Imputation methods make crime studies suspect: Detecting biases via regression discontinuity

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The Federal Bureau of Investigations (FBI) collects crime statistics from all police departments in the United States. A variety of datasets are derived from these statistics, although they are all commonly referred to as "Uniform Crime Reports." One of the crucial differences among these datasets is how they deal with missing statistics, specifically if they are coded as missing data or replaced by imputations. I show that the way missing statistics are accounted for, affects whether Right To Carry laws, giving immigrants legal status, and placebo laws impact crime. Further, I show that all currently employed methods for missing crime statistics lead to attenuation bias; i.e., the regressions underestimate the effect of policies that affect crime. This attenuation bias is due to the fact that police departments are less likely to submit their statistics to the FBI when crime is high, and I am able to provide empirical support for this hypothesis using a regression discontinuity design.

The Federal Bureau of Investigations (FBI) collects crime statistics from police departments.[1] A variety of datasets are derived from this data collection (FBI state, FBI county, FBI agency, NACJD county), although the are all commonly referred to as the "Uniform Crime Reports." One of the crucial differences among these datasets is how they deal with missing data: whether they are kept as missing data or imputed using a variety of methods.

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[1] Often, police and sheriff departments send the reports to state agencies, and the state agencies send these reports to the FBI.

Specifically, the FBI state data replaces missing data with the crime rate of agencies of the same city population group-state-year [Targonski, 2011].[2] These imputations significantly change the crime estimates. For instance, in the latest state data, the FBI inflated the number of violent crimes in Indiana, West Virginia, and Mississippi by 9.9%, 13%, and 68%, and the number of murders in Mississippi by 56% [McGinty, 2018]. The FBI state data has been used to study the causes of crime by Levitt [1996], Donohue and Levitt [2001], Duggan [2001], Caceres-Delpiano and Giolito [2012], and Cunningham and Shah [2018].

In contrast, before 1994, the National Archive of Criminal Justice Data (NACJD) replaced missing data with the average crime rate of other agencies in the same county-year. The NACJD data has been used by Lott and Mustard [1997], Mustard [2003], Heaton [2006], and Durlauf et al. [2016].

Alternatively, scholars have used the police departments statistics. City-years with missing reports are coded as missing values [Evans and Owens, 2007, Cunningham, 2016, Stuart and Taylor, 2017, Fu and Wolpin, 2018] or replaced by interpolation, using different years of data for the same city [Mello, 2018]. I refer to this last method as "longitudinal" imputation.

I first show that existing methods for missing reports lead to underestimating the level of crime and to underestimating the impact of government policies on crime. This occurs because police departments are less likely to report crime when it is high. Thus, any variable that increases crime, reduces the likelihood that crime is reported. Consequently, any method that assumes that crime data is missing at random leads to attenuation bias.

I employ a regression discontinuity design to show that police departments are less likely to report crime statistics when crime is high. The underlining assumptions in the regression discontinuity design are as follows. Cities with 9,999 people have the same crime rate as cities with 10,000 people. However, between 1972 and 2004, police departments in cities with 10,000 people were more likely to submit complete crime statistics, compared to departments in cities with 9,999 people. The higher compliance is due to the fact that, for these years, the FBI published the number of crimes for cities with population greater than 10,000, *Crime in the United States*, thus

[2] The FBI use the following city population groups: cities over 250,000, cities 100,000 to 250,000, cities 50,000 to 100,000, cities 25,000 to 50,000, cities 10,000 to 25,000, cities 2,500 to 10,000, cities under 2,500 and universities, rural and State police, suburban counties.

giving the FBI a greater incentive to obtain complete crime statistics for those cities.

Consistent with the hypotheses, the likelihood that the police provides complete data increases discontinuously at the population threshold of 10,000, for those years, but not for earlier or later years. Further, reported crimes increase discontinuously when a city population exceeds 10,000 for the years 1972-2004 (but not for earlier or later years).

To reconcile these two results, suppose I can divide the cities that report crime when the population is 10,000 into two groups. The first group consists of cities that would have reported crime statistics even if the population was 9,999. The second group consists of cities that would not have reported crime statistics if the population was 9,999. I assume that the reported crime rate in the first group is the same as the crime rate for cities with population 9,999.[3] Thus, given that the reported crime rate for cities with population 10,000 exceeds the crime rate for cities with population 9,999, it follows that the crime rate in the second group exceeds the average crime rates for cities with population 9,999. Thus the evidence is consistent with the hypothesis that in high crime cities, police departments are less likely to provide crime statistics voluntarily, and more likely to provide these statistics only after prodding by the FBI.

The regression discontinuity design also provides a test for the validity of imputation procedures. Specifically, I replace the missing crime rates by the crime rate of departments in the same FBI city population group-state-year, the crime rate of departments in the same county-year (NACJD), and by interpolated values of crime rates for the same agency, but different years (longitudinal). All procedures lead to imputed crimes rates that increase discontinuously at the population of 10,000 and thus that underestimate crime.

Second, I show that a Heckman procedure provides consistent estimates of the impact of policies on crime. The use of the Heckman procedure has been criticized in criminology because of the lack of valid exclusion restrictions [Bushway et al., 2007]. Specifically, in most applications it

[3]This is the exclusion restriction necessary to use the Heckman procedure. This assumption would fail if, for instance, the FBI was more likely to audit the statistics for cities with 10,000 or more people. However, there was no auditing of police statistics for most of the years studied, since the FBI only started conducting audits of local police agencies reports in 1997. Further, even for the years when there was auditing, it is unlikely that it would have affected the crime statistics, since audits are voluntary and carry no penalties [Poston, 2012], occur every three years, include between six to nine departments per state selected at random, and review a sample of just 300 hundred incidents per agency [Jacobs, 2012]. (In 2015, 22,524 agencies reported data to the FBI. Thus, between 1.3 and 1.9% of reporting agencies are reviewed every three years.)

is difficult to find variables that are uncorrelated with crime, but correlated with whether crime is reported. However, the regression discontinuity design provides evidence that the indicator variable for whether population is greater than 10,000 affects whether the police reports crime statistics. Further, it is reasonable to assume that this indicator variable should not affect crime (when controlling for population size). Thus, the discontinuous change in reporting at 10,000 people allows to control for missing data and obtain consistent estimates of the impact of policies on crime.

Third, I show that the choice of imputation measure affects whether crime is impacted by Right To Carry laws and increases in the number of legal immigrants. For instance, passage of Right To Carry legislation does not significantly affect property crime when imputed at the county level (NACJD), increases property crime by 18% with longitudinal imputation, and increases property crime by 40% when using FBI city population group imputation. Similarly, a 1% increase in the fraction of the county population that is a legalized immigrant does not affect crime when using county imputations (NACJD) and reduces crime by 3% when using FBI city population group imputations or longitudinal imputations.

Fourth, I show that the existence of multiple crime measures can lead to conclude incorrectly that certain policies impact crime. This occurs if statistically significant findings are more likely to be reported.[4] To show this, I examine "placebo laws," laws that are randomly generated and hence should have no statistical impact on crime. I generate 10,000 placebo laws and examine how frequently they are found to affect crime for at least one of the four measures of crime (the crime measure which excludes cities that did not report crime statistics and the ones that impute missing statistics using the FBI, NACJD, and longitudinal methods). If the four measures were essentially the same, then if the effect was significant for one measure, then it would be significant for all four measures. However, I find that 23% of the placebo laws have a statistically significant affect on at least one imputation measure of crime.

