust to heteroskedasticity are in parentheses while standard errors clustered at the city level are in square brackets.

Table 2. Summary Statistics on Police and Crime

			Levels (per 100,000 population)				Log Differences			
Variable	**N**		**Mean**	**S.D.**	**Min.**	**Max.**	**Mean**	**S.D.**	**Min.**	**Max.**
Sworn police (UCR)	12,157	O	248.5	114.0	52.6	786.6	0.014	0.056	-1.359	1.148
		B		107.2				0.012		
		W		39.1				0.055		
Sworn police (ASG)	11,960	O	261.8	132.0	40.3	779.8	0.015	0.080	-1.401	1.288
		B		122.9				0.012		
		W		46.4				0.079		
Violent crimes	12,021	O	930.8	629.4	6.6	4189.0	0.035	0.171	-1.804	1.767
		B		411.9				0.020		
		W		473.3				0.170		
Murder	12,274	O	14.2	10.5	0.0	110.9	0.015	0.410	-4.277	4.091
		B		8.1				0.015		
		W		6.7				0.410		
Rape	12,101	O	46.4	29.8	0.0	310.5	0.035	0.323	-4.384	4.199
		B		16.6				0.029		
		W		24.7				0.322		
Robbery	12,187	O	424.9	344.2	1.1	2,358.0	0.034	0.212	-2.639	2.565
		B		242.5				0.018		
		W		244.3				0.211		
Assault	12,176	O	465.2	338.9	1.0	2,761.3	0.037	0.228	-2.833	3.129
		B		204.4				0.023		
		W		270.7				0.226		
Property crimes	12,177	O	5,980.4	2,415.2	155.6	18,345.2	0.015	0.124	-2.330	1.769
		B		1,316.3				0.014		
		W		2,025.3				0.124		
Burglary	12,192	O	1,588.2	815.9	37.7	6,713.5	0.011	0.158	-2.457	2.030
		B		417.7				0.018		
		W		701.1				0.157		
Larceny	12,185	O	3,528.3	1,513.0	84.2	11,590.7	0.017	0.135	-2.228	2.146
		B		934.4				0.015		
		W		1,191.0				0.134		
Motor vehicle theft	12,186	O	862.7	570.7	8.4	5,294.7	0.012	0.178	-2.833	1.899
		B		363.2				0.017		
		B		440.4				0.177		

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. Summary statistics are reported both in levels per 100,000 population and in growth rates. All statistics are weighted by 2010 city population.

Table 3. First Stage Estimates

	"Forward" Models		"Reflected" Models	
	(1)	(2)	(3)	(4)
ASG measure	0.173	0.153		
	(0.014)	(0.013)		
UCR measure			0.362	0.356
			(0.029)	(0.029)
F-statistic	158.4	139.7	152.2	148.2
N	11,030	11,030	11,030	11,030
year effects	yes	yes	yes	yes
state-year effects	no	yes	no	yes

Each column reports results of a least squares regression of the growth rate in a given measurement of the number of police officers on the the growth rate in the other measurement of police. Columns (1) and (2) report results for the "forward" regressions in which the UCR measure is employed as the endogenous covariate and the ASG measure is employed as the instrumental variable while columns (3) and (4) report results for the "reflected" regressions in which the ASG measure is employed as the endogenous covariate and the UCR measure is employed as the instrumental variable. For each set of models, the first column reports regression results, conditional on both the UCR and the ASG measures of the growth rate in the city's population and a vector of year dummies. The second column adds a vector of unrestricted state-by-year dummies. 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 4. Estimates of the Effect of Police on Crime

	Least Squares Estimates				2SLS Estimates			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	"Forward"		"Reflected"		"Forward"		"Reflected"	
Violent crimes	-0.124	-0.126	-0.057	-0.063	-0.282	-0.357	-0.326	-0.337
	(0.037)	(0.038)	(0.024)	(0.023)	(0.114)	(0.144)	(0.088)	(0.107)
Murder	-0.270	-0.217	-0.230	-0.128	-1.314	-0.806	-0.717	-0.505
	(0.071)	(0.094)	(0.063)	(0.059)	(0.426)	(0.389)	(0.237)	(0.265)
Rape	-0.083	-0.091	-0.072	-0.011	-0.375	-0.067	-0.207	-0.243
	(0.069)	(0.090)	(0.067)	(0.037)	(0.380)	(0.343)	(0.181)	(0.253)
Robbery	-0.198	-0.215	-0.079	-0.082	-0.442	-0.494	-0.526	-0.590
	(0.047)	(0.046)	(0.032)	(0.029)	(0.175)	(0.180)	(0.124)	(0.127)
Assault	-0.056	-0.053	-0.019	-0.015	-0.041	-0.043	-0.139	-0.116
	(0.043)	(0.049)	(0.029)	(0.035)	(0.144)	(0.210)	(0.104)	(0.137)
Property crimes	-0.072	-0.052	-0.017	-0.017	-0.094	-0.135	-0.186	-0.144
	(0.028)	(0.025)	(0.020)	(0.035)	(0.076)	(0.091)	(0.057)	(0.068)
Burglary	-0.062	-0.057	-0.032	-0.041	-0.184	-0.284	-0.167	-0.163
	(0.042)	(0.036)	(0.026)	(0.021)	(0.113)	(0.130)	(0.086)	(0.099)
Larceny	-0.040	-0.023	0.007	0.000	0.042	-0.033	-0.095	-0.056
	(0.030)	(0.027)	(0.020)	(0.017)	(0.090)	(0.105)	(0.061)	(0.075)
Motor vehicle theft	-0.193	-0.130	-0.077	-0.033	-0.456	-0.243	-0.525	-0.369
	(0.049)	(0.042)	(0.033)	(0.025)	(0.195)	(0.150)	(0.105)	(0.114)
Cost-Weighted crime	-0.220	-0.162	-0.123	-0.115	-0.619	-0.639	-0.591	-0.393
	(0.053)	(0.070)	(0.034)	(0.041)	(0.187)	(0.252)	(0.149)	(0.193)
year effects	yes	yes	yes	yes	yes	yes	yes	yes
state-year effects	no	yes	no	yes	no	yes	no	yes

Note: Columns (1)-(4) reports results of a least squares regression of the growth rate in each of ten crime rates on the first lag of the growth rate in the number of sworn police officers. Columns (1) and (2) report results for the "forward" regressions in which the UCR measure is employed as the regressor while columns (3) and (4) report results for the "reflected" regressions in which the ASG measure is employed as the regressor. For each set of models, the first column reports regression results, conditional on both the UCR and the ASG measures of the growth rate in the city's population and a vector of year dummies. The second column adds a vector of unrestricted state-by-year dummies. Columns (5)-(8) 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 (5) and (6) report results for the "forward" regressions in which the UCR measure is employed as the endogenous covariate and the ASG measure is employed as the instrumental variable while columns (7) and (8) report results for the "reflected" regressions in which the ASG measure is employed as the endogenous covariate and the UCR measure is employed as the instrumental variable. All models are estimated using 2010 city population weights. Two sets of standard errors are reported below the coefficient estimates. Huber-Eicker-White standard errors that are robust to heteroskedasticity are reported in the second row below the coefficient estimates.

