Does Inequality really increase Crime?
Theory and Evidence

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Abstract

The standard model of the economics of crime predicts that inequality and (property) crime are positively associated. We show that, allowing for simple modification to the standard framework, such relationship becomes ambiguous. In order to give robustness to this intuition, we conduct a meta-analysis based on 37 empirical papers and 1,130 effect sizes. We find evidence of the presence of (positive) publication bias. When the bias is taken care of, the true effect of inequality on crime becomes almost zero. Finally, we also provide evidence of the source of the estimates' heterogeneity.

Keywords: *Inequality* and *Crime*.

JEL Classification: *K42*, *D6* and *I30*.

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1. Introduction

According to the OECD, the average income of the richest 10 % of the population is about nine times more than the poorest 10%, and this gap has been widening in the last decades. Typically, academics have focused on the impact of inequality on economic growth or financial development (Kuznets (1955) and Greenwood and Jovanovic (1990)). However, in recent years, there is been a growing literature that analyzed the impact of inequality on less studied variables, as crime. According to the standard economics of crime model (Becker, 1968; Ehrlich, 1973; Chiu and Madden, 1998), criminals are rational people that commit a crime according to the maximization of their expected utility. Such models predict that inequality should lead to more crime because it increases the incentives to commit property crime. Empirical evidence seems to confirm such theoretical predictions. For example, in the most cited paper, Fajnzylber et al. (2002) explored the link between inequality and robberies in a panel data for 37 countries during 1970−94 period.[1] Controlling for the endogeneity of inequality, the authors found strong evidence of a positive impact of inequality. In another highly cited paper, Demombynes and Özler (2005) studied the role of local inequality on property crime for South Africa. Again, the authors found a strong positive effect. Nevertheless, Kelly (2000), one of the first works on this topic, did not find that the Gini coefficient had any statistically significant effect on property crime for urban US counties. And, this is not an isolated case.

In our paper, we aim to give a contribution to the existing literature from a theoretical and empirical point of view. We present a simple economic of crime model with two groups, the poor and the rich. The utility of criminals is a positive function of the income of the victims and a negative one of the criminals' own income. We then define an increase in inequality as a simultaneous transfer in income from the poor to the rich. In such a framework, we confirm the standard economics of crime model that predicts a positive relationship between inequality and crime. We then make protection endogenous and consider the case where only the poor offend and

[1] As a February 2018, when we stopped looking for studies to be included in our meta-analysis.

the rich protect themselves. We derive the supply and demand of crime as a function of protection. Then we consider how an increase in inequality shifts the two demand and supply. We show that the two curves move in different directions, having an ambiguous effect on crime. Ultimately, whether crime increases or decreases depends on the elasticity of the curves. However, inequality still has a cost, as it increases protection unambiguously.

The second contribution of the paper is to test whether the ambiguous effect of inequality, found in our model, have empirical grounds. In order to do so, we conduct a meta-analysis. A meta-analysis consists in collecting the results from all individual studies that study such relationship, with the aim of making sense of the heterogeneity in the results from different studies. More specifically, through meta-analysis techniques, we want to identify the true income-inequality coefficient after correcting for publication bias. This latter refers to the preference to report a particular result by a researcher, which we suspect to be positive in our case.[2] Also, we explore the drivers that are likely to explain the heterogeneity in the inequality-crime estimates. To our knowledge, this is the first meta-analysis in the economics of crime literature.

Our meta-analysis is based on 37 published and unpublished studies. We collect all the estimates on the inequality-crime relationship and compute partial correlation coefficients. In such a way we can directly compare studies that have different functional forms, different crime or inequality measures. We gather 1,130 estimates, about 30 per study. When we control for publication bias, the true effect of inequality on crime is mostly zero or economically insignificant. This result reassures us that such relationship might be ambiguous, as we have shown in the model. Results are robust to various econometric techniques and specifications. We then explore the drivers of the heterogeneity of the estimates. Our analysis reveals that estimates from studies employing panel data, with many regressors and considering property crimes are associated with lower values. On the other hand, models controlling for unemployment, deterrence and gini coefficient are more likely to find greater values.

[2]Stanley and Doucouliagos (2012) showed that publication selection may represent a serious problem in nearly two-thirds of the empirical areas of economics. This is particularly true for areas where there is unambiguous theoretical predictions, as inequality-crime or minimum wage-employment.

The paper is organized as follow. Section 2 presents the theoretical model. It follows section 3 which presents the selection criteria used in the meta-analysis, and the summary statistics. Section 4 presents the formal tests to detect publication bias. The next section, 5 explores the source of the estimates heterogeneity. The following section, 6 concludes.