Thus, I have provided empirical support for two biases caused by existing crime measures. First, the measured impact of policies that affect crime is smaller than the real impact because the police is less likely to report higher levels of crime. Second, policies that do not affect crime

[4]This is often referred to a the "File Drawer Problem" [Rosenthal, 1979].

can be found to have a statistically significant effect on crime because there are multiple crime measures and journals tend to publish statistically significant results.

Perhaps to alleviate the second concern, Duggan [2001] and Miles and Cox [2014] show that their results are statistically significant for two different imputation measures of crime. This clearly makes the results more robust since I find that placebo laws have a statistically significant affect on two imputation measures only 14% of time. Both concerns can be addressed by limiting the analysis to cities with 100,000 people, where missing data is rare [Cullen and Levitt, 1999], or by using a Heckman procedure, such as the one used in this paper.

I first discuss the data and research design. Then I derive the regression discontinuity results using graphs and local regressions. Finally, I apply the results to study the impact on crime of Right To Carry laws, the legalization of immigrants, and placebo laws.

1 Data, samples, and summary statistics

The data that I use is drawn from multiple administrative sources. The number of crimes reported to the FBI by each police department for the years 1960-2015 is obtained from the Uniform Crime Report.[5] City population size is from the Decennial Census (1960, 1970, 1980, 1990, 2000, 2010) and from the Census of State and Local Finances (various years).[6] For the regression discontinuity design, I do not replace missing values of population with interpolated values.

I examine whether cities above and below the 10,000 population threshold have similar demographics and police presence in order to ensure that they have similar underlying crime rates. City demographics (share of the population that is black, between 18 and 64, and 65 and over) are obtained from the Decennial Census (1960, 1970, 1980, 1990, 2000, 2010) and the American Community Survey 2010-2014 (5 year estimate). I interpolate values to obtain measures of city demographics for the years in between. The number of police officers is obtained through the Uni-

[5]United States Department of Justice. Federal Bureau of Investigation. Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest, various years. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor]. The 1962 data for Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, Wyoming, Alaska, and Hawaii was inadvertently erased. Thus, for these state-years, the indicator variable for whether the agency submitted a report is set to missing [Maltz, 2018].

[6]The Census of State and Local Finances collects financial data from all government every 5 years, in years ending in “2” and “7” and a rotating subset of government for the other years.

form Crime Report, while police expenditures come from the Census of State and Local Finances. I do not replace missing policing data with interpolated values.

I restrict the sample to cities that reported data to the FBI at least once in the 1970s, 1980s, 1990s and 2000s. I further restrict the sample to city-years where the population was between 2,500 and 25,000. This population range includes the 1972–2004 threshold for inclusion in *Crime in the United States* (10,000) and the thresholds for two FBI city population groups used in imputations (cities 2,500 to 10,000 and cities 10,000 to 25,000). Finally, I restrict the sample to city years where the fraction of the population that is black is less than 20%.

Table 1 provides summary statistics for the three samples used in this study. The main sample covers the years 1972 to 2004. I conduct falsification tests using the 1960–1971 and 2005–2015 samples. For the years 1972-2004, 14 percent of observation are incomplete, outlier or missing. When crime is reported, there are 40 crimes per thousand individuals, and of those, 38 are property crimes (burglary, larceny, and vehicle theft).

Dates for enactment of Right To Carry laws are from Grossman and Lee [2008] and Ayres and Donohue [2009]. The number of legalized immigrants and county controls are obtained from Baker [2015].

2 Research design

In this paper, I show that including a city in *Crime in the United States* increases the likelihood that a police department submits a complete report to the FBI. I present both graphical and regression evidence for this finding. I further show that an indicator variable for whether a population exceeds 10,000 can be used to correct for the omitted data bias using a Heckman selection procedure. This section explains how regression discontinuity and the Heckman procedure are used in the empirical sections of this paper.

The regression discontinuity evidence only uses cities with population x between 10,000 $- h$ and $10,000 + h$, where $h > 0$ is the bandwidth. For these cities, I estimate a local linear regression $\mu_{-}(x)$ for the likelihood that a city with population x less than 10,000 does not submit a valid response:

$$\mu_-(x) = \alpha_- + \beta_-(x - 10{,}000).$$

Similarly, I estimate a local linear regression $\mu_+(x)$ for the likelihood that a city with population x greater than 10,000 does not submit a valid response:

$$\mu_+(x) = \alpha_+ + \beta_+(x - 10{,}000).$$

The impact of publication in *Crime in the United States* on the likelihood that a department fails to submit of complete report is the jump in intercepts:

$$\tau = \alpha_+ - \alpha_-$$

The local linear regressions, μ_- and μ_+, are estimated by minimizing the weighted prediction error. Specifically, if y_i is an indicator variable for whether city i failed to submit a complete report, the scalars α_+, β_+, α_-, β_- minimize:

$$\sum_{i:\, x_i \in [10{,}000, 10{,}000+h]} \Big(y_i - \mu_+(x_i)\Big)^2 K(x_i) \quad \text{and} \quad \sum_{i:\, x_i \in [10{,}000-h, 10{,}000]} \Big(y_i - \mu_-(x_i)\Big)^2 K(x_i),$$

where the triangular kernel weights $K(x_i)$ ensure that observations closer to 10,000 receive more weight:

$$K(x) = 1 - \left|\frac{x - 10{,}000}{h}\right|.$$

Finally, the bandwidth h is selected to minimize the mean squared error; see Calonico et al. [2014] and Calonico et al. [2017].

The same procedure described in this section is used to determine whether publication in *Crime in the United States* increases the crime rate among the departments who submit complete reports, changes the city demographics, and changes police resources.

Thus, the regression discontinuity results imply that there is a variable T that affects whether the police submits a crime report, but does not affect the true crime rate, where the variable T is an indicator for whether the city population exceeds 10,000. This "exclusion restriction" allows me to obtain consistent estimates of the impact of a crime-inducing variable x on crime y.[7] For exposition, I first assume that the data consists of a single cross-section and that x is the only explanatory variable.

If all the agencies submitted crime statistics, crime would be determined as

$$y = x\beta + u,$$

where $\beta > 0$ and where u is an omitted crime-inducing variable. However, not all agencies submit crime statistics. The regression discontinuity designed provides evidence that agencies with higher crime rates, y, and with fewer than 10,000 people are less likely to do so. Thus, if an agency submits crime statistics despite a high value of the observed crime-inducing variable, x, and a population less than 10,000 ($T = 0$), the unobserved crime-inducing variable, u, must be small.

The inverse Mills ratio, $\lambda = \lambda(\gamma x + T) > 0$ is a measure of the likelihood that a police department fails to send crime statistics as a function of x, T, but excluding u. Since $\lambda' < 0$, when the population is greater than 10,000 ($T = 1$), the police department is less likely to fail to send crime statistics. Further, $\gamma < 0$ since agencies are less likely to send complete crime reports when crime is high. Finally, if a department submits crime statistics even though it is was unlikely to do so (λ large), the value of u must be small. I.e., among departments that send a report,

$$y = x\beta + \delta\lambda + \nu,$$

where $\delta < 0$ and $E\nu = 0$.