Table 5. Pooled Estimates of the Effect of Police on Crime: Within-State Differences

Estimator	Violent Crime	Murder	Rape	Robbery	Assault	Property Crime	Burglary	Larceny	Motor Vehicle Theft	Cost-Weighted Crime
One-step GMM	-0.346	-0.653	-0.157	-0.543	-0.080	-0.139	-0.222	-0.045	-0.307	-0.493
	(0.104)	(0.254)	(0.247)	(0.126)	(0.142)	(0.067)	(0.094)	(0.074)	(0.104)	(0.172)
Two-step GMM	-0.343	-0.586	-0.191	-0.567	-0.099	-0.141	-0.201	-0.049	-0.328	-0.451
	(0.097)	(0.242)	(0.231)	(0.118)	(0.128)	(0.062)	(0.090)	(0.068)	(0.100)	(0.172)
MLE	-0.326	-0.553	-0.190	-0.540	-0.093	-0.135	-0.184	-0.048	-0.321	-0.406
	(0.092)	(0.229)	(0.221)	(0.113)	(0.122)	(0.060)	(0.085)	(0.065)	(0.096)	(0.170)
Empirical Likelihood	-0.343	-0.585	-0.192	-0.567	-0.099	-0.141	-0.199	-0.049	-0.324	-0.451
	(0.097)	(0.241)	(0.229)	(0.118)	(0.128)	(0.062)	(0.089)	(0.068)	(0.103)	(0.171)
Test statistic:	0.02	0.62	0.28	0.34	0.13	0.01	0.97	0.05	0.68	1.38
N	10,074	10,389	10,179	10,254	10,237	10,239	10,257	10,248	10,251	9,904

Note: Each column reports results of a pooled IV regression of the growth rate in each of ten crime rates on the first lag of the growth rate in the number of sworn police officers, conditional on both the UCR and ASG measures of the growth rate in population and a vector of unrestricted state-by-year dummies. The parameter estimates reported in the table efficiently pool the "forward" and "reflected" IV parameters reported in columns (6) and (8) of Table 4. The one-step GMM estimator uses the identity weighting matrix while the two-step GMM estimator uses the updated variance estimator from the one-step procedure. Following Guggenberger and Hahn (2005), the empirical likelihood estimator uses five Newton iterations from the two-step GMM estimator. All models are estimated using 2010 city population weights. Huber-Eicker-White standard errors are reported in parentheses below the coefficient estimates. Below the parameter estimates and the standard errors, we report the value of of the overidentification statistic from the two-step GMM procedure. The test statistic corresponds to the pooling restriction that we estimate a common parameter on the growth rate in police and refers to a test of the equality of the "forward" and "reflected" coefficients. The test statistic 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			
	Full Sample	LEMAS Subsample		
	UCR-ASG (1)	UCR-ASG (2)	UCR-LEMAS (3)	LEMAS-ASG (4)
Panel A. Growth Rate in Crimes				
Murder	-0.004	-0.008	0.004	0.011
	(0.002)	(0.006)	(0.004)	(0.006)
Rape	-0.001	-0.014	0.006	0.020
	(0.003)	(0.014)	(0.010)	(0.014)
Robbery	0.000	-0.034	0.009	0.039
	(0.006)	(0.020)	(0.015)	(0.021)
Assault	0.005	0.010	-0.017	-0.027
	(0.004)	(0.017)	(0.012)	(0.018)
Burglary	-0.021	-0.004	0.002	0.007
	(0.010)	(0.026)	(0.020)	(0.027)
Larceny	0.010	0.003	-0.003	-0.004
	(0.011)	(0.033)	(0.021)	(0.032)
Motor vehicle theft	-0.005	0.002	0.020	0.016
	(0.008)	(0.022)	(0.016)	(0.024)
Panel B. Growth Rate in Police				
LEMAS police measure		-0.037		
		(0.045)		
ASG police measure			-0.047	
			(0.021)	
UCR police measure				-0.083
				(0.048)
Panel C. Growth Rate in Population				
UCR population measure	-0.036	0.725	0.466	-0.181
	(0.114)	(0.399)	(0.271)	(0.317)
ASG population measure	-0.024	-0.227	-0.432	-0.190
	(0.096)	(0.473)	(0.317)	(0.391)
F-test: all variables	0.24	0.34	0.17	0.02
F-test: crime variables	0.14	0.42	0.51	0.05
F-test: police variable		0.41	0.03	0.08
F-test: population variables	0.73	0.09	0.23	0.50

Note: Each column corresponds to a particular incarnation of measurement errors in the growth rate of police personnel. In column (1), the measurement error is calculated as the difference between the UCR series and the ASG series. Column (2) is identical to column (1) except that the estimates are obtained using only the years for which the LEMAS sample is valid. In column (3) the measurement error is calculated as the difference between the UCR series and the LEMAS series. In column (4), the measurement error is calculated as the difference between the LEMAS series and the ASG series. Due to the limited 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. Panel A reports the results of a series of regressions of growth rates in the difference in the measurement errors on the growth rates in each crime category, while Panels B and C report coefficients from a regression of the growth rate in the measurement errors on the growth rate in the remaining police measurement and the growth rate in population, respectively. Below Panel C, we present p-values from a series of F-tests on the joint significance of each set of variables. Each of the models conditions on an unrestricted vector of state-by-year 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. Robustness of Results to the Inclusion of Covariates
1970-2002 Sample

