Revisiting the Income Inequality-Crime Puzzle

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Abstract

From a theoretical perspective, there is a consensus on the positive relationship between income inequality and crime. Despite this, the existing empirical evidence has been unable to draw firm conclusions regarding the effects between these variables. In this work, we conduct a meta-analysis to establish whether such a link actually exists. Our analysis is based on a unique data set containing 1,431 observations from 47 studies. Once publication bias is taken into account, we find that the relationship between inequality and crime is statistically and economically not significant. We also find a high degree of heterogeneity among estimates. We argue that such results could depend on the measurement errors in the key variables and the presence of omitted variables. However, clearer theoretical indications are necessary to better identify the costs and benefits of offending, to then fully capture how income inequality affects crime.

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1. Introduction

According to the standard economics of crime models, criminals are rational individuals who engage in illegal activities following a cost-benefit analysis (Becker, 1968). Such models predict that higher income inequality should lead to more crime because it affects the incentives to engage in illegal behaviour (Ehrlich, 1973; Chiu and Madden, 1998 and Merlo, 2004). If poor individuals are those more likely to offend, and the rich are the potential victims, an increase in the incomes of the latter brings greater illegal payoff for the poor. On the other hand, if inequality grows due to a decrease in legal incomes for the poor, then crime should increase due to lower opportunity costs. If inequality is caused by a simultaneous increase/decrease in the incomes of the rich/poor, then these models predict that crime unambiguously increases. Other powerful approaches, such as those based on the strain or the social disorganization theory (Merton, 1938; Shaw and McKay, 1942), lead to a similar positive relationship. These theories are also better equipped to explain violent crime compared to the rational choice model.

That income inequality increases crime seems confirmed by simply plotting several inequality measures against homicides rate in various countries, as we do in Figure 1. However, the empirical evidence, based on regression techniques, does not show that income inequality is unambiguously affecting crime. For example, Kelly (2000) found that, in the USA, income inequality is positively related to violent, but not property crime. On the other hand, Demombynes and Özler (2005) found evidence of a positive impact on property, but much less on violent crime. Even among other studies that report positive effects, the size of the inequality-crime relations varies considerably. How can we reconcile such evidence with the theoretical predictions? In this work, we aim at contributing to answer this question. We first provide some arguments that help explaining why income inequality might not necessarily affect crime. We argue that the typical regressions employed in the literature present errors in the measurement of crime. As it is well known, official data are only imprecise measures of the level of criminal activity, due to low reporting rate (MacDonald, 2001). Such measurement error is not innocuous and it is likely to be associated with crime determinants, biasing the income inequality-crime coefficient (Gibson and Kim, 2008). Moreover, there is no consensus on which income inequality measure to employ in the empirical analysis. This is mainly due to the lack of clear theoretical guidance on how to better represent the costs and benefits of committing a crime (Ehrlich, 1973). The prevalent use of the Gini coefficient seems to be dictated more by the availability of such statistics, rather than theoretical consideration. Additionally, the typical regression is likely to suffer significantly by the omission of variables that are simultaneously related to both inequality and crime (Atems, 2020). For example, failing to control for deterrence, whether public and private, might severely bias the coefficients.

The second contribution of this work is to estimate the average effects of income inequality on crime from the literature. We do this through a meta-analysis, which consists of collecting the results from individual studies on a particular relationship (Stanley and Doucouliagos, 2012). Such type of analysis provides more precise estimates, because it corrects for the presence of publication bias, a problem in many empirical studies. Our data set includes 47 studies and 1,431 estimates, more than 30 per study. In order to be able to directly compare studies with different functional forms, crime or inequality measures, we compute two effect sizes: partial correlation coefficients, and Fisher's z-statistics. We employ inverse variance weighting methods, that assign greater weights to more precise studies. Although our preferred econometric technique is the unrestricted weighted least squares, we also consider random effects and hierarchical methods. Our findings suggest that the effect sizes are positive and statistically significant. However, once we control for publication bias, the effect sizes are mostly zero or economically insignificant. In other words, we do find that, on average, inequality does not explain crime levels. We also find some evidence of publication bias, in favour of positive results. Although such analysis is revealing, the heterogeneity of effect sizes might depend on the characteristics of the studies. Therefore, our third contribution is to conduct a meta-regression to analyze the regressions' characteristics associated with the effect sizes. For example, we can verify whether the type of crime, or inequality measure, matters. We divide the effect sizes from regressions employing different crime categories, such as homicide and theft. To capture heterogeneous incentives for potential offenders, we classify inequality measures based on their sensibility to changes in the different parts of the income distribution. The results show that none of the crime categories, nor the income inequality measures, are significantly associated in a statistical sense with the effect sizes. The meta-regression also reveals that including proxies for the benefits and costs of crime, such as poverty or GDP, does not have an impact on the size of the estimates. Nevertheless, we do find that the majority of regressions includes such variables, which could help explain the absence of an effect of inequality on crime. Additionally, we find that the inclusion of deterrence increases the effect sizes, which confirms the bias introduced when relevant variables are omitted. Moreover, cross-section studies have higher estimates, while those that include race-related variables lower.

The paper is organized as follow. Section 2 provides the theoretical background. It follows section 3 which presents the selection criteria used in the meta-analysis and the summary statistics. Section 4 presents the estimated effect sizes. Section 5 reports the results of the meta-regressions. Section 6 concludes.

2. Theoretical Background

The standard theoretical approach employed by economists to describe criminal behaviour is based on the rational choice under uncertainty. Crime is a gamble and individuals compare the expected utility from it with the certainty of the utility obtained in other, non-illegal, activities. Although Becker (1968) is thought to be the first to have modelled the rational choice model to explain criminal behaviour, it is Ehrlich (1973) who provided a specific theoretical framework, as well as empirical, to understand the role of inequality. In his occupational choice model, Ehrlich showed that criminal behaviour is driven by an increase in the average differential return from illegal activity, defined as the difference between illegitimate payoffs and legitimate wages. The former represent the benefits of crime, whereas the latter its costs, namely the opportunity costs. According to this framework, an increase in income inequality might lead to more crime either because the criminal payoffs increase- via greater incomes of the rich- or the opportunity costs of the poor decrease, or both simultaneously. Such an approach assumes that low-income individuals are those most likely to be the offenders, whereas the rich are the potential victims. Both assumptions enjoy some empirical support, especially the former (Machin and Meghir, 2004). Given its focus on the average differential return of illegal activities, the rational choice approach is considered to be more suitable to explain offences that involve an economic gain, such as property crime. Of course, offenders could get some utility by committing pure violent crimes but such possibility is not well studied. Following Becker (1968) and Ehrlich (1973)'s approach, the subsequent theoretical contributions on the income inequality crime relationship almost unanimously confirmed their positive associations. For example, Chiu and Madden (1998) showed that higher inequality unambiguously increases burglary crime. Similarly Josten (2003), İmrohoroğlu et al. (2000), Merlo (2003), Bourghignon (1999) relate positively crime and inequality. Few theoretical works explored the possibility that such a relationship might be more complex. For example, Bourguignon et al. (2003) said that, while inequality matters to study crime, the focus should be on specific parts of the income distribution, rather than the overall distribution.[1]

Along with the rational choice approach, scholars from other disciplines-mainly sociology and then criminology- have provided powerful theories to link economic inequality with criminal behaviour. For example, Merton's strain theory (Merton, 1938) assumed that a high level of inequality places economically unsuccessful individuals next to successful ones, spawning a sense of frustration and resentfulness by the former. In turn, this can lead to criminal behaviour. In a similar vein, the social disorganization theory assumed that the deterioration of social capital in a community is responsible for higher crime (Shaw and McKay, 1942). Income inequality, along with high levels of residential mobility and racial heterogeneity, can be a key factor in such a deterioration (Sampson and Groves, 1989;Blau and Blau, 1982).[2] One of the differences between the strain and the social disorganization theories is that the former assumes a direct link between inequality and crime, while the latter a mediated one, through lower social capital. Compared to rational choice, both theories are better suited to identify a link between income inequality and

[1]Another exception is Deutsch et al. (1992) who showed that the effect of higher inequality on crime might be ambiguous under certain circumstances. However, the author assumed that also the rich could be criminals which reduce their willingness to commit a crime because of a high loss if punished.

[2]Other theories that link inequality are presented in Matsueda and Grigoryeva (2014).

violent crimes.