It follows that

$$\frac{\partial y}{\partial x} = \beta + \delta\gamma\lambda'.$$

[7] In other words, T can be excluded from the equation that determines the true crime rate.

Since δ, γ, and λ' are negative

$$\frac{\partial y}{\partial x} < \beta.$$

Thus estimating the effect of a crime inducing variable x on crime on the sample of cities that report crime, underestimates the true impact of x.

Similarly, the effect of a crime-reducing variable x is muted on the sample of departments that submit crime statistics; i.e., $\beta < 0$, $\gamma > 0$ and hence

$$\frac{\partial y}{\partial x} > \beta.$$

In a panel setting, crime in city i and year t, y_{it}, is a function of the crime-inducing variables x_{it}, city fixed effects, c_i, and year fixed effects, d_t:

$$y_{it} = x_{it}\beta + c_i + d_t + u_{it}. \tag{1}$$

It is tempting to add the inverse Mills ratio, λ to Equation (1), and then estimate β by demeaning all variables, where "demeaning" refers to subtracting the city means of a variable for the years when crime is reported. However, this method leads to inconsistent estimates, since it leads the error for period t to be a function of the omitted crime inducing variables for all periods, while the inverse Mills ratio only controls for the omitted crime inducing variable for period t [Wooldridge, 2002, page 582].

In the absence of sample selection, Equation (1) can be estimated by adding the city means of all explanatory variables (Mundlak [1978]'s estimator).[8] This provides the same estimates of β as demeaning all variables, as long as the city fixed effects can be written as a linear function of the mean values of the city characteristics and white noise; i.e., $c_i = \overline{x}_i\gamma + v_i$ where $\overline{x}_i$ is the average value of x for city i. Further, by adding the inverse Mills ratio, Mundlak's estimator provides consistent estimates in presence of sample selection [Wooldridge, 2002, page 583]. Thus, in this

[8]Here the city means are computed over all observations, regardless of whether crime is observed or not.

paper, "Heckman estimator" refers to

$$y_{it} = x_{it}\beta + \overline{x}_i\gamma + \gamma\lambda_{it} + \gamma_t d_t \lambda_{it} + v_{it},$$

where the inverse Mills ratio λ_{it} is interacted with the year-fixed effects d_t, since the selection effect is likely to vary by year.

3 Graphical results for Regression Discontinuity

This section provides graphical evidence for the discontinuity in the fraction of cities that provide crime statistics around the 10,000 population threshold. Panel (a) in Figure 1 graphs the percent of agencies that submit incomplete, outlier or missing reports to the FBI, by city population, for the years 1971 through 2004. Specifically, cities are split into evenly spaced "bins," disjoint intervals of population sizes. For each bin, the figure graphs on the x-axis the average population share and, on the y-axis, the fraction of agencies that submitted incomplete, outlier or missing reports. Each point on the graph also contains a 95% confidence interval which is computed from the variance in whether police departments in a bin provide crime statistics .

Further, a polynomial of order four is fit through the points on the graph. The difference between the polynomial fit and the average within bin is the bias. Finally, the number of bins used is selected as to minimize the sum of the variances and the sum of the biases.

The graph indicates that agencies serving larger cities are less likely to submit incomplete, outlier or missing reports. This is not surprising given that larger police departments are more likely to have administrators compile crime statistics. Further, the fraction of incomplete reports appears to decrease discontinuously past the population threshold of 10,000, which corresponds to publication in *Crime in the United States*.

To provide further evidence, I conduct falsification tests by computing the same graph over the periods 1960 through 1971 and 2005 through 2015, panels (b) and (c).[9] Note that the proportion of cities that submit complete reports has increased over time. For instance, for cities with population

[9] *Crime in the United States* provides data for cities with population greater than 25,000 units 1971, 20,000 in 1971, and all cities starting with 2005.

2,500, the proportion of cities that fail to submit complete reports is (approximatively) 70% for 1960-1971, 22.5% for 1972-2004, and 15% for 2005-2015. However, I only find a discontinuity in the reporting rate at 10,000 for the years 1972-2004, the only years for which 10,000 is the threshold for publication in *Crime in the United States*.

Panel (a) in Figure 2 graphs the average crime rate by city size for the years 1971 through 2004, and for cities that submit crime statistics to the FBI. Larger cities have higher crime rates, however the number of crimes increases discontinuously at a population size of 10,000. This finding can be explained by the fact that cities do not submit crime statistics when crime is high unless prodded to do so, which occurs more frequently when the population exceeds 10,000. Again, I conduct falsification tests over the periods 1960 through 1971 and 2005 through 2015, panels (b) and (c). Larger cities have higher crimes for 1960–1971, but not for 2005–2015. Further, over those time periods, crime does not increase discontinuously at the 10,000 threshold, and, actually, may decrease.

The discontinuous increase in crime for the years 1971 to 2004 could be due to city demographics changing discontinuously at the 10,000 population threshold.[10] show that Figure 3 graphs the average city demographics by population size. While the demographics change monotonically with population size, they do not change discontinuously at 10,000.[11] Thus, the discontinuity in the number of crimes does not appear to be caused by a discontinuity in demographics.

Alternatively, the discontinuous increase in crime for the years 1971 to 2004 could be due to policing changing discontinuously at the 10,000 population threshold. Figure 5 graphs the number of police officers per capita and real police spending per capita, by city population size. While policing mostly changes monotonically with populations size, I do not find any discontinuities at 10,000. Thus, the discontinuity in the number of crimes does not appear to be caused by a discontinuity in policing.

The regression discontinuity design can also be used to measure imputation bias. If unbiased, the imputed crime rate would be continuous in population size at the 10,000 threshold. Panel (a)

[10] In fact, showing that the covariates change continuously at the threshold is a sufficient condition for the regression discontinuity estimator defined in the next section [Calonico et al., forthcoming].

[11] The picture for percent black is a bit ambiguous. However, the regression approach used in the next section shows conclusively that the share of the population that is black does not increase discontinuously at a population of 10,000.

in Figure 4 graphs the imputed crime rate, by city size, for the years 1971 through 2004, where missing reports are replaced by the crime rate of agencies of the same city population group-state-year (FBI). The bias in the data, as proxied by the size of the discontinuity, appears to increase when we impute the crime rate using other cities crime statistics (compare this figure to Panel (a) in Figure 2). Similarly, panels (b) and (c) graph imputed crime rates where missing data is replaced by average crime rates in the same county-year (NACJD) or by interpolating the crime rate using different years of data for the same city. All three interpolation methods keep a bias, although the largest is with the FBI city population group-state year method.

4 Local regression results for Regression Discontinuity

This section provides regression evidence for the discontinuity in the fraction of complete reports and crime rates around the 10,000 threshold. I re-estimate the graphical results in to order to obtain magnitudes for the reporting biases. In Table 2, each cell describes a different regression discontinuity estimate. For instance, column "1972-2004", row "Missing", shows that for the years 1972-2004, when population size exceeds 10,000 the likelihood that the agency fails to submit a complete report decreases by 2.5% points. Further, there is no discontinuity in reporting for earlier years, thus indicating that the inclusion of crime statistics in *Crime in the United States* is the cause for the higher reporting rate.