	"Forward" Models						"Reflected" Models					
	Endogenous Covariate: UCR Instrument: ASG						Endogenous Covariate: ASG Instrument: UCR					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Violent crimes	-0.333	-0.339	-0.348	-0.319	-0.340	-0.359	-0.222	-0.216	-0.162	-0.148	-0.144	-0.158
	(0.153)	(0.154)	(0.154)	(0.155)	(0.160)	(0.162)	(0.115)	(0.117)	(0.115)	(0.116)	(0.118)	(0.118)
Murder	-1.103	-1.079	-1.052	-1.023	-1.037	-1.082	-0.506	-0.462	-0.382	-0.357	-0.364	-0.392
	(0.404)	(0.407)	(0.408)	(0.411)	(0.420)	(0.429)	(0.305)	(0.308)	(0.305)	(0.306)	(0.313)	(0.316)
Rape	-0.055	-0.038	-0.096	-0.067	-0.030	-0.043	0.090	0.084	0.154	0.169	0.216	0.198
	(0.278)	(0.280)	(0.278)	(0.281)	(0.286)	(0.291)	(0.220)	(0.224)	(0.217)	(0.220)	(0.224)	(0.224)
Robbery	-0.576	-0.578	-0.553	-0.547	-0.552	-0.573	-0.690	-0.689	-0.619	-0.616	-0.600	-0.620
	(0.191)	(0.192)	(0.190)	(0.192)	(0.196)	(0.200)	(0.139)	(0.141)	(0.138)	(0.139)	(0.142)	(0.143)
Assault	0.085	0.065	0.028	0.080	0.048	0.032	0.128	0.128	0.173	0.195	0.215	0.211
	(0.206)	(0.208)	(0.210)	(0.212)	(0.216)	(0.220)	(0.147)	(0.149)	(0.149)	(0.151)	(0.150)	(0.152)
Property crimes	-0.201	-0.219	-0.213	-0.193	-0.207	-0.225	-0.111	-0.108	-0.087	-0.079	-0.086	-0.092
	(0.105)	(0.105)	(0.106)	(0.106)	(0.108)	(0.108)	(0.088)	(0.089)	(0.087)	(0.088)	(0.090)	(0.090)
Burglary	-0.368	-0.373	-0.376	-0.353	-0.352	-0.370	-0.120	-0.119	-0.105	-0.099	-0.064	-0.063
	(0.148)	(0.149)	(0.152)	(0.153)	(0.158)	(0.156)	(0.120)	(0.122)	(0.122)	(0.124)	(0.125)	(0.125)
Larceny	-0.081	-0.111	-0.107	-0.094	-0.125	-0.138	-0.037	-0.034	-0.005	0.001	-0.019	-0.024
	(0.124)	(0.124)	(0.124)	(0.125)	(0.127)	(0.128)	(0.096)	(0.096)	(0.095)	(0.096)	(0.097)	(0.096)
Motor vehicle	-0.339	-0.318	-0.307	-0.277	-0.273	-0.295	-0.377	-0.365	-0.334	-0.327	-0.324	-0.337
	(0.161)	(0.161)	(0.160)	(0.159)	(0.163)	(0.165)	(0.147)	(0.148)	(0.143)	(0.143)	(0.145)	(0.149)
Cost-Weighted Crimes	-0.763	-0.740	-0.732	-0.703	-0.720	-0.754	-0.374	-0.346	-0.278	-0.255	-0.262	-0.280
	(0.224)	(0.224)	(0.222)	(0.221)	(0.226)	(0.228)	(0.180)	(0.182)	(0.177)	(0.177)	(0.181)	(0.183)
state-by-year effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
economic covariates	no	yes	yes	yes	yes	yes	no	yes	yes	yes	yes	yes
lagged economic covariates	no	no	yes	yes	yes	yes	no	no	yes	yes	yes	yes
demographic variables	no	no	no	yes	yes	yes	no	no	no	yes	yes	yes
polynomials and interactions	no	no	no	no	yes	yes	no	no	no	no	yes	yes
linear time trends	no	no	no	no	no	yes	no	no	no	no	no	yes

Note: Each column reports results of a 2SLS regression of the growth rate in each of ten crime rates on the first lag of the growth rate in the number of sworn police officers. Columns (1)-(6) report results for the "forward" regressions in which the UCR measure is employed as the endogenous covariate and the ASG measure is employed as the instrumental variable while columns (7)-(12) report results for the "reflected" regressions in which the ASG measure is employed as the endogenous covariate and the UCR measure is employed as the instrumental variable. For each set of models, the first column reports regression results, conditional on both the UCR and the ASG measures of the growth rate in the city's population and a vector of unrestricted state-by-year dummies. The second column adds a vector of economic covariates while the third column adds the first lag of each of these covariates. In the fourth column, we add demographic controls which capture the proportion of a city's population that is comprised of each of sixteen age-gender-race groups. In the fifth column, we add polynomial terms and interactions of the demographic variables. Finally, in column (6), we add city-specific linear time trends. All models are estimated using 2010 city population weights. Huber-Eicker-White standard errors that are robust to heteroskedasticity are reported below the coefficient estimates.

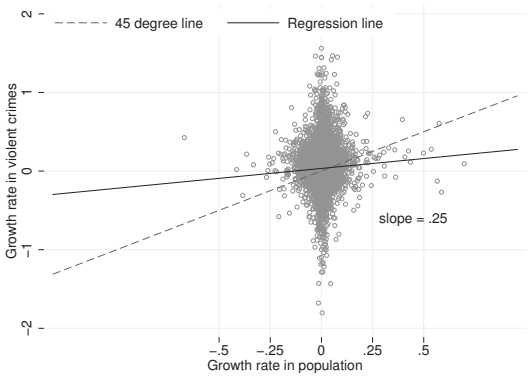
Table 8. Extant Estimates of the Effect of Police on Crime
Implied Elasticities

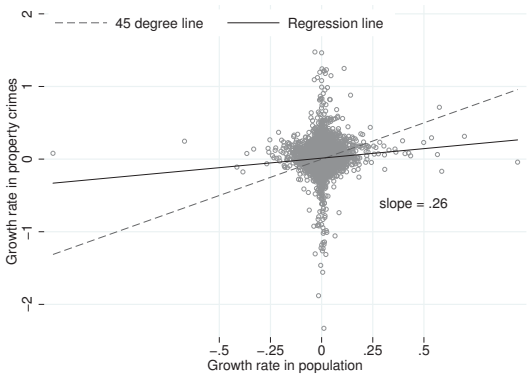
Article	Years	Cross-Sectional Units	Research Design	Violent Crime	Murder	Robbery	Property Crime	Burglary	Motor vehicle theft
Marvell and Moody (1996)	1973-1992	56 cities	lags as control variables		-0.13* (0.05)	-0.22* (0.06)		-0.15* (0.04)	-0.30* (0.07)
Levitt (1997)	1970-1992	59 cities	mayoral elections	-0.79 (0.61)	-3.03 (2.03)	-1.29 (1.00)	0.00 (0.34)	-0.55 (0.67)	-0.44 (0.98)
McCrary (2002)	1970-1992	59 cities	mayoral elections	-0.66 (0.65)	-2.69 (2.07)	-0.98 (1.09)	0.11 (0.43)	-0.47 (0.77)	-0.77 (1.08)
Levitt (2002)	1975-1995	122 cities	number of firefighters	-0.44* (0.23)	-0.91* (0.33)	-0.45 (0.26)	-0.50* (0.24)	-0.20 (0.26)	-1.70* (0.57)
Corman and Mocan (2005)	1974-1999	New York City	monthly time series		-0.50 (1.04)	-0.39 (0.45)		-0.28 (0.23)	-0.58* (0.25)
Klick and Tabarrok (2005)	2002-2003	Washington DC	high terrorism alert days	0.00 (-)				-0.30* (0.18)	-0.84* (0.25)
Evans and Owens (2007)	1990-2001	2,074 cities	COPS grants	-0.99* (0.33)	-0.84* (0.47)	-1.34* (0.52)	-0.26 (0.16)	-0.59* (0.18)	-0.85* (0.35)
Lin (2009)	1970-2000	51 states	state sales tax	-1.13 (0.74)	-2.73* (1.31)	-1.86* (1.12)	-2.18* (0.93)	-1.59* (0.80)	-4.14* (1.82)
Our preferred estimates	1960-2010	242 cities	measurement errors	-0.34* (0.10)	-0.59* (0.24)	-0.57* (0.12)	-0.14* (0.06)	-0.20* (0.09)	-0.33* (0.10)

Note: This table reports implied elasticities along with their associated standard errors that arise from eight 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.