Despite having a consensus from a theoretical point of view, the empirical evidence on the inequality-crime relationship presents contradicting results. Fajnzylber et al. (2002), one of the most cited studies in this literature, found that income inequality is associated with higher homicides and robbery, using a panel for 36 countries worldwide. In another highly cited study, Demombynes and Özler (2005), found that inequality is a strong predictor of property crime but much less of violent crime in South Africa. The author also found that the use of different level of geographical aggregation matters in explaining the role of inequality. Kelly (2000), based on US counties, found that inequality, measured by the Gini coefficient, did not have any effect on property crime whereas it had a strong, and significant effect, on violent crimes.[3] In a study based on US counties, Brush (2007) showed how a cross-sectional approach produced a positive effect whereas negative for time series. We will study the average effect of such a relationship found in the literature in 4.2. Before we present some arguments that could highlight why inequality might not necessarily cause crime:

Measurement of Crime. The majority of empirical studies employs official crime data, usually recorded by the police. As it is well established by the literature, these statistics suffer from under-reporting. The resulting measurement error would be not problematic if it was randomly related to crime determinants, but this is seldom the case. MacDonald (2001) highlighted how the victim's unemployment status affects the probability of reporting. Soares (2004) showed how the overall level of institutional development, which includes police presence, is positively related to crime reporting rates across countries. To attenuate such measurement error, many authors have employed the crime categories that suffer the least from this problem, mainly homicide rates. The use of the homicide rates could be questionable if the researcher aimed at testing the implication of the rational choice model, which supposedly better predicts property crime.[4] A further solution

[3]However it is interesting to note that the definition between violent and property crime for Kelly (2000) and Demombynes and Özler (2005) is different. For example, the former includes murder forcible rape, robbery, and aggravated assault as a violent crime. On the other hand, Demombynes and Özler (2005) includes aggravated assault and rape.

[4]Of course, homicide rates could be economically motivated, as discussed by Dix-Carneiro et al. (2018).

to this measurement problem could be to employ data from crime victimization surveys, which are less likely to suffer from reporting issues. Such statistics have been little used in the literature, especially because oftentimes they are not representative at sub-national levels.[5] How can the use of poorly measured crime data affect the income inequality crime relationship? Although it is difficult to provide a definite answer, a study by Gibson and Kim (2008) showed that measurement errors in official crime data overstate the role of inequality on crime: when data from the crime victimization are used, the role of economic inequality on crime is highly attenuated.

Measurement of Income Inequality. The measurement of income inequality in explaining crime, is probably even more challenging. First of all, there is a well-known difficulty related to the availability, and reliability, of data at the extreme ends of the income distribution (Saez and Zucman, 2016 and Schneider and Enste, 2000). Most importantly, the theoretical contributions do not offer, except for some exceptions, clear indications on how income inequality should affect crime. Consequently, there is no consensus on the income inequality measures to be used in the empirical analysis. In the first attempt to quantify the inequality-crime link, Ehrlich (1973) proxied the average differential return from illegal activity employing two separate measures: the mean income, which represented the benefits of crime, and the percentage of individuals who earned less than half of the median income, representing the cost of crime. The choice of the former might be questionable, because the use of the mean, or median income, could be considered as a proxy of both as the illegal payoffs and legitimate opportunities (Chisholm and Choe, 2005). The majority of the subsequent authors opted for employing a single measure of inequality to capture the average differential return from illegal activity. The choice of the correct measure is not easy: there are many different income inequality measures, each with particular features (Cowell, 2011). For example, income measures could differ in term of the sensitivity to changes in a different part of the income distribution. The majority of authors, as we will see shortly, employed the Gini coefficient, a measure more sensitive to changes in the middle of the income distribution. As such, its use could be little informative to describe crime incentives. For example, Bourguignon et al.

[5] In our sample, only 3.5% of all regressions have uses data from victimization surveys.

(2003) argued that the part of the distribution which affects most the income distribution was, for Colombia, composed of those individuals whose standard of living lied below 80 per cent of the mean. Measures that capture only changed at the top, such as the share of income held by the richest, are also little informative, as they exclude the role of opportunity cost. Additionally, there is no consensus on the measures of living standards to employ. In the vast literature on income inequality, it is well known that there are substantial differences between measures based on wealth, earnings, consumption, income, etc. Other differences might arise regarding the subjects of the analysis, households or individuals. Such a wide range of possibilities is reflected in the empirical literature. For example, Dahlberg and Gustavsson (2008) suggested using permanent income, rather than transitory. Hicks and Hicks (2014) used conspicuous consumption, proxied by visible expenditure. Chisholm and Choe (2005) suggested that the Gini coefficient should be multiplied by the mean income. Finally, Bhorat et al. (2020) suggested that the relationship between income inequality and crime might be nonlinear.

Omitted Factors. Many variables could be simultaneously affecting income inequality and crime levels. As mentioned earlier, the rational choice model interprets income inequality as capturing the average differential return from illegal activity. However, a single measure is unlikely to be able to capture the full spectrum of criminal costs and benefits. As such, it would be recommendable to include other variables that affect the incentives of crime. For example, poverty and unemployment rates, wages of the unskilled could be considered as a proxy for the cost of crime. On the other hand, some income measures related to the potential targets of the criminals should be included. Demombynes and Özler (2005) stated that, if all costs and incentives of crime are taken into consideration, the inequality measure should not necessarily be related to crime. Continuing, similar arguments could be made for another key variable: deterrence. This is likely to be negatively associated with the crime level (Di Tella and Schargrodsky, 2004; Evans and Owens, 2007; Bell et al., 2014). Deterrence is also likely to be associated with income inequality: an increase in the incomes of the rich provides incentives to invest in public, or private, protection (Merlo, 2003; Chiu and Madden, 1998; Jayadev and Bowles, 2006). Richer neighbourhoods may even have lower crime rates (Chiu and Madden, 1998). If protection measures divert criminals to the income deciles below the top ones, then the use of the inequality measure should reflect changes in these deciles. In general, as pointed out by Brush (2007), there might be many time-varying variables that cause income inequality and crime to move together, biasing the estimates. The use of fixed effects does not necessarily solve the omitted variable issues (Gibson and Kim, 2008). Another issue would be one of reverse causality (Barenboim, 2007 and Atems, 2020).

3. Description of the Data

The previous section challenged the unambiguity between inequality and crime. The next step is to provide a statistically-based overview of the existing literature through a meta-analysis (Stanley and Doucouliagos, 2012). We have two main objectives: the first is to analyze the average effect size, net of publication bias. The second is to study the heterogeneity among the estimates. These two objectives are complementary. Before presenting the tests and results, we explain how we selected the individual studies and the effect sizes employed in the meta-analysis.

3.1. Selection Criteria

The meta-analysis has been conducted following standard reporting guidelines (Havránek et al., 2020; Stanley et al., 2013). The academic works employed have been selected according to the following criteria:

Title of Paper. We include those studies whose title includes both the word inequality and crime. It is accepted the use of words that echo such concepts, such as income dispersion or concentration, for inequality, or criminal activity, illegal behaviour or violence, for a crime. For example, Brzezinski (2013)'s title "Top income shares and crime" is accepted. We exclude all those studies that include inequality as a regressor but whose principal focus, using the title perspective, is not inequality, or income dispersion. We do so because we are interested in works that focus explicitly on income inequality and crime relationship.

Crime as a Dependent Variable and Inequality as a Regressor. We include results from any study (j) with at least one econometric regression (i) that takes the following form:

$$Crime_{i,j} = \alpha + \theta Inequality_{i,j} + \gamma X_{i,j} + \varepsilon_{i,j} \quad (1)$$

where *Crime* might be any crime measures, whether economically motivated or not. The analysis includes single crime categories as well as aggregate crime indexes.[6] *Inequality* refers to any income-related measure of inequality, such as wages, wealth and consumption/expenditures.[7] We exclude other types of inequality, based on education for example. If a study reported income and non-income related inequality measures, we retain only the results from the former and discard the second, such as for Kelly (2000). *X* represents a series of control variables employed in the regressions, whose role will be analyzed insection 5. Those studies which exclusively contain descriptive statistics, or qualitative, reviews are excluded.