In the row "Crimes," the dependent variable is the per capita number of crimes for agencies that submitted complete reports. When the population size exceeds the threshold, the number of crimes increases by 2.6 per thousand individuals. Further, there is no discontinuous increase in crime for earlier or later years. Thus, the increase in the crime rate for 1972-2004 must be due to additional agencies reporting crime reports when their population exceeds 10,000.

Note that the 2.6 crimes per 1,000 increase is a very large increase since it is brought upon by only 2.6% additional reports. To see this, note that a city with a 9,999 population reports around 40 crimes per 1,000 individuals. When the population increases to 10,000, the additional reporting agencies must report, on average, 147 crimes per 1,000 in order for the new average to be $(40 + 0.025 \times 147)/1.025 = 42.6$.

In Table 3, each regression is re-estimated controlling for city demographics and year fixed effects as suggested in Calonico et al. [forthcoming].[12] The results do not change appreciably.[13]

5 Applications

This section shows that different imputation procedures lead to different policy recommendations with respect Right To Carry legislation, legalizing immigrant, and placebo policies. Further, I uses a Heckman procedure to provide unbiased point estimates of these policies and to provide further evidence that the police is less likely to report statistics when crime is high.

The Heckman procedure requires finding a variable that affects whether a police department submits a complete report, but does not affect the crime rate (exclusion restriction). Bushway et al. [2007] criticized the use of Heckman procedure in criminology because of the lack of valid exclusion restrictions. However, in the previous sections I have provided empirical evidence that an indicator variable for whether population is greater than 10,000 affects selection and it is reasonable to assume that this indicator variable should not affect crime (when controlling for population size). Thus, the previous sections have provided empirical evidence for the validity of the Heckman procedure to control for missing crime statistics.

In order for the indicator for the 10,000 population threshold to be predictive of missing data, I only examine the periods 1972 through 2004, and I only examine cities that with population between 2,500 and 25,000 and with less than 20 percent of the population that is black (for all years). These sample restrictions make the magnitude of our findings not comparable to prior studies. However, these estimates are more likely to be consistent since making the sample more homogeneous reduces the bias from employing the wrong functional form for the control variables [Imbens, 2015].

In the first application, I examine whether sample selection affects the measured impact of

[12] In order to estimate the model, I only include years with at least 1,000 observations.

[13] For the years 2005-2015, reporting also appears to increase discontinuously at the 10,000 threshold. However, this effect is likely due to a lag in the adjustment process. This can be seen by re-estimating the regression for the years 2006-2015. For these years, I find not discontinuity in the reporting rate. In the next section, I graph the adjustment in reporting for the years 1972 to 2004, Figure 6. I find that it take several years for the behavior of the police department to fully adjust to the new reporting threshold.

Right To Carry concealed weapons legislation. Lott and Mustard [1997] found that Right To Carry legislation reduced crime using county data from 1977 to 1992. Their study has been criticized over numerous fronts. First, Maltz and Targonski [2002] noticed that the study used the NACJD county-level Uniform Crime Reports, which contains many errors the authors did not attempt to control for, and hence argued that the results may be untrustworthy. However, Lott and Whitley [2003] argued that these concerns are misplaced since a mismeasured left-hand side variable does not lead to bias, and only results in less precision in the estimated coefficients [Hausman, 2001].

Second, Ayres and Donohue [2003] noticed that states that did not pass Right To Carry laws tended to be states victimized by the 1980s crack epidemic. Thus, Lott and Mustard results could be interpreted as the crack epidemic having raised crime, rather than Right To Carry laws having lowered crime. Because it is difficult to find good controls for the crack epidemic, Ayres and Donohue [2003] analyze a longer time period, thus reducing the impact of failing to control for the crack epidemic, and find that Right To Carry laws increased crime.

In this study, I recast these two criticisms. First, I showed in Section 3 that the imputation method in the NACJD county data leads to biased estimates (rather than noisy estimates). Second, it can be seen in Table 1 that this bias is especially large in Lott and Mustard's sample because they examined a time period with many incomplete reports. In order to examine the impact of imputation method on policy findings, I regress the log of the city crime rate on: an indicator variable for whether a city is in a Right To Carry state, percent of the city population that is black, 18–65, older than 65, city and year fixed effects, as well as a cubic function of the city population, allowed to differ on either side of 10,000. The Heckman selection procedure used in this paper requires a balanced panel. For this reason, I interpolated missing population sizes.

I estimate the regressions on the subset of observations with crime reports (column "OLS"), with missing observations imputed ("FBI," "NACJD," and "Longitudinal"), and on the sample with crime reports but with the inverse Mills ratio as a control for sample selection ("Heckman"). The inverse Mills ratio is computed by estimating a different probit regression for each year of data, where the dependent variable is an indicator variable for whether a city submitted a report, and the explanatory variables include an indicator for whether the population exceeds 10,000, in addition to all the variables used to predict crime, and their city means.

Consistent with expectations, for most years, the likelihood with which a department submits a complete report is higher if: the city population exceeds 10,000 and the city is not in a Right To Carry state. These results are graphed in Figure 6, rather than displayed in a table with 33 columns – one for each different year's probit regression. Specifically, the coefficient estimates for the probit regressions are used to compute the marginal effects; in other words how changes in the explanatory variables affect the likelihood of submitting a complete report. The marginal effects are computed for a city of population 9,999. In order to improve readability, I graphed a locally weighted scatterplot smoother (LOWESS) of the marginal effects rather than the marginal effects themselves. To further improve readability, I only graphed the coefficient for two variable variables.

In the Heckman estimator, the inverse Mills ratio is interacted with year fixed effects since the selection effect is likely to vary by year. In order not to report 32 rows for the interaction terms, the year fixed effects are centered.[14] This way, the (non-interacted) inverse Mill ratio indicates the selection effect for the average year.

As discussed in Section 2, the Hausman selection model accounts for city fixed effects differently than in most panel regressions. Rather than subtracting city means from the explanatory variables, the Hausman selection procedure includes city means as additional explanatory variables ("Mundlak's estimator for fixed effects"). Further, the inverse Mills ratio is interacted with the year fixed effects to allow for selection to impact crime differently each year. Finally, I compute standard error by state cluster bootstrap rather than the usual analytic formula.

Depending on the method, Right To Carry laws increase crime by 18%–27%, see Table 4. Further, the inverse Mills ratio (λ) is statistically significant at the 1% confidence level. Thus one needs to control for selection in order to obtain consistent estimates. Finally, the sign of the inverse Mills ratio provides further empirical evidence that the police is less likely to submit complete reports when crime is high: an agency that is unlikely to submit a report (large λ), but does (is in sample), tends to have a lower crime rate.

[14] The (uncentered) year fixed effect d_t is equal to 1 if the year is equal to t and zero otherwise. The uncentered year fixed effect $\hat{d}_t$ is equal to d_t minus the average value of d_t.

Most of the crimes in this sample are property crimes (burglaries, larcenies, and vehicle thefts), see Table 1. Thus, I would expect the impact of Right To Carry on property crimes to be similar as its impact on overall crime. However, when using the FBI and NACJD imputations for property crimes, the coefficient for "Right To Carry" doubles in size and the coefficient for NACJD imputations becomes statistically insignificant, Table 5.[15] Thus, the results obtained using imputations appear to be less robust than the results obtained on the cities that submitted reports (with or without controls for selection).