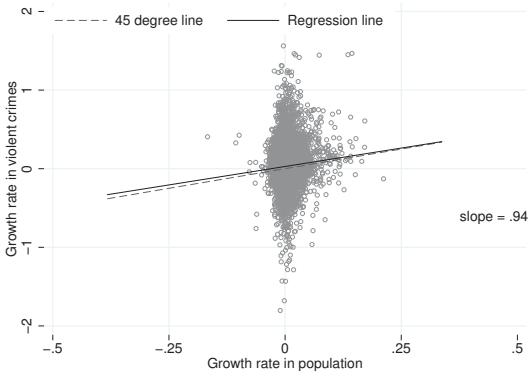
Appendix Figure 1A. Correlations Between UCR Police Measure and Crime

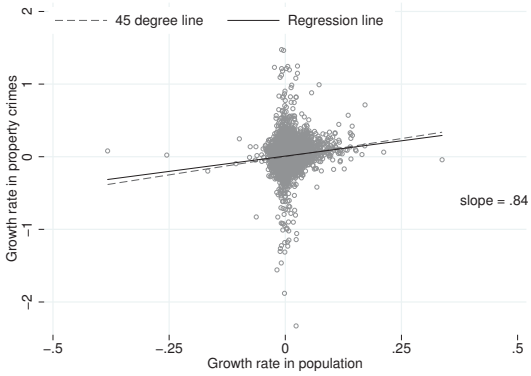
Panel A. Raw Population Measure





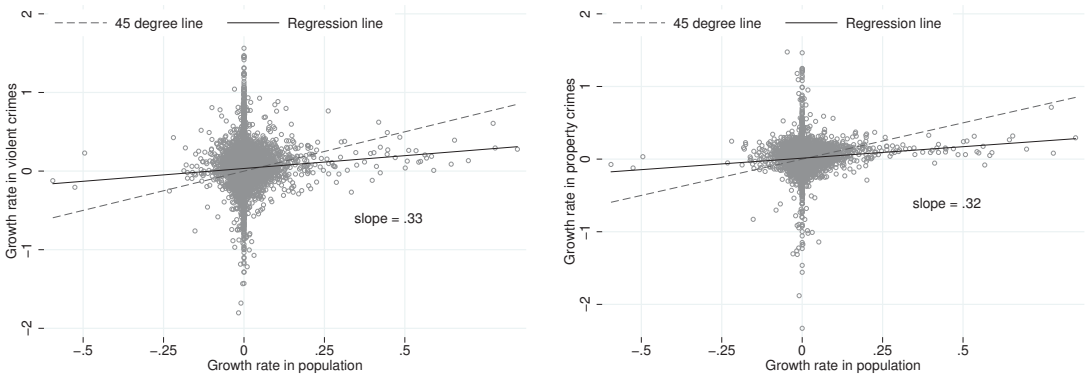
Panel B. Smoothed Population Measure



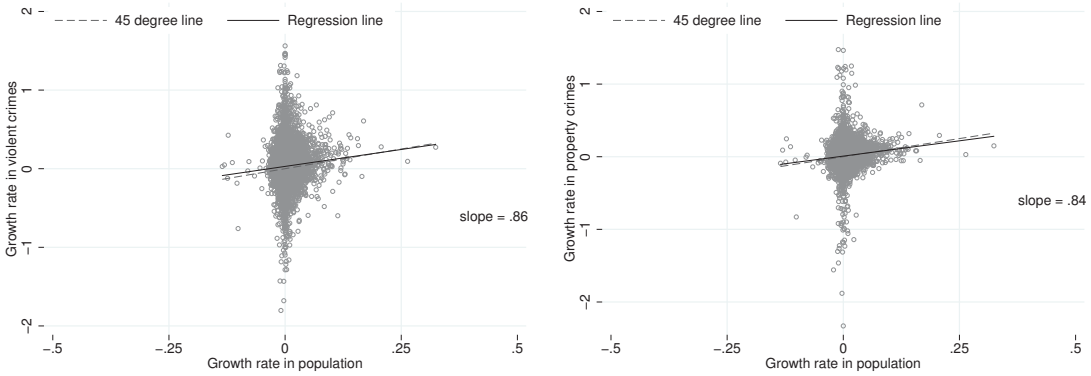


Appendix Figure 1B. Correlations Between ASG Police Measure and Crime

Panel A. Raw Population Measure



Panel B. Smoothed Population Measure



Appendix Table 1. Sensitivity of Pooled GMM Estimates to the Inclusion of Additional Population Measures

	Violent Crime	Murder	Rape	Robbery	Assault	Property Crime	Burglary	Larceny	Motor Vehicle Theft
Panel A. Data, 1960-1993 [Full Sample]									
without births measure	-0.320	-0.723	-0.236	-0.532	-0.047	-0.115	-0.224	-0.009	-0.334
	(0.101)	(0.239)	(0.242)	(0.127)	(0.134)	(0.064)	(0.092)	(0.070)	(0.099)
with births measure	-0.330	-0.734	-0.205	-0.538	-0.030	-0.119	-0.202	-0.016	-0.327
	(0.101)	(0.240)	(0.243)	(0.127)	(0.134)	(0.063)	(0.091)	(0.070)	(0.098)
Panel B. Data, 1960-2003 [N=147 cities]									
without births measure	-0.334	-0.501	-0.112	-0.553	-0.079	-0.153	-0.174	-0.068	-0.321
	(0.096)	(0.232)	(0.233)	(0.119)	(0.125)	(0.061)	(0.087)	(0.067)	(0.101)
with births measure	-0.367	-0.559	-0.143	-0.561	-0.093	-0.157	-0.195	-0.065	-0.370
	(0.098)	(0.236)	(0.236)	(0.122)	(0.127)	(0.062)	(0.090)	(0.068)	(0.101)
Panel C. Data, 1970-2008 [Full Sample]									
without births measure	-0.257	-0.676	0.087	-0.655	0.079	-0.151	-0.207	-0.068	-0.323
	(0.089)	(0.240)	(0.177)	(0.111)	(0.116)	(0.066)	(0.090)	(0.073)	(0.107)
with SEER measure	-0.267	-0.693	0.062	-0.661	0.060	-0.159	-0.218	-0.073	-0.334
	(0.089)	(0.241)	(0.177)	(0.111)	(0.116)	(0.065)	(0.090)	(0.073)	(0.105)

Note: Each column reports results of an estimation procedure in which the "forward" and "reflected" IV regression coefficients are pooled via GMM. Each panel in the table first presents parameter estimates that are conditional on the UCR and ASG population meausures. Next, a third measure (either using NCHS natality data or the SEER population measure) is added to the model. In Panel A, we estimate models on the full sample of data using the years 1960-1993, the years for which births data are available for all cities. In Panel B, we estimate models using the years 1960-2003 using the sample of 147 cities for which we have births data for this period. In Panel C, we estimate models on the full sample of data using the years 1970-2008, the years for which the SEER population measure is available. All models are conditional on a vector of unrestricted state-by-year dummies and are estimated using 2010 city population weights. As clustered and robust standard errors are very similar across all models, we report Huber-Eicker-White standard errors that are robust to heteroskedasticity in parentheses below the coefficient estimates.