2. Theory

2.1. The Standard Model

The main theoretical mechanism to link inequality and crime is what Becker (1968) called supply of offenses. To compute this supply, Becker assumes that a person commits an offense if the expected utility to him exceeds the utility he could get by using his time and other resources in other activities. According to this framework, crime rates depend on the potential gains from crime and the associated opportunity cost. Such net gains, in turn, depend on wealth differences between the rich and poor. Chiu and Madden (1998) said: ”thus, if income inequality increases so that low incomes become lower and higher incomes become higher, then the level of crime is driven up from two sources: the alternative to crime is less attractive for criminals and the potential proceeds from crime are greater”. Similar arguments have been used by Josten (2003), among the others.

2.1.1. Incentives for Crime

We summarize the basic argument following a simple model developed by (Bourghignon, 1999). Let us consider a society with individuals endowed with heterogeneous levels of resources, say income, y. We assume that the utility function of income is logarithmic, with implies a constant relative risk aversion equal to one.[3] In the economic model of crime, crime is a choice under uncertainty: an individual can maintain his initial income endowment with certainty, or engage in crime activity.[4] We further assume that one criminal can only attack one other member of society.

[3]This is not a completely innocuous assumption. In the appendix we derive some conditions on the relative risk aversion of the utility function in order to generalize our simple model to an arbitrary one.

[4]We assume that the decision to engage in crime is binary, that it, to be a criminal or not. Other frameworks assume that individuals may divide their time in legal and illegal activities. See Ehrlich (1973).

If the victim has an income y', the criminal appropriates a fraction β of this income, with $0 \leq \beta \leq 1$. However, crime is risky because it can be prevented. Let q be the probability that the criminal is apprehended, in which case the criminal income is reduced in a proportion f. The punishment is multiplicative because it depends on the offender initial endowment. The net utility v of the criminal activity for an individual of income y is[5]

$$v(y,y') = (1-q)[log(y+\beta y') - log(y)] + q[log(y(1-f)) - log(y)] \quad (1)$$

In (1), we are explicit in the dependence of the net gains of crime on the income y of the offender and the income y' of the victim. The utility also depends on the apprehension probability q and the parameters f and β. But in order to keep our analysis as simple as possible, in this subsection we will consider these variables as fixed. I can be easily shown that for any $y, y' > 0$ and $1 > q > 0$ we have

$$\frac{\partial v(y,y')}{\partial y} < 0, \quad \frac{\partial v(y,y')}{\partial y'} > 0 \quad (2)$$

From the first expression, we conclude that the poorer are the individuals in the society the higher are the incentives for crime. The second one implies that incentives also increase with the victim income. The other partial derivatives have the expected signs as well. Crime's net utility increase with the fraction of potential gains β and decrease with the probability of being caught q or with a higher punishment f.

2.1.2. Crime Supply

This equation about the economic incentives for crime can be easily be extended to encompass inequality. To do so, we consider that the society is composed by the fractions λ_P and λ_R of poor and rich individuals, respectively. We assume that $\lambda_P \geq \lambda_R$. Their incomes are y_P and y_R, respectively, with $y_P < y_R$. Individuals decide whether to engage in a criminal activity or not, and

[5]Notice that we are assuming that the crime rate is low, and so individuals are not computing the probability of being crime victims when they decide whether to become criminals or not.

if so, they decide whether to offend a poor or a rich individual. Accordingly, both poor and rich agents may be potentially perpetrators or victims.

The probability of apprehension q is likely changing over groups, because rich individuals may invest in private security or put pressure the government for targeted public protection.[6] We will discuss this possibility in what follows, but for the moment let us assume that q is the same for all groups. This assumption implies that only rich individuals are attacked because, *ceteris paribus*, the payoff of a successful crime will be higher if the victim is rich. [7] As such $y' = y_R$ for all crimes.

In order to have interior solutions, we assume that it exists a moral cost of being criminal, which is strictly positive and different for each individual.[8] This moral cost is a random variable which is draw from a distribution ψ. The fraction of individuals engage in crime is $\psi[v]$, that is, those individuals with moral costs lower than their net utilities.

The supply of crimes C_S, written as a rate over the entire population, is given by:

$$C_S = \lambda_P \psi[v(y_P, y_R)] + \lambda_R \psi[v(y_R, y_R)] \tag{3}$$

First we notice that given that $\lambda_P > \lambda_R$, $y_P < y_R$, and (1) decreases with income, the number of poor offenders is higher than the number of rich offenders. Both rich and poor can be criminal depending on the parameters of the model. Let define q_J^* as the probability of being arrested that makes null the net utility of an individual in the group $J \in \{P, R\}$, that is $v(y_J, y_R; q_J^*) = 0$. As the function (1) is strictly decreasing in q with negative (positive) values for q close to one (zero), the existence and uniqueness of q_J^* is guaranteed by continuity. Individuals in the group $J \in \{P, R\}$ engage in crime if $q < q_J^*$. It is direct to notice that $0 < q_R^* < q_P^* < 1$. Accordingly, for low levels of q, that is $q < q_R^*$ both rich and poor individuals may engage in crime; for q such that $q_R^* < q < q_P^*$ only poor individuals are criminals (and rich individuals are victims), and for $q > q_R^*$ nobody chooses to participate in illegal activities because the probability of being caught is too

[6]This is true for the other parameters as well. The punishment f may be higher for a rich victim who can hire a better prosecutor lawyer and increase the offender punishment. However, we focus our analysis in p.