In the second application, I examine whether sample selection affects the measured change in the crime rate brought upon by giving undocumented immigrants legal status under the 1986 Immigration Reform and Control Act. Baker [2015] examines this question using a seldom used dataset (FBI County Uniform Crime Report), which counts the numbers of crimes for all the agencies that submit data and thus implicitly assumes that the number of crimes committed by agencies that did not submit a crime report is zero [Targonski, 2011].[16] Baker's analysis covers the years 1980-1999 and a subset of counties, since for privacy concerns, the Immigration and Naturalization Service did not provide data for counties where fewer than 25 applicants for legalized immigration status. This leads to a relatively small dataset, and thus, for this application I include cities regardless of their black share (thus making the sample less homogeneous). Furthermore, because there are too few observations for some states, I use Semykina and Wooldridge [2010] analytic formula for standard errors, rather than state cluster bootstrap standard errors.

Since the data is publicly available, I used the same county-level (and state-level) control variables as in Baker [2015]: population, unemployment rate, poverty rate, income, employment, per capital number of police officers, a crack index, and lagged abortions. The number of police officers per capita is clearly endogenous, and so is the crack index since it is based in part on the Uniform Crime Reports. Nonetheless, I included these variables in order to attempt to replicate Baker [2015]'s results. In addition to these county controls, I include a cubic function of city population, allowed to differ on either side of 10,000.

Again, I estimate the regressions on the subset of observations with crime reports, on the

[15] I excluded from this sample any city that has zero property crimes in any year.
[16] This dataset is also used in Duggan [2001].

datasets where missing observations are imputed, and on the subset of observation with crime reports but with a term to account for selection. Depending on the method, a 1% increase in legalized immigration decreases crime by 2.6%, 1.3–2.9%, and 3.1%, see Table 6. Further, the significance of the inverse Mills ratio (λ) signifies that only the Heckman procedure provides consistent estimates. Finally, the sign of the inverse Mills ratio provides further empirical evidence that departments are less likely to submit complete reports when they experience higher crime: an agency that is unlikely to submit report (large λ), but does (is in sample), tends to have a lower crime rate.

In the third application, I examine whether city crime is affected by placebo laws, laws that affect a random selected subset of state and years. Specifically, for each year (between 1972 and 2004), and for each state, the placebo law is in effect with probability one half, and these probabilities are independent across states and years. I determine the effectiveness of the placebo law by regressing crime on the placebo law, city controls, and city and year fixed effects. Crime is measured in four different ways: the missing reports are treated as missing values, or imputed using the FBI, NACJD, or longitudinal procedures. Thus, the regressions lead to four coefficients for the variable "placebo law," one for each crime measure.

First, I suppose that a placebo law impacts crime if at least one coefficient is statistically significant. This scenario can occur if scholars examine different measures of crime, but each scholar only looks at one measure of crime, and only reports statistically significant effects. Second, I suppose that the placebo law impacts crime if two of the coefficients are statistically significant and of the same sign (i.e., the placebo law increases crime using both measures or decreases crime using both measures). This scenario can occurs if each scholar only looks at two measures of crime, and reports the results only if they are both statistically significant and of the same sign.

I generated 10,000 placebo laws in order to examine how frequently they are found to affect crime under these two scenarios. Specifically, the top left entry in Table 7 is the frequency with which placebo laws impact crime at the 10% for at least one of the four measures of crime. If the four measures were essentially the same, then, when the effect is significant for one measure, it is significant for all four measures, and this occurs 10% of the time. However, I find that placebo laws affect at least one measure of crime 23% of the time, thus showing that the crime measures are significantly different from one another. I report similar finding for 5% and 1% confidence level in the middle and right columns of Table 7.

The bottom left entry in Table 7 is the frequency with which placebo laws impact crime at the 10% level for at least two of the four measures of crime. Thus, if studies validate their findings with two crime measures, only 14% of placebo laws impact crime at the 10% confidence level. Thus, providing regression findings using two different measures of crime significantly decreases type II errors (i.e., decreases the likelihood of concluding that placebo laws impact crime).

6 Conclusion

It is well known that police departments underreport the number of crimes [Eterno and Silverman, 2012]. In some instances these underreports could affect academic findings on how to reduce crime. For instance, hiring additional police officers may lead to a larger fraction of crimes being reported on official statistics, thus underestimating the benefits of hiring additional police [Vollard and Hamed, 2012]. However, it is unclear how the measured impact of other policies is affected by the underreporting of crimes.

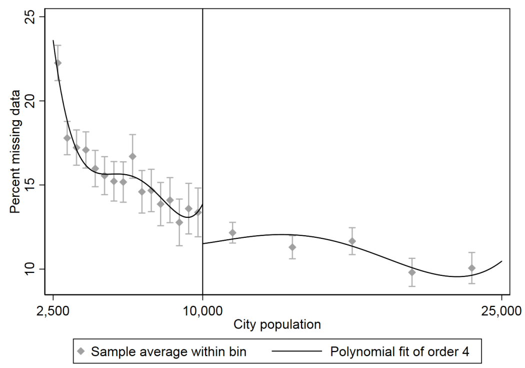
In contrast, it is less well known that crime statistics in the United States are bases on imputations, since many police departments fail to submit complete reports. I show that the missing data leads, in general, to a downward bias in the measured impact of policies on crime. Specifically, I use a regression discontinuity design to show that police departments are less likely to submit complete reports when crime is high. This implies that official statistics underestimate crimes and empirical studies based on the official statistics underestimate the impact on crime of Right To Carry laws and legalizing immigrants.

The regression discontinuity is based on the observations that the FBI is more likely to insure that the police submits a complete report if it publishes these numbers in its official crime statistics, *Crime in the United States*. For the years 1972–2004, this occurred when the city population exceeded 10,000, thus making police departments in cities with 10,000 people more likely to submit a complete report than in cities with 9,999 people. Thus, the additional monitoring of crime reporting that occurs at 10,000 people, provides a natural experiment to determine which departments are less likely to submit complete reports.

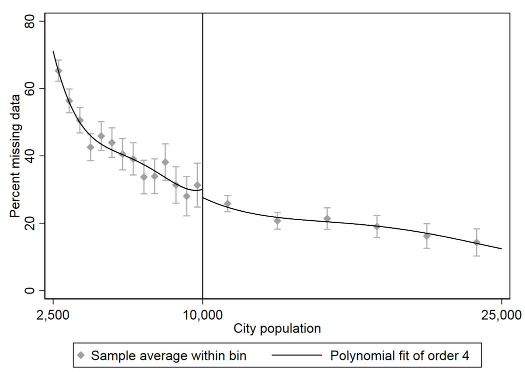
Another issue that has received less notice is that multiple imputation methods are currently employed. Thus, researchers are implicitly using different imputation methods depending on whether they download the Uniform Crime Report data from the Federal Bureau of Investigation or the National Archive of Criminal Justice Data, and whether they download the state or county level data. The use of multiple imputation procedures is a concern since I show that they lead to different findings regarding the impact on crime of Right To Carry laws, legalizing immigrants, and placebo laws. Thus, conventional significance levels overestimate the statistical significance of coefficients in published studies if journals tend to publish statistically significant results.