Search Engines & Discipline. We have searched studies, written in English, through academic search engines. We entered keywords such as "inequality/inequitable development/income distribution" and "crime/criminal activity(ies)/illegal behavior". We employed the engines *Google Scholar*, *Research Gate*, *ISI web of science* and *Econlit*. We first collected primary works, i.e., those most known, in terms of citations or relevance of the authors. We thoroughly read these studies' literature review sections to find cited works that satisfied our criteria. We also reviewed the studies that cited these primary works, using *Google Scholar*, *Research Gate* and *Social Science Citation Index*.[8] We collected studies published in academic journals, referred and non-referred, and also on working paper series.[9] We include only studies in the economics academic field. We define economic journals as identified with IDEAS/RePEc (2018a). Similarly, we consider works that appear in economics working paper series, as defined by IDEAS/RePEc (2018b). The

[6] For example Menezes et al. (2013) focused exclusively on homicides, whereas (Song et al., 2020) used an aggregated measure.

[7] We consider the inequality measure used by Buonanno and Vargas (2019), based on land, as a proxy for wealth.

[8] We did not restrict the search to any period and we concluded on the twenty-second of February 2021. The inputs of the database would be revised by two research assistants.

[9] Despite that many of the published works first appeared as working papers, we decided to use the published version, whenever available. The reason is that authors usually release many versions of a study before sending it for publication. This would have forced us to make a choice on which study to use, which would have introduced additional biases.

main reason to focus on economics is to delimit the area of our search while studying in-depth the literature within a single discipline. By doing so, we can include studies that would have been overlooked in another meta-analysis approach. Considering only these studies also increases the likelihood that a regression includes the key standard rational choice model variables, such as deterrence (Becker, 1968).[10]

[Table 1 ABOUT HERE]

The search has produced 47 studies: 38 in journals and 9 in working papers series. A complete list of the works included can be seen in Table 1.[11]

3.2. Effect Sizes

Some regressions might specify the crime variable in rate, whereas others in level; rates might be calculated per 10,000 or 100,000 inhabitants; some specifications use logs whereas other do not. The same applies for the inequality measures, where we have an even greater degree of heterogeneity, with different measures and scales. Given that we cannot readily compare the θs among all the studies, we transformed them into partial correlation coefficients. These are unitless measures of the "strength and direction of the association between two variables, but it holds other variables constant. That is, a partial correlation coefficient provides a measure of association, ceteris paribus" (Stanley and Doucouliagos, 2012: p.25). The formula to calculate them is:

$$Part\ Corr\ Coeff_{i,j} = \frac{t - statistics_{i,j}}{\sqrt{t - statistics_{i,j}^2 + Degrees\ of\ Freedom_{i,j}}} \tag{2}$$

Standard errors are calculated in this way:

$$Standard\ Error_{i,j} = 1 - \sqrt{(1 - Part\ Corr\ Coeff_{i,j}^2)/Degrees\ of\ Freedom_{i,j}}. \tag{3}$$

[10]We are aware that such restrictions could potentially lead to some biases. However, we think that using, indiscriminately, studies from all other fields, would create even more of such problems. Many other meta-analysis apply restrictions, whether to publication status, period considered, etc.

[11]The database employed in this study is available upon request.

Where the *t-statistics*$_{i,j}$ represents the t-test of the θ. Whenever the test statistic was not reported we calculated it by dividing the coefficient by the standard error. Similarly, if only the p-values were reported, we calculated the test statistics. Sometimes the authors reported only the levels of significance without further details. In such cases, we assigned 0.01, 0.05 and 0.1 if the coefficients were significant at the 1%, 5% and 10% respectively. One of the possible drawbacks of using partial correlations is that the standard error depends on the correlation coefficient itself. To take this issue into account, we convert the correlation coefficient into Fisher's z units. We report all the results using both partial correlations and Fisher's z.[12] The use of partial correlation coefficients, and Fisher's z statistics, is widespread in meta-analysis (Havránek, 2015; Heimberger, 2020). We also drop the coefficients for the interaction of inequality with other variables, and inequality expressed in a polynomial of second degree or higher. [13] For studies that employ impulse response functions, we consider the first three periods. .[14]

As the main analysis, we report all θ coefficients from all the regressions, that meet the criteria, in the 47 studies that regress crime against an inequality measure. We decided to do so to increase the number of observations and model heterogeneity in section (5). By reporting only one coefficient per study, we would have lost important within-study heterogeneity, especially on deterrence. Moreover, Bijmolt and Pieters (2001) showed that using multiple measurements per study is to be preferred for detecting the "true" underlying population effect size. Using all effects causes within study dependence which we will address using cluster standard errors. As robustness, we will estimate the effect sizes only with the mean values for each study.

The final number of effect sizes in our analysis is 1,431, slightly less than thirty per study. 1,052 are zero or positive, about 73.52%. Nevertheless, there is a high degree of heterogeneity

[12]We would have preferred to employ elasticities as main effect sizes because they represent an economic measure rather than statistical, as the partial coefficients. However, only twenty per cent of all estimates in our analysis measures both the crime and inequality in log form. In the main text, we provide some estimates using them.

[13]These represent 55 estimates. Although we do not report the exercises, when we also employ such estimates the results are unchanged.

[14]Whenever the results are not reported in a table, we measured the estimates, and standard errors, from the graph. This is possible only if the graph reports the confidence intervals bounds. This technique has been employed previously in meta-analysis studies (Nguyen, 2020).

among studies as can be seen in Figure 2.

[Figure 2 ABOUT HERE]

4. The Effect of Inequality on Crime, net of Publication Bias

4.1. Graphical Analysis

In this section, we graphically explore the role of publication bias. We need to take care of such bias to estimate correctly the impact of inequality on crime. A standard way to do so is through a funnel graph, which consists of plotting the effect size on the horizontal axis and its standard error, on the vertical axis. The most precise estimates will be at the top of the graph and are less likely to be susceptible to publication bias. Less precise effect sizes will be distributed widely at the bottom of the graph. The intuition is that when researchers have large studies, i.e., with many observations, they are less inclined to look for statistically significant results and will report smaller estimates. On the other hand, if publication bias exists, researchers using smaller samples need to try harder to find statistically significant results that support their theory. When there is no publication bias, the results are independent of their standard error and should be symmetrical around the most precise estimates. If the graph is asymmetrical, with more estimates concentrated on one side, it means that the researcher has some preference over some results.

We report the funnel graph for the inequality-crime relationship in Figure 3a. Taken as a whole, the graph is roughly funnel-shaped, although it appears to be some degree of positive publication bias.

[Figure 3a and Figure 3b ABOUT HERE]

Despite the emergence of clear patterns in the graph, we need to rely on the formal test to estimate correctly the impact of inequality on crime among groups.

4.2. Formal Tests

In this subsection we aim at finding the precise estimate of the effect size of the inequality-crime regressions, controlling for the presence of publication bias. In other words, we run a regression with the effect size as the dependent variable, whereas the constant and the coefficient's standard errors are the independent variables. This is the well-known Funnel Asymmetry Effect Test, or FAT-PET (Egger et al., 1997). The regression model we are testing is thus this one:

$$\textit{Effect Size}_{i,j} = \lambda_0 + \lambda_1 \textit{Standard Error}_{i,j} + \varepsilon_{i,j} \tag{4}$$

Where, again, *i,j* stands for the *ith* estimates in the *jth* study. As *Effect Size* we consider the partial correlation coefficient and the Fisher's z. If the literature on inequality and crime is free of publication bias, the coefficient λ_1 should not be statistically significant. λ_0 represents the true effect of inequality on crime, i.e., net of publication bias.[15]

As Ioannidis et al. (2017) pointed out, such regression suffers from heteroskedasticity because the conditional distribution of the errors for the standard errors is not constant. As a result, the OLS estimator is not BLUE anymore. To take this into account, the accepted practice in meta-analysis is to employ the generic inverse-variance method, which provides greater weights to more precise estimates. There is still debate on the best meta-analytical econometric models. We report the results using three different econometric techniques: unrestricted weighted least squares (WLS), random-effects (RE) and hierarchical-multilevel (Mixed). These models make different assumptions on how to weigh the effect sizes variances. Stanley and Doucouliagos (2015) showed that the unrestricted weighted least squares method, with its multiplicative variance structure, is to be preferred to random effects, which has an additive variance structure when there is publication bias. Unrestricted weight least squares are also preferred to fixed effects in the presence of heterogeneity, as this model does assume there is no excess heterogeneity. Taking this into consideration,

[15] Alternatively, we could have estimated the precision-effect estimate with standard error specification-PESEE-specification, which include the variance of the effect size. We decided not to employ such a test because Stanley and Doucouliagos (2012) showed that when there is a tiny true effect or no effect at all, the FAT-PET provides less biased and accurate estimates. As Table 2 shows, this is our case.