Appendix Table 2. Testing the "Displacement" Hypothesis

	Violent Crime	Murder	Rape	Robbery	Assault	Property Crime	Burglary	Larceny	Motor Vehicle Theft
Panel A. Full Sample (N=242)									
	-0.343	-0.586	-0.191	-0.567	-0.080	-0.141	-0.201	-0.049	-0.328
Panel B. Retaining a Randomly Selected City in Each MSA (N=167)									
	-0.364	-0.757	0.031	-0.639	-0.115	-0.156	-0.231	-0.061	-0.318
Rejections	0/500	1/500	2/500	0/500	0/500	0/500	0/500	0/500	0/500

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, conditional on both the UCR and ASG measures of the growth rate in population and a vector of unrestricted state-by-year dummies. Panel A presents estimates for the full sample found in Table 5 of the paper. Panel B presents the mean elasticity arising from 500 replications of a routine in which we select a sample of cities in which we retain a random city in each MSA. All models are estimated using 2010 city population weights. Huber-Eicker-White standard errors are reported in parentheses below the coefficient estimates. Below Panel B, we present the fraction of t-tests in which we reject the null hypothesis that the parameter estimate in Panel B equals the parameter estimate in Panel A.

SUPPLEMENTAL TABLE: CITY-BY-CITY RANKING OF ALL 242 CITIES

Rank	City	2010 Population	Poverty %	Per Capita Income	Cost of Crime Per Capita	Fraction of Income at Risk	Estimated Cost per Police Officer	Officers per 100K Population	Benefit-Cost Ratio
1	Sunnyvale, CA	140,081	6.2	$43,828	$169	0.004	$280,434	168.1	0.17
2	Waltham, MA	60,632	11.5	$32,014	$123	0.004	$110,926	246.8	0.21
3	Torrance, CA	145,438	6.3	$36,007	$184	0.005	$204,302	161.5	0.26
4	Palo Alto, CA	64,403	5.7	$70,242	$211	0.003	$235,566	148.6	0.28
5	Bayonne, NJ	63,024	12.3	$28,698	$242	0.008	$107,971	369.2	0.28
6	Cambridge, MA	105,162	15	$44,717	$284	0.006	$157,353	260.9	0.32
7	Santa Monica, CA	89,736	11.1	$58,399	$434	0.007	$257,434	230.4	0.34
8	Stamford, CT	122,643	11.1	$44,667	$271	0.006	$139,771	239.9	0.38
9	Warwick, RI	82,672	7.6	$30,422	$194	0.006	$113,538	203.3	0.39
10	Burbank, CA	103,340	8.2	$33,320	$271	0.008	$212,202	148.9	0.40
11	Ann Arbor, MI	113,934	20.2	$30,498	$172	0.006	$151,331	129.3	0.41
12	Cranston, RI	80,387	8.4	$27,752	$183	0.007	$108,909	180.0	0.43
13	Alameda, CA	73,812	10.1	$38,434	$299	0.008	$233,762	134.9	0.44
14	Royal Oak, MI	57,236	6.8	$37,095	$185	0.005	$129,915	143.6	0.46
15	Alexandria, VA	139,966	7.8	$54,345	$335	0.006	$149,329	225.6	0.46
16	Lakewood, OH	52,131	15.1	$27,452	$244	0.009	$135,616	170.8	0.49
17	San Mateo, CA	97,207	5.9	$44,949	$312	0.007	$247,161	119.3	0.49
18	Bloomington, MN	82,893	7.3	$34,400	$231	0.007	$157,981	137.5	0.50
19	Livonia, MI	96,942	5	$31,632	$204	0.006	$121,898	156.0	0.50
20	Santa Barbara, CA	88,410	14.1	$36,601	$381	0.010	$216,493	155.6	0.53
21	Meriden, CT	60,868	13.8	$27,625	$311	0.011	$132,685	203.6	0.54
22	Santa Clara, CA	116,468	8.6	$38,422	$380	0.010	$251,667	129.2	0.54
23	Somerville, MA	75,754	14.7	$32,517	$305	0.009	$152,442	163.4	0.57
24	Honolulu, HI	953,207	8.8	$29,516	$310	0.010	$111,955	222.8	0.58
25	Berkeley, CA	112,580	18.9	$36,498	$610	0.017	$276,945	176.5	0.58
26	Eugene, OR	156,185	20.7	$24,917	$292	0.012	$189,450	123.1	0.58
27	Yonkers, NY	195,976	13.8	$29,191	$516	0.018	$132,415	311.4	0.58
28	Glendale, CA	191,719	13	$29,823	$256	0.009	$157,361	128.0	0.59
29	Fullerton, CA	135,161	11.3	$30,580	$300	0.010	$204,863	115.3	0.59

30	Alhambra, CA	83,089	12.7	$24,327	$276	0.011	$224,309	95.0	0.60
31	San Jose, CA	945,942	10.8	$33,233	$396	0.012	$205,538	145.9	0.62
32	Pasadena, CA	137,122	13.5	$38,650	$497	0.013	$219,095	166.8	0.63
33	New Britain, CT	73,206	20.5	$21,056	$358	0.017	$124,314	208.6	0.64
34	Madison, WI	233,209	17.9	$29,782	$330	0.011	$129,070	181.4	0.66
35	Union City, NJ	66,455	20	$18,506	$506	0.027	$136,100	262.8	0.66
36	Anaheim, CA	336,265	13.7	$22,911	$405	0.018	$232,325	119.9	0.68
37	West Covina, CA	106,098	8.7	$25,303	$421	0.017	$269,994	107.1	0.68
38	Miami Beach, FL	87,779	15.6	$40,515	$971	0.024	$157,127	415.2	0.69
39	West Allis, WI	60,411	11.8	$24,464	$381	0.016	$111,661	223.2	0.71
40	Malden, MA	59,450	12.8	$27,202	$376	0.014	$133,689	182.1	0.72
41	Manchester, NH	109,565	13.2	$27,002	$393	0.015	$129,379	189.7	0.75
42	Quincy, MA	92,271	9.8	$32,786	$442	0.013	$127,042	215.2	0.75
43	Duluth, MN	86,265	20.3	$23,845	$363	0.015	$133,980	166.7	0.76
44	Austin, TX	790,390	18.4	$30,286	$508	0.017	$156,460	197.6	0.77
45	Kenosha, WI	99,218	16	$22,748	$335	0.015	$98,878	195.3	0.81
46	New York, NY	8,175,133	19.1	$30,498	$675	0.022	$86,163	420.7	0.87
47	Waterbury, CT	110,366	20.9	$21,545	$519	0.024	$100,310	274.2	0.88
48	San Leandro, CA	84,950	8.6	$27,831	$527	0.019	$241,532	115.6	0.88
49	Dearborn, MI	98,153	22	$23,024	$454	0.020	$112,765	208.4	0.90
50	Seattle, WA	608,660	12.7	$40,868	$612	0.015	$143,557	218.5	0.91
51	San Francisco, CA	805,235	11.9	$45,478	$992	0.022	$173,336	292.7	0.91
52	Sioux City, IA	82,684	15.4	$21,147	$403	0.019	$133,985	150.1	0.93
53	Downey, CA	111,772	10	$22,731	$472	0.021	$232,164	100.8	0.94
54	Jersey City, NJ	247,597	17.5	$30,490	$1,084	0.036	$146,467	354.1	0.97
55	Portland, ME	66,194	17.5	$27,794	$543	0.020	$103,907	249.5	0.98
56	Portland, OR	583,776	16.3	$29,797	$637	0.021	$167,863	179.0	0.99
57	Worcester, MA	181,045	18.3	$24,326	$671	0.028	$126,883	249.3	0.99
58	Green Bay, WI	104,057	15.5	$23,045	$459	0.020	$119,836	180.2	0.99
59	Lincoln, NB	258,379	14.9	$25,146	$398	0.016	$145,323	128.4	0.99
60	Garden Grove, CA	170,883	12.9	$20,971	$439	0.021	$208,089	97.6	1.01
61	San Diego, CA	1,307,402	14.1	$32,553	$542	0.017	$161,539	153.5	1.02
62	El Paso, TX	649,121	24.1	$17,812	$399	0.022	$99,822	181.9	1.02
63	Norwalk, CT	85,603	8.2	$43,303	$485	0.011	$108,272	202.9	1.03