Figure 1: Percent of agencies with incomplete, outlier or missing reports to the FBI, by city size and time period

(a) Years 1972–2004



(b) Years 1960–1971



(c) Years 2005–2015

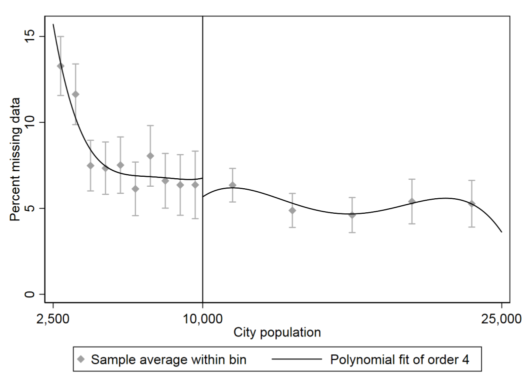
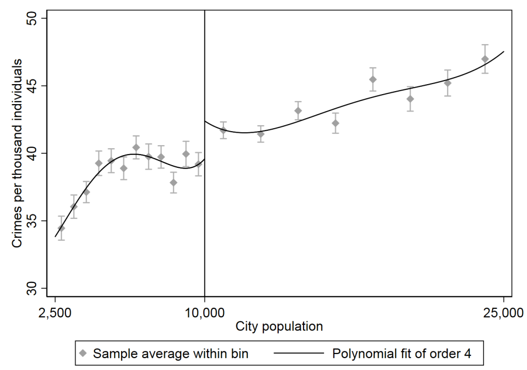
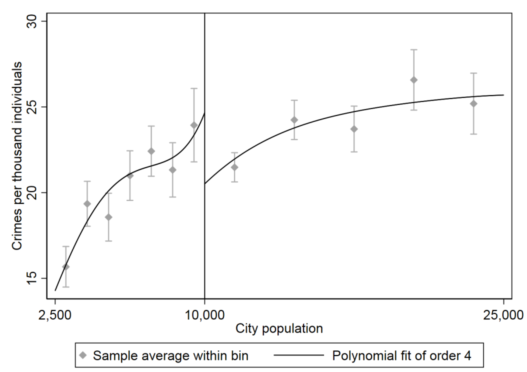


Figure 2: Crimes rates by city size and by time period

(a) Years 1972–2004



(b) Years 1960–1971



(c) Years 2005–2015

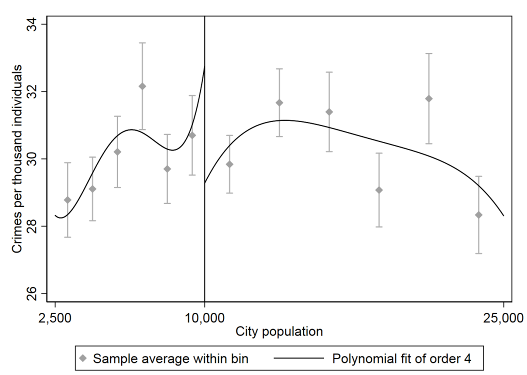
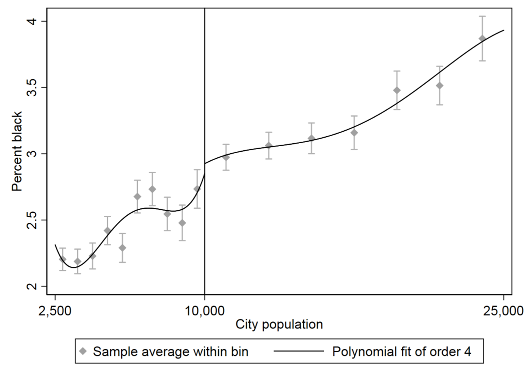
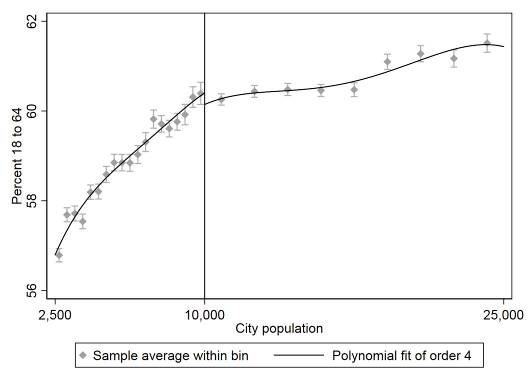


Figure 3: 1972-2004 demographics by city size

(a) Percent black



(b) Percent 18 to 64



(c) Percent 65 or older

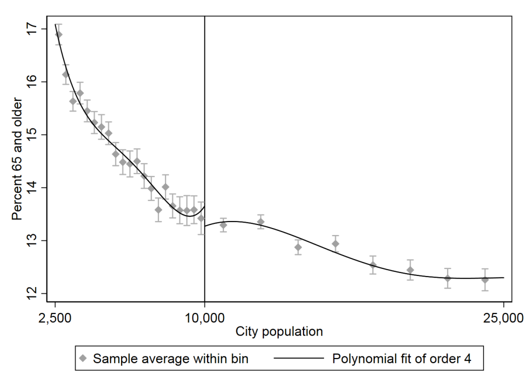
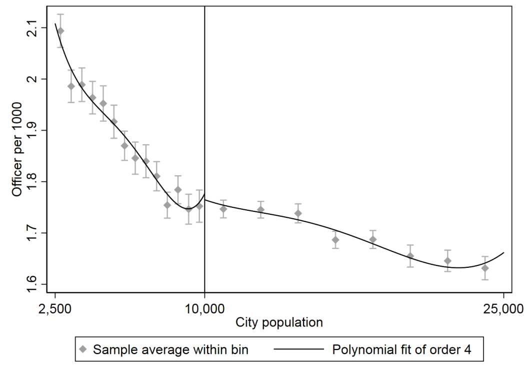


Figure 4: 1972-2004 policing by city size

(a) Police officers per thousand individuals



(b) Per capital spending on police (in real dollars)

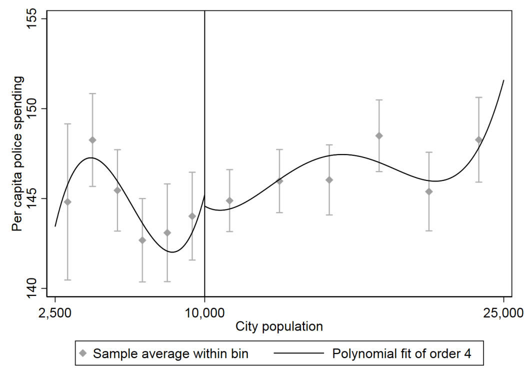
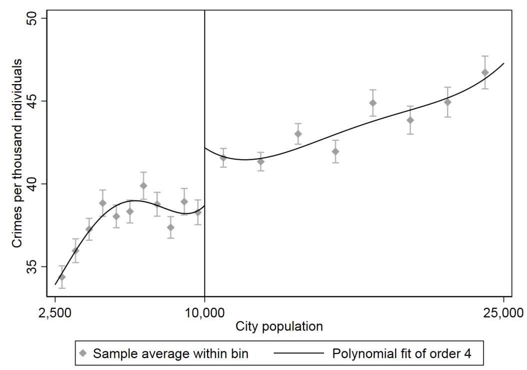
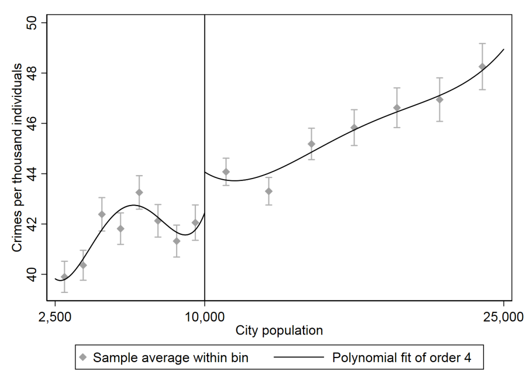