our preferred estimation technique is the unrestricted WLS.[16]

[Table 2 ABOUT HERE]

Column (1) and (2) of Table 2 show the results for the partial correlation coefficients and Fisher's z using unrestricted weighted least squares. We also report the results without controlling for the standard errors -in Panel A- to appreciate how the estimates change when publication bias is taken into account. The effect size without controlling for the standard error is small, positive and statistically different from zero using a 90% confidence interval. When we control for the standard error, this value (the intercept, or λ_0) is about three times smaller than that size, 0.0086. The publication bias, given by the coefficient of SE- the λ_1- is 1.227 and significant at the 1% level. According to Stanley and Doucouliagos (2012), a coefficient greater than 1 reflects some relevant degree of selection bias. In column (3) and (4) we report the results with random effects, which applies a residual maximum likelihood (REML) technique to estimate the additive (between-study) component of variance. The coefficient without controlling for publication bias is higher than the one with the unrestricted weighted least squares and highly significant. However, when we control for publication bias, the coefficients loses significance and become smaller than the ones with WLS. In (4), with Fisher's z as the dependent variable, the coefficient is even negative, although not statistically significant. In (5) and (6), we consider a mixed-effect model, which could be considered as a three-level meta-analytic model, where individual coefficients are nested within studies. This method provides coefficients somewhat greater than other specifications in Panel A. However, controlling for publication bias, the coefficient becomes much smaller, to 0.0555 for partial correlation. Finally, in (7) and (8) we consider one observation per study, using the average means and standard errors. In such a way, we can be further reassured of the independence of observations. We report the results with an unrestricted weighted least squares model, as it is our preferred econometric model. The results reveal that the coefficient estimates are lower than the

[16] We do not report the results with fixed effects, because the coefficients are identical to the ones with unrestricted weighted least squares, although with different standard errors.

ones found in column (1) and (2). The presence of publication bias is also significant. Overall, all results confirm that the effect sizes are statistically and economically small.

As a robustness exercise, we perform a ”trim and fill” method, which consists of removing the effect sizes which are causing the funnel plot to be asymmetric (Duval and Tweedie, 2000). Then, a new estimate is calculated using only the effect sizes in the trimmed funnel plot, free of publication bias. Finally, using the new corrected estimate, the missing coefficients are imputed to fill the funnel plot. Using such a method, and applying a WLS with partial correlations, we find a corrected estimate of 0.0109, close to 0.0086, the one found with the FAT-PET test. In Figure 3b, we show the funnel graph with the imputed coefficients. Although we do not report the result in the table, using the sub-sample with the elasticities, and controlling for publication bias, we find an average value of 0.202 which is statistically insignificant.

Overall, our findings show that the effect of inequality on crime is very close to being zero. These are different from the ones found in previous meta-analysis studies. Kim et al. (2020) performed a meta-analysis on the relationship between inequality and crime, based on cross-sectional studies. The authors employed a random effects model and found that the weighted mean of the effect size, the Fisher’s z, was 0.413. Contrary to our study, Kim et al. (2020) did not find evidence of publication bias. In another meta-analysis, Pratt and Cullen (2005) found inequality, among variables, to be an important predictor of homicides. These works are not easily comparable to ours as the studies included are largely different. To further understand the presence of heterogeneity in the income inequality- crime relationship, we conduct a meta-regression in the next section.

5. Meta-Regression Analysis

In the theoretical section, we highlighted several factors which might explain a limited impact of income inequality on crime levels. We discussed the potential role of different inequality and crime measures, as well as the exclusion of key control variables. In this section, we formalize such arguments and run the following meta-regression:

$$Effect\ Size_{i,j} = \lambda_0 + \lambda_1 Standard\ Error_{i,j} + \lambda_k Moderators_{i,j} + \varepsilon_{i,j} \tag{5}$$

Moderators are the variables that help to explain the sign and direction of the effect sizes. We divided the moderators' variables into the following categories:

Crime Variables. In the baseline specification we consider the most relevant crime categories: *Homicide*, *Robbery*, *Burglary* and *Theft*. *Homicide* is the most frequent category with 26%. It is followed by *Theft* with 14% and *Robbery* and *Burglary*, each 7%. As an alternative specification we classify crime into three main categories: *Property Crime*, *Violent Crime* and *Total Crime*. The former includes all crimes where there is a tangible economic loss, independently of the use of violence. Accordingly, a robbery would be considered as a property crime (Hernández et al., 2017). On the other hand, we consider a crime as violent if a) no violence was employed; b) there was no intention to gain pecuniary benefits. Examples of violent crimes are assaults or rapes. We include homicide in this latter category, although in some cases they could be economically motivated. We decided to classify crime according to such criteria to capture economically motivated offences, which should adhere better to the rational choice model. Still, it is difficult to draw a line between types of crime. *Total Crime* includes crime indexes, or groups of specific crimes, that are both violent and economically motivated. As shown in Table 3, the relative majority, 41%, of all the crimes are *Property Crime*, followed by *Violent Crime*, with 35%, and *Total Crime*, with 24%. Additionally, we categorize crimes following the approach used by the agency FBI: a crime is a property if an economic gain was obtained without the use of violence. Accordingly, theft would be a property crime whereas a robbery would be a violent one. This classification causes violent crime to be the most frequent category with 57%.

Inequality Measures. As mentioned in the theory section, there is no consensus on the most appropriate measures to use. In the baseline specification, we separate them based on the sensibility to changes at different parts of the income distribution (Bourguignon et al., 2003). We classify the inequality measures into three groups: *Inequality Bottom Wt*, *Inequality Middle Wt* and *Inequality Top Wt*. As the variables' names suggest, the first gives more weights to changes at the bottom of the income distribution, the second to the middle and the last to the top. *Inequality Bottom Wt* includes general entropy with α -the weight given to distances between incomes at different parts of the income distribution- equal zero and one, including the Theil index; the Atkinson measures with ε - a measure of inequality aversion- equal to one and two; any measure of decile dispersion ratio and all the poverty measures. *Inequality Equal Wt* includes the Gini coefficient; the general entropy with α equal to two; Atkinson with ε equal to 0.5, and income polarization.[17] Finally, *Inequality Top Wt* includes the income share held by the richest and the variance of incomes. We also consider the most relevant inequality measures separately, namely the Gini coefficient, *Theil GE* which includes all types of general entropy measures and *% Held by Richest*, the share of income owned by the richest segment of the population. *Inequality Equal Wt* is, by far, the most frequent category, driven by *Gini* which is the most used inequality measure (58%), followed by *Theil & GE* (14.1%).

Cost and Benefits of Crime; Deterrence. We included three income-related variables which capture the costs and benefits of crime: unemployment rate, poverty and GDP(or a proxy). The latter is the most frequent control variable, employed in 74% of the regressions. Interestingly, only 43% include a measure of poverty, which is recognized to be highly related to inequality. About 91% of the regressions contains, at least, one of these three variables. We also include a binary variable equals to one of the regression controls for deterrence. Around 51% of all the studies include it, usually a measure of the police workforce. As we mentioned earlier, deterrence might be related both to inequality and crime.

Study Characteristics. We include several variables that capture the studies' characteristics. For example, we consider the number of years since the work came out, *Years*. In such a way we can evaluate whether there have been trends over the years, especially considering that many important studies were published at the beginning of the 2000s. We also include a binary variable equal to one if the study is published in an academic journal or not. Finally, we also control for the

[17] Of course, income inequality and polarization are different, although related, concepts (Esteban and Ray, 1994). It is therefore challenging to classify polarization measures within our framework.

number of Google citations, as of the twenty-second of February 2021. The top three cited works are: Fajnzylber et al. (2002)- 1407 citations-, Kelly (2000)- 1035 citations- and Demombynes and Özler (2005)- 476 citations.

Data & Econometric Issues. We include the variable *Single Country* which takes value one if the study is based on a single country. Its mean value is 0.87, which reflect the difficulty to compare crime statistics among different countries. We also include a dummy variable equal to one if the study focuses exclusively on the *USA* or *China*. About 39% of the total number of regression's coefficients are from this country. The second most represented country is China, with 11%. We consider the variable *Cross Country* which takes value one if the study is a cross-section. Only 13% of the regressions are pure cross-section, while the rest are mainly panel data. Additionally, we consider a binary variable equal to one whether the estimation technique is OLS and zero otherwise. We also include a binary variable that takes value one whether an instrumental variable approach was used. This includes techniques such as two-stage least squares or dynamic GMM.[18] About 28% of all regressions explicitly take care of the possible endogeneity of the inequality measure. Finally, we include a binary variable equal to one if the regression includes time dummies.