64	Evansville, IN	117,429	19.5	$21,153	$563	0.027	$104,894	239.1	1.05
65	Billings, MT	104,170	12.1	$26,449	$343	0.013	$113,833	132.4	1.06
66	Midland, TX	111,147	12.9	$31,592	$405	0.013	$112,727	157.3	1.06
67	Albany, NY	97,856	25.3	$23,341	$971	0.042	$119,058	353.1	1.08
68	Hayward, CA	144,186	12.5	$24,868	$638	0.026	$204,963	134.8	1.08
69	Kalamazoo, MI	74,262	35.4	$18,375	$872	0.047	$113,737	331.7	1.08
70	Colorado Springs, CO	416,427	11.8	$28,402	$620	0.022	$153,559	173.6	1.08
71	Scranton, PA	76,089	18.8	$19,068	$387	0.020	$80,190	205.1	1.10
72	Des Moines, IA	203,433	16.3	$24,096	$572	0.024	$126,723	189.1	1.11
73	Reno, NV	225,221	16.3	$27,714	$732	0.026	$180,771	166.9	1.13
74	Riverside, CA	303,871	14.9	$22,665	$625	0.028	$201,584	127.8	1.13
75	St. Joseph, MO	76,780	16.4	$21,002	$405	0.019	$105,390	155.8	1.15
76	Pasadena, TX	149,043	19.6	$19,940	$538	0.027	$123,982	175.5	1.15
77	Fort Lauderdale, FL	165,521	18.2	$35,828	$1,008	0.028	$148,957	272.5	1.16
78	Pensacola, FL	51,923	16.9	$29,341	$702	0.024	$97,932	288.4	1.16
79	Passaic, NJ	69,781	27.5	$14,424	$764	0.053	$114,317	266.0	1.17
80	Raleigh, NC	403,892	14.6	$30,079	$598	0.020	$122,848	192.7	1.18
81	Salt Lake City, UT	186,440	17.5	$26,055	$807	0.031	$138,982	228.2	1.19
82	Los Angeles, CA	3,792,621	19.5	$27,620	$1,072	0.039	$166,875	246.9	1.21
83	Hialeah, FL	224,669	20.1	$15,080	$539	0.036	$130,209	156.1	1.24
84	Boston, MA	617,594	21.2	$31,856	$1,174	0.037	$124,332	354.2	1.24
85	Fall River, MA	88,857	20.2	$20,337	$775	0.038	$118,277	244.8	1.25
86	Tampa, FL	335,709	19.5	$28,362	$1,052	0.037	$134,938	290.5	1.25
87	Lynn, MA	90,329	19.3	$21,616	$774	0.036	$143,334	201.3	1.25
88	Elizabeth, NJ	124,969	16.7	$19,196	$1,017	0.053	$135,838	275.0	1.27
89	Spokane, WA	208,916	18.7	$23,408	$690	0.029	$173,054	144.4	1.29
90	Denver, CO	600,158	19.2	$30,806	$949	0.031	$134,563	255.3	1.29
91	Lynchburg, VA	75,568	22.7	$21,586	$556	0.026	$90,751	221.4	1.29
92	St. Paul, MN	285,068	22	$25,066	$832	0.033	$144,724	207.5	1.29
93	Pawtucket, RI	71,148	17.8	$21,568	$424	0.020	$73,313	206.9	1.30
94	Racine, WI	78,860	18.9	$19,980	$853	0.043	$124,041	242.9	1.32
95	Long Beach, CA	462,257	19.1	$25,929	$895	0.035	$162,218	194.7	1.32
96	Erie, PA	101,786	25	$18,242	$568	0.031	$114,065	174.4	1.33
97	Lowell, MA	106,519	17.5	$22,730	$737	0.032	$115,347	223.0	1.33

98	Tyler, TX	96,900	20.2	$27,169	$676	0.025	$123,589	190.7	1.34
99	Pueblo, CO	106,595	21.2	$19,620	$651	0.033	$122,043	185.3	1.34
100	New Haven, CT	129,779	25.2	$21,789	$1,040	0.048	$105,828	337.2	1.36
101	Hamilton, OH	62,477	22	$19,709	$727	0.037	$116,693	212.4	1.37
102	Independence, MO	116,830	14.7	$22,738	$650	0.029	$124,402	176.5	1.38
103	Great Falls, MT	58,505	15	$22,450	$543	0.024	$131,470	139.0	1.38
104	San Angelo, TX	93,200	17.9	$21,827	$519	0.024	$105,891	162.7	1.41
105	Lexington, KY	295,803	17.4	$28,345	$650	0.023	$112,480	191.2	1.41
106	Hampton, VA	137,436	12.6	$24,051	$674	0.028	$117,610	188.5	1.42
107	Warren, MI	134,056	14.2	$22,110	$591	0.027	$111,527	172.6	1.43
108	Waterloo, IA	68,406	17.9	$22,057	$588	0.027	$105,946	180.0	1.44
109	New Bedford, MA	95,072	22.7	$20,447	$953	0.047	$105,140	293.1	1.44
110	Paterson, NJ	146,199	26.6	$15,543	$1,090	0.070	$114,219	305.3	1.46
111	Asheville, NC	83,393	20.5	$25,888	$721	0.028	$89,847	255.6	1.46
112	Council Bluffs, IA	62,230	15.2	$20,893	$799	0.038	$140,356	180.5	1.47
113	Tacoma, WA	198,397	17.1	$25,377	$996	0.039	$163,302	185.9	1.53
114	Sacramento, CA	466,488	17.3	$25,427	$1,116	0.044	$227,729	149.4	1.53
115	Ogden, UT	82,825	21.3	$19,548	$538	0.028	$100,995	160.8	1.54
116	Lansing, MI	114,297	24.7	$19,673	$1,139	0.058	$162,564	211.5	1.54
117	Springfield, MO	159,498	21.7	$20,793	$697	0.034	$101,560	207.1	1.55
118	Greenville, SC	58,409	17.2	$30,480	$980	0.032	$95,918	307.1	1.55
119	Lancaster, PA	59,322	27.6	$15,768	$1,016	0.064	$102,759	293.3	1.57
120	Syracuse, NY	145,170	31.1	$17,866	$1,284	0.072	$110,328	343.7	1.58
121	Santa Ana, CA	324,528	17.9	$16,613	$705	0.042	$200,256	103.7	1.58
122	Davenport, IA	99,685	17.7	$23,565	$751	0.032	$136,344	161.7	1.59
123	Springfield, IL	116,250	16.2	$28,542	$979	0.034	$124,475	229.9	1.59
124	Topeka, KS	127,473	18.8	$23,524	$884	0.038	$111,002	231.2	1.60
125	Fort Worth, TX	741,206	17	$23,792	$879	0.037	$119,083	212.3	1.62
126	Charlotte-Mecklenburg, NC	731,424	13.9	$30,984	$1,092	0.035	$120,965	253.5	1.66
127	Bridgeport, CT	144,229	20.8	$19,854	$1,327	0.067	$121,503	304.9	1.67
128	Fresno, CA	494,665	24.9	$19,709	$1,010	0.051	$165,716	169.0	1.68
129	Washington, DC	601,723	18.5	$42,078	$2,675	0.064	$111,236	665.0	1.69
130	Abilene, TX	117,063	17.8	$20,826	$649	0.031	$118,954	150.2	1.69
131	Omaha, NB	408,958	15.3	$26,123	$839	0.032	$128,648	178.1	1.71