Figure 5: 1972-2004 imputed crimes

(a) Cross-sectional imputation by FBI population group-state-year



(b) Cross-sectional imputation by county-year (NACJD)



(c) Longitudinal imputation

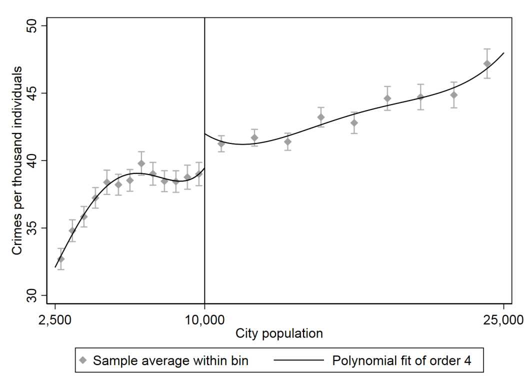
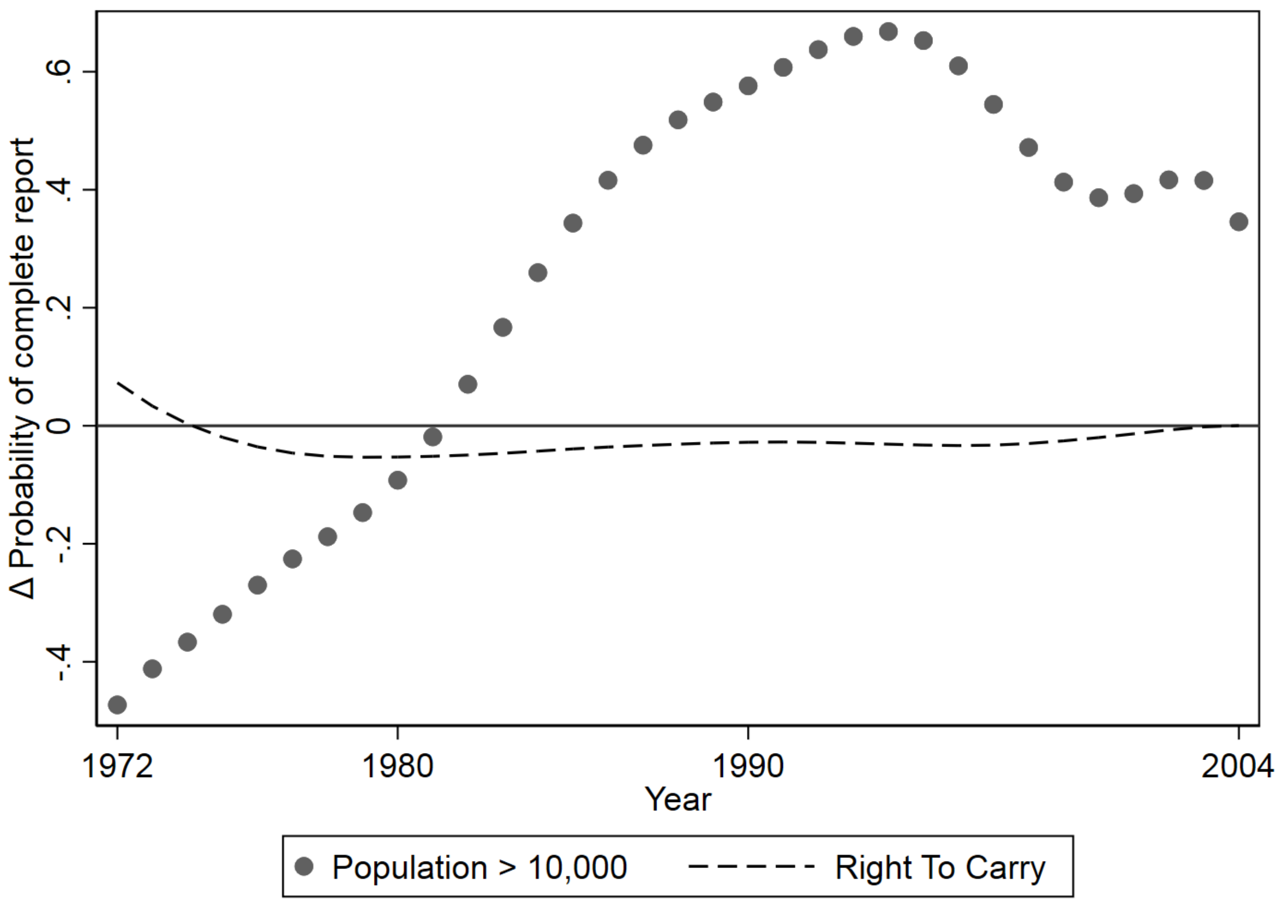


Figure 6: First stage of Hausman selection model



For each year, I estimated a probit regression for the probability that the police submits a complete crime report. The coefficient estimates are used to compute the marginal effects; in other words how changes in the explanatory variables affect the likelihood of submitting a complete report. The marginal effects are computed for a city of population 9,999. In order to improve readability, I graphed a locally weighted scatterplot smoother (LOWESS) of the marginal effects rather than the marginal effects themselves. To further improve readability, I have graphed the coefficient for only two variables: whether the city population exceeds 10,000, and whether the city is located in a Right To Carry state. The following variable are also included in the probit specification: percent of the population that is black, 18-64, or 65 and older; cubic function of the population, allowed to differ on either side of 10,000; city-averages of each explanatory variable.

Table 1: Summary statistics

Variable	1972–2004	1960–1971	2005–2015
Uniform Crime Data			
Percent incomplete	9	14	3
Percent outlier	0	1	0
Percent incomplete, outlier, or missing	14	34	7
Crimes per capita	40	23	30
Property crimes per capita	38	23	28
Census and ACS city data			
Percent black	3	2	3
Percent 18 to 64	59	55	61
Percent 65 or older	14	10	16
Number of observations	91,969	12,497	18,301

Sample is restricted to cities with population between 2,500 and 25,000 with less than 20 percent of the population that is black.