Other Relevant Control Variables. We include a binary variable equal to one if the percentage of female heads was included. This variable is a standard measure of deprivation in the economics of crime literature (Glaeser and Sacerdote, 1999). It has been included in 30% of the regressions. Finally, we include a binary variable if the regression controls for a race-related variable.

A description of the variables, their unweighted means and standard errors can be found in Table 3.

[Table 3 ABOUT HERE]

We also include the standard error to control for the presence of publication bias. Notwithstanding that we can still interpret λ_1 as the publication bias, we cannot do the same for λ_0 as

[18]We also included the Panel SVAR model employed by Atems (2020) because it specifically deals with reverse causality.

the true effect is now replaced by $\lambda_0 + \lambda_k Moderators_{i,j}$ (Stanley and Doucouliagos, 2012).[19] Our results should be interpreted merely as correlations, not as causal (Anderson and Kichkha, 2017). We run (5) using an unconstrained weighted least squares model, both for partial coefficients and Fisher's z statistics.[20].

We start in Table 5, which reports the full models for each effect size- partial correlations and z score.[21] In column (1) and (2), we include four crime categories- *Homicide*, *Robbery*, *Theft* and *Burglary*- along with *Inequality Bottom Wt* and *Inequality Equal Wt*, leaving *Inequality Top Wt* as the excluded category. As we can see, there are no significant differences, at least at the conventional statistical level, between crime and inequality measures. Considering column (1), *Homicide*, *Robbery*, *Theft* and *Inequality Bottom Wt* have a positive sign, while *Burglary* and *Inequality Equal Wt* positive. However, we cannot comment on these signs, given the relatively high p-values. In column (3) and (4) we exclude *Inequality Equal Wt*. The coefficient for *Inequality Top Wt* is positive but highly insignificant. Finally, in column (5) and (6) we interact with the crime and inequality measures to study the impact of various inequality/crime combinations on the effect sizes. This analysis shows that such interactions is significant only for *Homicide*Ineq Equal* with a positive sign. For the other crime categories, the sign of the interactions with *Inequality Bottom Wt* is always positive,with *Inequality Equal Wt* is negative.

Moving to the moderator variables, we do find that the inclusion of proxies for the costs and benefits of crime does not help to explain the heterogeneity of the effect sizes. *Unemployment* and *Poverty* have a positive sign, while *GDP/Income* is generally negative. Nevertheless, these are imprecisely estimated. On the other hand, the inclusion of a deterrence measure is associated with higher coefficients. This is consistent with the fact that deterrence should be negatively correlated with crime and positively with inequality, suggesting a negative bias in models that exclude it. *Years* is negative although not statistically significant. Interestingly, whether the work is published in an academic journal increases the effect size, but such a coefficient has a p-value higher than

[19]The coefficient λ_0 would represent the true effect if all the moderators variables were zero.
[20]We do not report the results with the other models, which are available upon requests
[21]The results with the other econometric techniques are similar and available upon requests.

0.1. The number of Google Scholar citations does not seem not to have any effects. Estimates from studies using data on a single country report lower estimates, although the result is slightly insignificant. That could be partially explained by the fact that cross-country analysis is likely dominated by the presence of South American countries that have both a high level of inequality and crime. Additionally, we do not find that employing data from the USA, or China, is affecting the magnitude of the effect size. Continuing, studies with a cross-sectional structure have positive coefficients, and significant at the 10% level. This seems to confirm the intuition by Brush (2007) that cross-sectional studies might report greater coefficients than panel data or time series. Controlling for time fixed effects decreases both partial correlations and Fisher's z, but the estimates are not statistically significant. Regarding the estimation technique, we do find that the sign for *OLS* and *Instrumental Variables* are negative but not significant. This latter suggests that, on average, the typical omitted variable bias might be positive.

[Table 5 ABOUT HERE]

Estimates in regressions that control for *Female Head* are positive but statistically not different from zero. However *Race*, ceteris paribus, is associated with lower effect sizes. This suggests a positive bias in the models that do not include it. Finally, the coefficient for the standard errors- an indicator of the role of publication bias- is positive and never significant. Controlling for various factors captures a significant proportion of the heterogeneity of the effect sizes.

In Table 6 we perform further tests, varying the crime and inequality measures.[22] In column (1) and (2), we include *Property Crime*, *Violent Crime*, leaving *Total Crime* as the excluded category. The coefficients are small, negative and not statistically significant for both categories. *Per se* , such results are not extremely revealing because *Total Crime* includes both property and violent crime. However, it is interesting to note is that there are no statistically significant differences between *Violent Crime* and *Property Crime*. This is a further confirmation that inequality is not necessarily associated with higher estimates for economically motivated crime. Turning to the

[22]To save space, we do not report the whole set of other moderator variables, which are included in all regressions. Results are available upon request.

inequality measure, the results are similar to the previous table, with *Inequality Bottom Wt* with a bigger sign than *Inequality Equal Wt*. In column (3) and (4) we interact *Property Crime* and *Violent Crime* with the *Inequality Bottom Wt* and *Inequality Equal Wt*. These interactions show that the coefficients associated with *Inequality Bottom Wt* are greater in size compared to the ones with *Inequality Equal Wt*, although not statistically significant. Changes at the bottom of the distribution might be associated with greater effect sizes because the majority of potential criminals are coming from the left part of the income distribution. In column (5) and (6) we employ the FBI classification and find that both property and violent are negatively related to the effect sizes. The baseline category, *Total Crime FBI* represent a mixture of the two types of felonies. Finally, in (7) and (8) we consider separate inequality measures, namely *Gini*, *Theil & GE* and *% Held by the richest*. In this case, we do not find that any of them affects the size of the effect.

6. Conclusion

There exists a wide consensus that income inequality causes crime. The disparity in incomes is thought to create incentives for the poor to steal from the rich. Although this argument is compelling, the existing empirical literature fails to find any unambiguous effect. To better investigate such a relationship, we conducted a meta-analysis employing the regression estimates from the studies in this literature. Our analysis is based on 47 studies and 1,431 estimates. The main findings are that the effect sizes, after controlling for publication bias, are small, economically and statistically insignificant. In other words, we do not find that, on average, income inequality is having the effect presumed in the theoretical literature. We identify mainly three potential reasons that could explain such result: measurement errors in the crime variables; misspecification of the income inequality measures; and the presence of relevant omitted variables. Additionally, we run meta-regressions to study whether specific crime categories or income inequality measures affect the size and magnitude of the effect sizes. We do not find a significant association between such variables, when taken independently. We also find that the cross-sectional studies and regression that include proxies for deterrence and race significantly impact the effect sizes.

There are direct implications from our results. First of all, there should be an effort to obtain better measurements of crime and income inequality. For example, the use of victimization surveys could reduce the errors in reporting crimes, especially property ones. Additionally, it would be useful to determine which are the most appropriate income inequality measures in order to better capture the incentives of committing an offence. Inequality measures offer the advantage of providing the net benefits of crime in a single measure. However, they also challenge the researcher to identify simultaneously who is most likely to commit an offence and who might be a victim. A better understanding of the relationship between income inequality and crime will allow to design better policies and welfare programmes. Governments could thus focus on the income distribution's segments associated with a greater propensity to commit an offence. Identifying what happens when there is change at the top of the income distribution should also have an impact on policies. If increases in income inequality change the incentives of the rich to invest in protection, then some criminal activity might be diverted to the income deciles next to the top ones (Decreuse et al., 2015). The government should take into account such negative externality via greater public protection or through tax breaks for buying private protection.