132	Wichita, KS	382,368	15.6	$24,715	$892	0.036	$137,633	176.2	1.71
133	Peoria, IL	115,007	19.1	$28,384	$1,087	0.038	$141,951	208.2	1.71
134	Laredo, TX	236,091	29.2	$14,447	$822	0.057	$116,628	189.7	1.73
135	Corpus Christi, TX	305,215	18.8	$22,921	$812	0.035	$144,678	150.9	1.73
136	Schenectady, NY	66,135	20.6	$19,810	$1,177	0.059	$121,696	257.4	1.75
137	East Orange, NJ	64,270	21.4	$20,298	$1,645	0.081	$107,370	404.2	1.77
138	Beaumont, TX	118,296	22.1	$23,137	$989	0.043	$117,109	222.4	1.77
139	Vallejo, CA	115,942	13.9	$26,575	$1,002	0.038	$238,381	110.6	1.77
140	St. Petersburg, FL	244,769	14.3	$26,735	$1,169	0.044	$147,939	207.1	1.78
141	Wichita Falls, TX	104,553	17	$22,079	$837	0.038	$124,303	175.9	1.78
142	Grand Rapids, MI	188,040	24.3	$19,868	$884	0.045	$134,669	169.7	1.80
143	Amarillo, TX	190,695	16.7	$23,149	$840	0.036	$135,743	159.3	1.81
144	Lawrence, MA	76,377	26.5	$16,557	$865	0.052	$107,561	206.6	1.81
145	Fort Wayne, IN	253,691	15	$23,145	$812	0.035	$111,891	185.5	1.82
146	Galveston, TX	47,743	22.5	$23,963	$1,094	0.046	$104,148	268.1	1.83
147	Tucson, AZ	520,116	21.3	$20,243	$1,078	0.053	$144,402	188.5	1.85
148	Winston-Salem, NC	229,617	19.3	$24,472	$934	0.038	$99,580	234.5	1.86
149	West Palm Beach, FL	99,919	18.2	$29,620	$1,699	0.057	$143,357	295.0	1.87
150	Minneapolis, MN	382,578	22.7	$29,551	$1,339	0.045	$149,664	222.3	1.88
151	Lubbock, TX	229,573	20.4	$22,726	$859	0.038	$130,861	162.0	1.89
152	Lorain, OH	64,097	27.9	$18,212	$695	0.038	$122,696	139.5	1.89
153	Huntsville, AL	180,105	15.4	$29,255	$1,024	0.035	$112,755	221.2	1.91
154	Waco, TX	124,805	28.7	$17,323	$961	0.055	$120,480	190.4	1.95
155	Springfield, MA	153,060	27.6	$17,962	$1,389	0.077	$111,274	298.0	1.95
156	Toledo, OH	287,208	23.8	$18,758	$1,024	0.055	$114,777	213.0	1.95
157	Charleston, WV	51,400	16.9	$32,174	$1,348	0.042	$94,083	340.6	1.96
158	Phoenix, AZ	1,445,632	19.1	$24,460	$1,200	0.049	$141,151	201.3	1.97
159	Columbus, OH	787,033	21.4	$23,144	$1,228	0.053	$119,892	242.4	1.97
160	Providence, RI	178,042	26.3	$20,735	$972	0.047	$84,086	273.1	1.97
161	Tuscaloosa, AL	90,468	29.8	$21,042	$1,055	0.050	$82,744	300.7	1.98
162	Akron, OH	199,110	23.9	$19,664	$1,050	0.053	$110,866	222.7	1.98
163	Albuquerque, NM	545,852	15.7	$25,819	$1,060	0.041	$125,693	195.8	2.01
164	Chattanooga, TN	167,674	21.3	$23,434	$1,265	0.054	$111,178	263.4	2.01
165	San Antonio, TX	1,327,407	18.9	$21,812	$847	0.039	$120,836	161.3	2.03