Table 2: For the years 1972-2004, when the city population exceeds 10,000, the city is more likely to report crime statistics and more likely to report higher crime rates

	1972-2004	1960-1971	2005-2015
Missing	-2.493**	0.388	-2.987
	(1.098)	(3.327)	(1.908)
Crimes	2.600***	-3.361*	-1.323
	(0.770)	(1.813)	(1.600)

Estimates of the impact of the city population exceeding 10,000 on whether crime data is missing and on the crime rate. All coefficients are estimated with local linear regressions with triangular kernel weights. The unit of observation is the city-year. * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 3: For the years 1972-2004, when the city population exceeds 10,000, the city is more likely to report crime statistics and more likely to report higher crime rates – estimates that control for city demographics (percent black, percent 18 to 64, percent 65 or older) and year fixed effects

	1972-2004	1960-1971	2005-2015
Missing	-2.810**	-0.164	-3.233*
	(1.151)	(3.420)	(1.953)
Crimes	2.399***	0.222	-3.319**
	(0.756)	(1.644)	(1.664)

Estimates of the impact of the city population exceeding 10,000 on whether crime data is missing and on the crime rate. All coefficients are estimated with local linear regressions with triangular kernel weights. The unit of observation is the city-year. * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 4: Reported crimes rates are higher after states enact Right To Carry laws and point estimate is largest when accounting for sample selection (Heckman procedure)

	OLS	FBI	NACJD	Longit.	Heckman
Right To Carry Law	0.178***	0.200***	0.197***	0.180***	0.266***
	(0.066)	(0.066)	(0.073)	(0.064)	(0.083)
λ					-0.542***
					(0.203)
Percent Black	0.001	0.001	0.003	0.000	0.003
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)
Percent 18-64	-0.007	-0.005	-0.005	-0.007	-0.007
	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)
Percent 65 or older	-0.024***	-0.022***	-0.020***	-0.025***	-0.024***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Median income					
N. of observations	87,858	96,890	103,627	102,564	87,858

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The unit of observation is the city-year. The crime rate (the dependent variable) is measured in logs. The Heckman regression uses as an instrument an indicator variable for whether the city population exceeds 10,000. λ denotes the inverse Mills ratio. In addition to the listed variables, all regressions include a cubic function of the population, allowed to differ on either side of 10,000 and year-fixed effects. All regressions include city fixed effects, although the Heckman selection procedure includes the mean of each explanatory variables as an additional control, rather than demeaning each explanatory variable. The Heckman procedure also includes the interaction of the inverse Mills ratio and year fixed effects. Standard errors are clustered at the state level. * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 5: Reported *property* crimes rates are higher after states enact Right To Carry laws and point estimate is largest when accounting for sample selection (Heckman procedure)

	OLS	FBI	NACJD	Longit.	Heckman
Right To Carry Law	0.182**	0.404**	0.388	0.182***	0.271***
	(0.069)	(0.180)	(0.395)	(0.067)	(0.085)
λ					-0.525***
					(0.203)
Percent Black	-0.002	0.013	0.025	-0.003	0.002
	(0.005)	(0.017)	(0.027)	(0.006)	(0.006)
Percent 18-64	-0.006	0.009	0.009	-0.005	-0.006
	(0.004)	(0.011)	(0.021)	(0.004)	(0.005)
Percent 65 or older	-0.023***	-0.019*	-0.020	-0.024***	-0.022***
	(0.005)	(0.010)	(0.019)	(0.005)	(0.005)
Median income					
N. of observations	87,573	96,560	103,267	98,752	87,573

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The unit of observation is the city-year. The property crime rate (the dependent variable) is measured in logs. The Heckman regression uses as an instrument an indicator variable for whether the city population exceeds 10,000. λ denotes the inverse Mills ratio. In addition to the listed variables, all regressions include a cubic function of the population, allowed to differ on either side of 10,000 and year-fixed effects. All regressions include city fixed effects, although the Heckman selection procedure includes the mean of each explanatory variables as an additional control, rather than demeaning each explanatory variable. The Heckman procedure also includes the interaction of the inverse Mills ratio and year fixed effects. Standard errors are clustered at the state level. * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 6: Reported crimes rates are lower after counties receive more legalized immigrants and point estimate is largest when accounting for sample selection (Heckman procedure).

	OLS	FBI	NACJD	Longit.	Heckman
Immigration	-2.621**	-2.945**	-1.255	-2.890**	-3.148***
	(1.167)	(1.200)	(2.395)	(1.163)	(0.793)
λ					-0.568***
					(0.055)
Population	0.124	0.136	-0.002	0.137	-0.038
	(0.183)	(0.171)	(0.170)	(0.190)	(0.107)
Unemp. Rate	0.018***	0.019***	0.010	0.018***	0.004
	(0.005)	(0.006)	(0.010)	(0.005)	(0.003)
Poverty Rate	-0.018***	-0.018***	-0.017**	-0.018***	-0.017***
	(0.006)	(0.006)	(0.007)	(0.006)	(0.003)
Income	-0.600**	-0.552**	-0.432	-0.595**	-0.584***
	(0.272)	(0.265)	(0.319)	(0.264)	(0.079)
Employment	0.321**	0.296*	0.193	0.296*	0.263***
	(0.150)	(0.150)	(0.183)	(0.158)	(0.083)
Police Per Capita	78.696***	73.734***	59.632***	72.944***	57.127***
	(18.399)	(15.384)	(20.032)	(15.377)	(13.612)
Crack Index	0.001	0.000	0.008	0.004	0.002
	(0.023)	(0.024)	(0.034)	(0.023)	(0.005)
Lagged Abortions	-36.001**	-32.141*	-37.780*	-36.384**	-36.201***
	(15.056)	(16.149)	(18.947)	(15.043)	(3.737)
N. of observations	54,421	58,852	61,983	62,027	54,421

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The unit of observation is the city-year, and all the listed explanatory variables are measured at the county level. The crime rate (the dependent variable), county population, county income and county employment are measured in logs. The Heckman regression uses as an instrument an indicator variable for whether the city population exceeds 10,000. λ denotes the inverse Mills ratio. In addition to the listed variables, all regressions include a cubic function of the city population, allowed to differ on either side of 10,000 and year-fixed effects. All regressions include city fixed effects, although the Heckman selection procedure includes the mean of each explanatory variables as an additional control, rather than demeaning each explanatory variable. The Heckman procedure also includes the interaction of the inverse Mills ratio and year fixed effects. Standard errors are clustered at the state level except for the Heckman regression which uses an analytic formula from Semykina and Wooldridge [2010]. * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 7: Likelihood of finding that a placebo law impacts crime

	Statistical significance level		
	10%	5%	1%
One measure of crime	23%	13%	3%
Two measures of crime	14%	7%	2%

The sample consists of all cities and the years 1972-2004. I generated 10,000 placebo laws, where a placebo law is in effect with probability on half, and these probabilities are independent across states and years. The true impact of each placebo law is the coefficient estimated by regressing city crime on the placebo law, city demographics (the population share that is black, 18 to 64, and 65 or older), and city and year fixed effects. However, since true crime is unobserved, I obtain four coefficient estimates corresponding to the crime rate for cities that submitted crime reports, and three imputation measures (FBI, NACJD, and longitudinal). The top left cell is the percent of the time the regression coefficient for "placebo law" is statistically significant at the 10% confidence level for at least *one* of the four measures. The bottom left cell is the percent of the time the regression coefficient for "placebo law" is statistically significant at the 10% confidence for at least *two* of the four measures. The center and right columns provide the percentages corresponding to 5% and 1% confidence levels.

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