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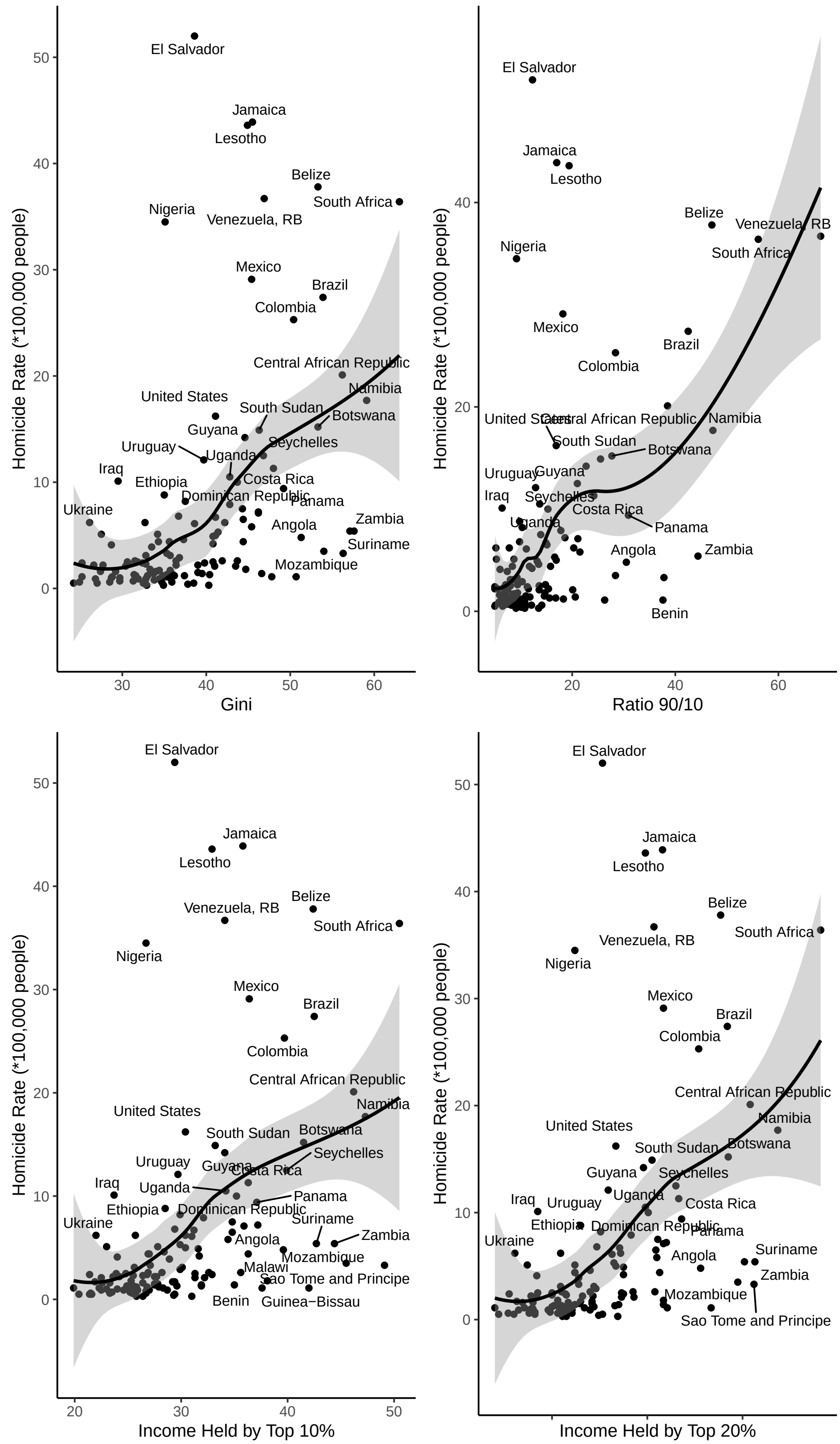
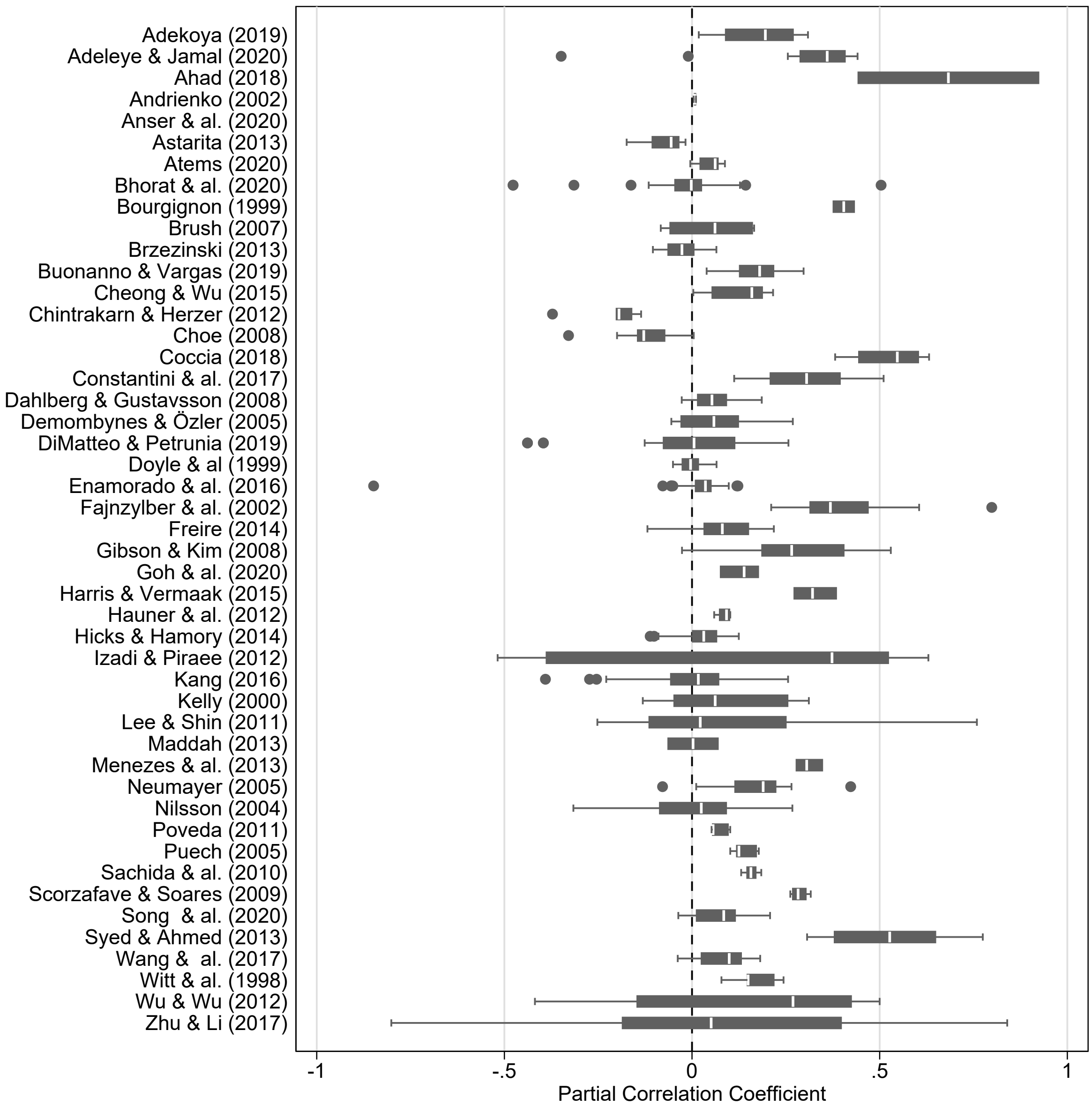
Figure 1: Homicide Rates and Inequality Measures, International Comparison



This figure shows four scatter plots with homicide rates versus different measures of inequality. The blue line represents the smoothed conditional mean. Data for homicides have been taken by the UNODC, while the data for inequality from the World Bank. The latest available year for each variable is considered.

Figure 2: Unweighted Partial Correlations per Study



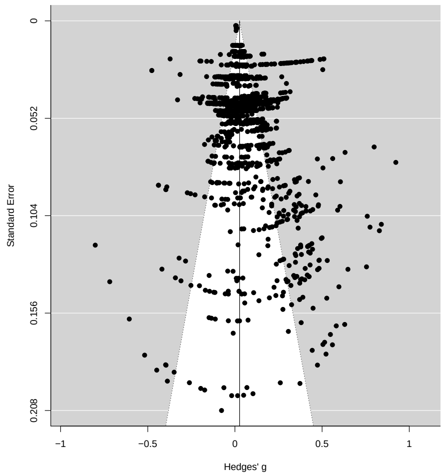
This figure shows an h-box with the average of the partial correlation coefficients and standard errors for each study.