166	Springfield, OH	60,608	25.8	$18,002	$998	0.055	$114,301	199.5	2.04
167	South Bend, IN	101,168	24.3	$18,805	$1,121	0.060	$104,447	245.2	2.04
168	Muncie, IN	70,085	30.4	$16,645	$520	0.031	$72,272	163.4	2.05
169	Bakersfield, CA	347,483	17.3	$23,022	$870	0.038	$183,081	107.9	2.05
170	Orlando, FL	238,300	16.6	$25,854	$1,612	0.062	$112,590	322.4	2.07
171	Durham, NC	228,330	17.9	$26,725	$1,007	0.038	$106,530	211.8	2.08
172	Oklahoma City, OK	579,999	16.6	$25,042	$1,132	0.045	$134,985	187.5	2.08
173	Greensboro, NC	269,666	18.1	$25,707	$1,104	0.043	$106,979	229.6	2.09
174	Milwaukee, WI	594,833	26.3	$18,884	$1,547	0.082	$104,380	329.3	2.10
175	Canton, OH	73,007	27.1	$17,144	$1,027	0.060	$106,903	212.5	2.11
176	Dallas, TX	1,197,816	22.3	$26,716	$1,507	0.056	$129,633	256.4	2.11
177	Rochester, NY	210,565	30.4	$17,865	$1,896	0.106	$120,049	344.8	2.13
178	Trenton, NJ	84,913	24.5	$17,400	$2,114	0.121	$106,591	429.3	2.15
179	Miami, FL	399,457	27.3	$19,745	$1,695	0.086	$139,839	261.1	2.16
180	Newark, NJ	277,140	25	$17,367	$2,600	0.150	$120,014	462.8	2.18
181	South Gate, CA	94,396	18.5	$13,913	$861	0.062	$210,718	87.2	2.18
182	Lawton, OK	96,867	18.6	$20,115	$918	0.046	$111,511	175.4	2.19
183	Pomona, CA	149,058	17.2	$16,682	$1,202	0.072	$219,705	115.7	2.20
184	Jacksonville, FL	821,784	14.3	$25,227	$1,244	0.049	$125,710	208.1	2.22
185	Brockton, MA	93,810	14.6	$22,254	$1,179	0.053	$127,042	194.9	2.22
186	Roanoke, VA	97,032	20.9	$22,530	$1,099	0.049	$86,225	266.8	2.23
187	Fort Smith, AR	86,209	21	$22,008	$993	0.045	$109,360	189.9	2.23
188	Monroe, LA	48,815	34.4	$20,120	$1,056	0.052	$63,798	340.8	2.26
189	Newport News, VA	180,719	13.5	$24,249	$1,089	0.045	$101,851	219.0	2.28
190	Philadelphia, PA	1,526,006	25.1	$21,117	$2,132	0.101	$96,304	453.3	2.28
191	Chicago, IL	2,695,598	20.9	$27,148	$1,603	0.059	$70,241	465.7	2.28
192	Rockford, IL	152,871	23.3	$21,422	$1,304	0.061	$138,962	190.5	2.30
193	Stockton, CA	291,707	19.8	$20,176	$1,446	0.072	$217,678	134.4	2.30
194	Hammond, IN	80,830	21.1	$17,844	$1,381	0.077	$103,549	263.2	2.36
195	Norfolk, VA	242,803	16.5	$23,773	$1,454	0.061	$90,786	314.2	2.38
196	Lake Charles, LA	71,993	20.9	$22,855	$1,109	0.049	$86,213	246.7	2.43
197	Cincinnati, OH	296,943	27.2	$23,982	$1,973	0.082	$114,262	330.4	2.43
198	Cleveland, OH	396,815	31.2	$16,302	$1,821	0.112	$94,526	365.8	2.45
199	Houston, TX	2,099,451	21	$25,927	$1,429	0.055	$114,078	236.9	2.46

200	Port Arthur, TX	53,818	24	$17,086	$1,223	0.072	$114,914	199.6	2.49
201	Buffalo, NY	261,310	29.6	$19,409	$1,956	0.101	$125,264	283.3	2.57
202	Hartford, CT	124,775	32.1	$16,798	$2,097	0.125	$110,837	337.5	2.61
203	Reading, PA	88,082	35	$13,135	$1,532	0.117	$110,296	247.6	2.61
204	Montgomery, AL	205,764	19.7	$24,409	$1,253	0.051	$94,206	235.4	2.63
205	Savannah, GA	136,286	23.8	$19,836	$1,220	0.061	$78,273	275.0	2.64
206	Nashville, TN	601,222	17.8	$26,550	$1,461	0.055	$117,135	219.3	2.65
207	Inglewood, CA	109,673	19.8	$19,508	$1,832	0.094	$191,557	168.1	2.65
208	Kansas City, MO	459,787	18.1	$25,683	$2,174	0.085	$130,392	292.5	2.66
209	Allentown, PA	118,032	24.6	$18,139	$1,241	0.068	$115,370	188.3	2.66
210	Portsmouth, VA	95,535	15.2	$22,302	$1,442	0.065	$106,002	235.3	2.69
211	Pittsburgh, PA	305,704	21.9	$24,833	$1,604	0.065	$101,645	272.3	2.70
212	Wilmington, DE	70,851	23.9	$25,228	$2,214	0.088	$93,479	403.6	2.74
213	Mobile, AL	195,111	21.5	$22,401	$1,122	0.050	$89,803	209.1	2.79
214	Indianapolis, IN	820,445	17.9	$24,334	$1,431	0.059	$117,840	200.3	2.82
215	Dayton, OH	141,527	31	$16,702	$1,955	0.117	$117,528	272.0	2.85
216	Knoxville, TN	178,874	23.4	$21,964	$1,292	0.059	$99,491	211.7	2.86
217	North Little Rock, AR	62,304	22.5	$23,578	$1,723	0.073	$87,544	317.8	2.89
218	Tulsa, OK	391,906	19.3	$26,069	$1,565	0.060	$122,882	205.4	2.89
219	Oakland, CA	390,724	18.7	$30,671	$2,516	0.082	$221,429	182.2	2.91
220	Albany, GA	77,434	32	$17,416	$1,170	0.067	$77,827	234.8	2.98
221	Shreveport, LA	199,311	22.1	$22,047	$1,618	0.073	$89,395	281.4	3.00
222	Atlanta, GA	420,003	22.6	$35,453	$2,133	0.060	$97,859	333.3	3.05
223	Memphis, TN	646,889	25.4	$21,007	$2,019	0.096	$99,214	310.4	3.05
224	Columbus, GA	189,885	18.2	$22,514	$1,041	0.046	$71,691	210.3	3.22
225	Camden, NJ	77,344	36.1	$12,807	$4,352	0.340	$122,706	510.1	3.24
226	Baltimore, MD	620,961	21.3	$23,333	$3,428	0.147	$97,815	478.5	3.41
227	Little Rock, AR	193,524	17.4	$29,229	$2,181	0.075	$105,633	275.7	3.49
228	Richmond, CA	103,701	16.4	$24,847	$3,010	0.121	$240,409	163.1	3.58
229	Richmond, VA	204,214	25.3	$26,034	$2,614	0.100	$96,612	351.6	3.59
230	Macon, GA	91,351	30.6	$17,110	$1,774	0.104	$75,503	298.8	3.66
231	San Bernardino, CA	209,924	27.4	$15,616	$1,969	0.126	$156,615	157.7	3.71
232	Pontiac, MI	59,515	32	$15,957	$1,918	0.120	$134,235	177.2	3.76
233	St. Louis, MO	319,294	26	$21,406	$3,486	0.163	$104,741	400.7	3.87

234	Baton Rouge, LA	229,493	25.5	$23,195	$2,286	0.099	$86,510	288.2	4.27
235	Jackson, MS	173,514	26.6	$19,095	$2,198	0.115	$81,255	261.0	4.83
236	Birmingham, AL	212,237	26.4	$19,775	$3,106	0.157	$85,184	350.4	4.85
237	Detroit, MI	713,777	34.5	$15,062	$3,691	0.245	$95,934	360.9	4.97
238	Youngstown, OH	66,982	32.7	$14,451	$3,057	0.212	$106,274	238.9	5.61
239	Saginaw, MI	51,508	37.4	$14,157	$2,970	0.210	$125,772	187.7	5.86
240	Flint, MI	102,434	36.6	$14,910	$3,291	0.221	$107,507	190.0	7.51
241	New Orleans, LA	343,829	24.4	$24,929	$3,963	0.159	$55,337	405.9	8.22
242	Gary, IN	80,294	34.2	$15,383	$4,376	0.284	$54,893	266.2	13.95

Available at http://emlab.berkeley.edu/~jmccrary/cmranking.xlsx.