Table 1: Summary of the Studies in the Meta-Analysis

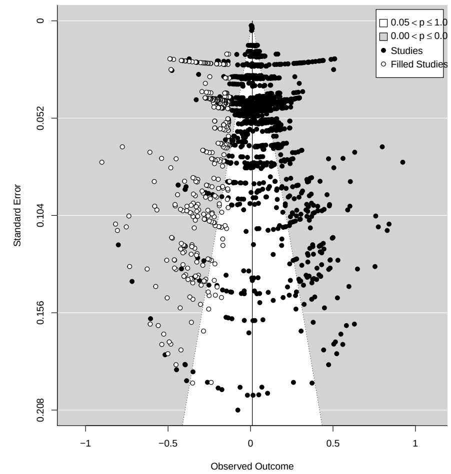
Study	Crime Variables	Inequality Measure
Adekoya (2019)	Total	Gini
Adeleye and Jamal (2020)	Violent	Gini
Ahad (2018)	Total	Gini
Andrienko (2002)	Property, Violent	Gini
Anser et al. (2020)	Violent	Gini
Astarita (2013)	Property, Violent	Gini, Quintile Ratio, Theil
Atems (2020)	Property, Total	Gini, Top Share, Theil
Bhorat et al. (2020)	Property	Atkinson, General Enthropy, Gini
Bourghignon (1999)	Property, Violent	Gini
Brush (2007)	Total	Gini , Top Share
Brzezinski (2013)	Property, Violent	Top Share
Buonanno and Vargas (2019)	Property, Violent , Total	Atkinson, Gini, Theil
Cheong and Wu (2015)	Total	Gini, Quintile Ratio
Chintrakarn and Herzer (2012)	Property	Gini , Top Share
Choe (2008)	Property, Violent	Gini
Coccia (2018)	Violent	Gini
Costantini et al. (2018)	Property, Violent	Gini , Top Share
Dahlberg and Gustavsson (2008)	Property, Total	Gini, Variance
Demombynes and Özler (2005)	Property, Violent	General Enthropy
Di Matteo and Petrunia (2019)	Violent	Gini, Quintile Ratio
Doyle et al. (1999)	Property, Violent	Gini
Enamorado et al. (2016)	Property, Violent	Gini
Fajnzylber et al. (2002)	Property, Violent	Gini, Quintile Ratio, Polarization
Freire (2014)	Violent	Quintile Ratio
Gibson and Kim (2008)	Property, Violent	Gini
Goh et al. (2018)	Violent	Gini, EHII
Harris and Vermaak (2015)	Violent	Gini
Hauner et al. (2012)	Property, Violent , Total	Variance
Hicks and Hicks (2014)	Property, Violent	Gini
Izadi and Piraee (2012)	Property	Atkinson, Gini
Kang (2016)	Property, Violent , Total	Gini, Theil
Kelly (2000)	Property, Violent , Total	Gini
Lee and Shin (2011)	Property	Gini, Polarization
Maddah (2013)	Total	Quintile Ratio
Menezes et al. (2013)	Violent	Gini
Neumayer (2005)	Total	Gini, Quintile Ratio
Nilsson (2004)	Property, Violent , Total	Poverty
Puech (2005)	Property, Violent , Total	Gini
Poveda (2011)	Violent	Gini
Sachsida et al. (2010)	Violent	Gini
Scorzafave and Soares (2009)	Property	Gini
Syed and Ahmed (2013)	Property	Gini
Song et al. (2020)	Total	Quintile Ratio
Wang et al. (2017)	Total	Gini, Polarization, Theil
Witt et al. (1998)	Property	Quintile Ratio
Wu and Wu (2012)	Property, Violent	Gini
Zhu and Li (2017)	Property, Violent , Total	Quintile Ratio

This table classifies the crime and inequality measures according to the criteria based on monetary loss (see the text for more detail). As such, they do not necessarily reflect the categories used by the authors, especially for crime measures.

Figure 3: Funnel Graph: All Estimates and ”Trim and Fill”



(a) Funnel Graph



(b) Funnel Graph using Trim and Fill

Figures (a) and (b) are funnel plots. The partial correlation coefficients are on the x axis and the standard errors are on the y axis. Figure (a) includes all the 1,431 estimates from the 47 studies. The line represents the average partial correlation estimates, employing unrestricted weighted least squares. Figure (b) additionally includes the imputed effect sizes obtained through the ”trim and fill” procedure.

Table 2: Estimating the Effect Sizes

	WLS-PC (1)	WLS-Z (2)	RE-PC (3)	RE-Z (4)	Mixed-PC (5)	Mixed-Z (6)	WLS-PC,A (7)	WLS-Z,A (8)
	Panel A: Without Considering Publication Bias							
Constant	0.0266*	0.0255*	0.0677***	0.0682***	0.1077***	0.1103***	0.0192	0.0189*
	(0.0149)	(0.0138)	(0.0039)	(0.0041)	(0.0212)	(0.0220)	(0.0116)	(0.0113)
	Panel B: FAT-PET							
Constant	0.0086	0.0069	0.0097	-0.0117	0.0555	0.0635*	0.0009	0.0001
	(0.0074)	(0.0057)	(0.0077)	(0.0078)	(0.0290)	(0.0295)	(0.0044)	(0.0042)
SE	1.2270***	1.2633***	1.1383***	1.5993***	0.8234*	0.7298*	1.8044***	1.8535***
	(0.3891)	(0.3951)	(0.1313)	(0.1368)	(0.3640)	(0.3614)	(0.3897)	(0.3951)
Observations	1431	1431	1431	1431	1431	1431	47	47

This table shows the estimates of the effect sizes- partial correlation (PC) and Fisher's z (Z)- using alternative estimation methods. Column (1) and (2) employ a weighted least square model using all estimates from the 47 studies. Column (3) and (4) use random effects with all the estimates. Column (5) and (6) employ a hierarchical model with effect sizes nested within studies. Finally, column (7) and (8) reports weighted least square results for the average estimates and standard errors for each study. Errors are clustered at the study level for the unrestricted weighted least square model. *,**, *** represent significance at the 10%, 5% and 1% levels, respectively.

Table 3: Moderator Variables

Variable	Definition	Mean	Std. Dev.
Homicide	Crime is Homicide	0.26	0.44
Robbery	Crime is Robbery	0.07	0.26
Theft	Crime is Burglary	0.14	0.35
Burglary	Crime is a Theft	0.07	0.26
Property Crime	Property Crime, Monetary Loss	0.41	0.49
Violent Crime	Violent Crime, Monetary Loss	0.35	0.48
Total Crime	Total Crime, Monetary Loss	0.24	0.43
Property Crime FBI	Violent crime, FBI Classification	0.29	0.46
Violent Crime FBI	Property Crime, FBI Classification	0.57	0.50
Total Crime FBI	Total Crime, FBI Classification	0.14	0.35
Inequality Equal Wt	Equally Weighted Inequality Measure	0.61	0.49
Inequality Bottom Wt	Bottom Weighted Inequality Measure	0.32	0.47
Inequality Top Wt	Top Weighted Inequality Measure	0.07	0.25
Homicide*Ineq Bottom	Interaction Homicide*Inequality Bottom Wt	0.07	0.25
Homicide*Ineq Equal	Interaction Homicide*Inequality Equal Wt	0.19	0.39
Theft*Ineq Bottom	Interaction Theft*Inequality Bottom Wt	0.04	0.20
Theft*Ineq Equal	Interaction Theft*Inequality Equal Wt	0.08	0.28
Burglary*Ineq Bottom	Interaction Burglary*Inequality Bottom Wt	0.02	0.15
Burglary*Ineq Equal	Interaction Burglary*Inequality Equal Wt	0.04	0.20
Robbery*Ineq Bottom	Interaction Robbery*Inequality Bottom Wt	0.02	0.14
Robbery*Ineq Equal	Interaction Robbery*Inequality Equal Wt	0.04	0.21
Theil & GE	General Enthropy Inequality Measures	0.14	0.35
Gini	Gini Inequality Measure	0.58	0.49
% Held by Richest	Income % held by the richest percentiles Inequality Measure	0.05	0.22
Unemployment	Control for Unemployment	0.64	0.48
GDP/Income	Control for GDP Income	0.74	0.44
Poverty	Control for Poverty	0.43	0.50
Deterrence	Time dummies are included	0.51	0.50
Years	Years since paper was published or uploaded in working paper series	7.38	5.32
Published	Paper has been published in an academic journal	0.81	0.39
Google Citations	Total number of Google Scholar citations	94.08	217.02
Single Country	Single Country Study	0.87	0.34
USA	Data from the China	0.39	0.49
China	Data from the USA	0.11	0.31
Cross Section	Data are cross sectional	0.13	0.34
OLS	Econometric technique is OLS	0.18	0.39
Instrumental Variables	Use of Instrumental Variables	0.28	0.45
Time FE	Control for Deterrence	0.48	0.50
Female Head	Control for Female Head	0.30	0.46
Race	Control for Race	0.40	0.49

Table 4: Meta-Regressions, Baseline

	PC	Z	PC With Top	Z With Top	PC Int	Z Int
	(1)	(2)	(3)	(4)	(5)	(6)
SE	0.4781	0.6902	0.4781	0.6902	0.5602	0.7801
	(0.6198)	(0.6353)	(0.6198)	(0.6353)	(0.6365)	(0.6544)
Homicide	0.0329	0.0310	0.0329	0.0310	-0.0323	-0.0342
	(0.0306)	(0.0295)	(0.0306)	(0.0295)	(0.0314)	(0.0317)
Robbery	0.0281	0.0242	0.0281	0.0242	0.0519	0.0525
	(0.0296)	(0.0283)	(0.0296)	(0.0283)	(0.0496)	(0.0485)
Theft	0.0109	0.0100	0.0109	0.0100	0.0619	0.0620
	(0.0094)	(0.0090)	(0.0094)	(0.0090)	(0.0433)	(0.0411)
Burglary	-0.0048	-0.0023	-0.0048	-0.0023	-0.0283	-0.0257
	(0.0360)	(0.0339)	(0.0360)	(0.0339)	(0.0639)	(0.0622)
Inequality Equal Wt	-0.0032	-0.0103			0.0110	0.0032
	(0.0339)	(0.0329)			(0.0345)	(0.0332)
Inequality Bottom Wt	0.0033	-0.0069	0.0065	0.0034	0.0035	-0.0038
	(0.0397)	(0.0377)	(0.0303)	(0.0282)	(0.0409)	(0.0386)
Inequality Top Wt			0.0032	0.0103		
			(0.0339)	(0.0329)		
Homicide*Ineq Bottom					0.0567	0.0504
					(0.0522)	(0.0499)
Homicide*Ineq Equal					0.0797*	0.0828**
					(0.0424)	(0.0411)
Theft*Ineq Bottom					-0.0194	-0.0260
					(0.0673)	(0.0650)
Theft*Ineq Equal					-0.0561	-0.0569
					(0.0438)	(0.0416)
Burglary*Ineq Bottom					0.1337	0.1234
					(0.0966)	(0.0966)
Burglary*Ineq Equal					-0.0196	-0.0181
					(0.0721)	(0.0706)
Robbery*Ineq Bottom					0.0290	0.0189
					(0.0711)	(0.0697)
Robbery*Ineq Equal					-0.0535	-0.0603
					(0.0616)	(0.0594)
Unemployment	0.0156	0.0123	0.0156	0.0123	0.0125	0.0094
	(0.0490)	(0.0472)	(0.0490)	(0.0472)	(0.0484)	(0.0467)
GDP/Income	-0.0381	-0.0322	-0.0381	-0.0322	-0.0371	-0.0313
	(0.0463)	(0.0467)	(0.0463)	(0.0467)	(0.0467)	(0.0471)
Poverty	0.0302	0.0296	0.0302	0.0296	0.0242	0.0227
	(0.0255)	(0.0254)	(0.0255)	(0.0254)	(0.0234)	(0.0233)
Deterrence	0.0812**	0.0742**	0.0812**	0.0742**	0.0900**	0.0836**
	(0.0339)	(0.0317)	(0.0339)	(0.0317)	(0.0360)	(0.0342)
Years	-0.0009	-0.0011	-0.0009	-0.0011	-0.0012	-0.0012
	(0.0031)	(0.0031)	(0.0031)	(0.0031)	(0.0032)	(0.0031)
Published	0.0674	0.0612	0.0674	0.0612	0.0663	0.0593
	(0.0529)	(0.0521)	(0.0529)	(0.0521)	(0.0538)	(0.0532)
Google Citations	0.0000	-0.0000	0.0000	-0.0000	0.0000	-0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Single Country	-0.0930	-0.0884	-0.0930	-0.0884	-0.1050	-0.0992
	(0.0638)	(0.0627)	(0.0638)	(0.0627)	(0.0637)	(0.0627)
USA	0.0479	0.0455	0.0479	0.0455	0.0617	0.0592
	(0.0668)	(0.0688)	(0.0668)	(0.0688)	(0.0709)	(0.0729)
China	0.0766	0.0707	0.0766	0.0707	0.0819	0.0744
	(0.0717)	(0.0697)	(0.0717)	(0.0697)	(0.0716)	(0.0700)
Cross Section	0.0962*	0.1009*	0.0962*	0.1009*	0.0903*	0.0944*
	(0.0503)	(0.0504)	(0.0503)	(0.0504)	(0.0494)	(0.0493)
OLS	-0.0086	-0.0144	-0.0086	-0.0144	-0.0047	-0.0106
	(0.0281)	(0.0286)	(0.0281)	(0.0286)	(0.0265)	(0.0266)
Instrumental Variables	-0.0227	-0.0232	-0.0227	-0.0232	-0.0183	-0.0190
	(0.0210)	(0.0208)	(0.0210)	(0.0208)	(0.0182)	(0.0182)
Time FE	-0.0258	-0.0254	-0.0258	-0.0254	-0.0270	-0.0274
	(0.0272)	(0.0272)	(0.0272)	(0.0272)	(0.0249)	(0.0246)
Female Head	0.0123	0.0159	0.0123	0.0159	0.0058	0.0105
	(0.0395)	(0.0383)	(0.0395)	(0.0383)	(0.0412)	(0.0403)
Race	-0.1368*	-0.1351*	-0.1368*	-0.1351*	-0.1339*	-0.1317*
	(0.0716)	(0.0724)	(0.0716)	(0.0724)	(0.0691)	(0.0699)
Observations	1431	1431	1431	1431	1431	1431

Notes: This table reports the results of a meta-regression on the effect sizes- partial correlation (PC) and Fisher's z (Z)- using unrestricted WLS. Errors are clustered at the study level. *,**, *** represent significance at the 10%, 5% and 1% levels, respectively.

Table 5: Meta-Regressions, Alternative

	PC	Z	PC Inter	Z Inter	PC FBI	Z FBI	PC Ineq Cat	Z Ineq Cat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SE	0.6133	0.7952	0.6686	0.8470	0.3866	0.5734	0.3070	0.6026
	(0.6089)	(0.6105)	(0.5954)	(0.5964)	(0.6031)	(0.6150)	(0.5928)	(0.6101)
Property Crime	-0.0241	-0.0224	-0.0337	-0.0317			-0.0172	-0.0166
	(0.0178)	(0.0184)	(0.0297)	(0.0312)			(0.0187)	(0.0191)
Violent Crime	-0.0215	-0.0200	-0.0806*	-0.0814*			-0.0145	-0.0139
	(0.0189)	(0.0193)	(0.0413)	(0.0433)			(0.0196)	(0.0198)
Inequality Equal Wt	0.0004	-0.0067	0.0011	-0.0085	-0.0006	-0.0076		
	(0.0343)	(0.0331)	(0.0379)	(0.0377)	(0.0327)	(0.0315)		
Inequality Bottom Wt	0.0134	0.0026	-0.0244	-0.0307	0.0047	-0.0058		
	(0.0407)	(0.0392)	(0.0451)	(0.0451)	(0.0401)	(0.0384)		
Violent*Ineq Equal			0.0485	0.0536				
			(0.0451)	(0.0463)				
Violent*Ineq Bottom			0.0917	0.0861				
			(0.0646)	(0.0663)				
Property*Ineq Equal			-0.0035	-0.0013				
			(0.0355)	(0.0367)				
Property*Ineq Bottom			0.0643	0.0572				
			(0.0606)	(0.0622)				
Violent FBI					-0.0879***	-0.0820**		
					(0.0327)	(0.0314)		
Property FBI					-0.0883***	-0.0821**		
					(0.0328)	(0.0313)		
Theil & GE							-0.0491	-0.0416
							(0.0449)	(0.0470)
Gini							-0.0426	-0.0352
							(0.0328)	(0.0333)
% Held by Richest							-0.0482	-0.0370
							(0.0393)	(0.0403)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1431	1431	1431	1431	1431	1431	1431	1431

Notes: This table reports the results of a meta-regression on the effect sizes- partial correlation (PC) and Fisher's z (Z)- using unrestricted WLS. All the regressions includes the controls employed in Table 4. Errors are clustered at the study level. *,**, *** represent significance at the 10%, 5% and 1% levels, respectively.