[7]We assume there is no congestion or externalities on crime.

[8]The same assumption is done by Bourguignon et al. (2003), who calls it honesty.

high.

2.1.3. Inequality

Now we can study the effects of inequality on the crime rate (3). There are many alternative ways to study a change in inequality. Here, we focus our analysis on the well known *Principle of Transfers*, developed by Dalton (1920). Such principle states that the measures of inequality ought to decrease under a *progressive transfer*, which is defined as an income transfer from a rich person to a poorer one. We adapt this definition to our case, in which we have two groups of homogeneous agents. We consider a transfer from one group to another as an equal transfer of each member of one group to each member of the other. We define a change in inequality as follows:

Definition. An infinitesimal increase of inequality, that we define as $i+\Delta i$, is a simultaneous change in incomes to $y_p - \Delta i$ and $y_R + \lambda \Delta i$.

In this sense, we define an increase in inequality as the following derivative:

$$\frac{\partial}{\partial i} = -\left(\frac{\partial}{\partial y_p} - \lambda \frac{\partial}{\partial y_R}\right) \tag{4}$$

Proposition. A negative progressive transfer increases the crime rate.

Proof. It is direct, since for the poor the net utility of committing a crime $v(y_P, y_R)$ decreases with y_P and increases with y_R. This implies that the crime rate among the poor individuals increases. For the rich $v(y_R; y_R)$ does not change under changes in y_R, because both benefits and opportunity cost of crime are increasing with y_R.

A similar model was presented by Bourguignon et al. (2003), which found it unlikely that modifications to such basic model would substantially alter its main implications, i.e. a direct relationship of inequality with crime. In the next sections, we will show that this is not always the case.

2.2. Endogenous Protection

In the previous section we assume that the level of protection is exogenous. However, in the opportunity cost approach to crime, potential victims seek to be less attractive to crime investing in private protection. In that sense, the previous section describe the supply of crime, but the price that victims are willing to pay for protection determines the demand of crime (in this case the demand has the opposite sign because crime is not an economic good but a ”bad”). Such an equilibrium approach has been discussed in several works. For example, Cook (2017) pointed out that the way criminals behave influences the sel-protective strategies by the potential victims. Therefore, changes in in self-protection makes alter criminal opportunities, making them more or less attractive. Simlarly, Dijk (1994) considered the rate of property crime as the interaction between the decisions of criminals to commit an offense and of the potential victims to protect their property.

2.2.1. Protection

In practice, the level of protection q is decided by the citizens through two main mechanisms. First, there is a level of public protection - police force, law enforcement and so on - which is decided in the democratic system. Following this approach, Merlo (2003) assumed that police level is defined by the majority rule. Secondly, this level of public protection is complemented by private investments in protection, as private guards, home alarms, and so. There is also a third alternative, that is that most powerful citizens in society use the public funds in their private protection (for instance, a larger fraction of police force is in richer neighborhoods). However, to keep the model as simply as possible we do not consider such possibility in our model.

We considered a model in which protection q depends on a public component q_0 and a private investment θ, such that $q = q_0 + q(\theta)$. We assume that the function $q(\theta)$ is concave and satisfies an Inada condition at zero, that is, $q' > 0$, $q'' < 0$ and $q'(0) = \infty$.

Given that now the level of protection is endogenous, we write (1) as an explicit function of q, that is $v(y,y;q)$. We define q_L and q_R as $v(y,y;q_L) = 0$ and $v(y_P,y_R;q_H) = 0$, respectively.[9] For simplicity, we assume that $q_L \leq q_0 \leq q_H$. Under this assumption, individuals do not attack other individuals in the same group ($q_L \leq q_0$), but poor individuals may have incentives to offend while

[9]The explicit solutions are $q_L = log(1+\beta)/log((1+\beta)/(1-f))$ and $q_H = log(1+\beta(y_R/y_P))/log((1+\beta(y_R/y_P))/(1-f))$.

rich individuals may be victims of these offenses ($q_0 \leq q_H$). Finally, we assume that exist a level of protection θ' such that $q_0 + q(\theta') = q_H$.

2.2.2. Crime Supply, Demand and Equilibrium

The crime rate supply C_S given a level of rich private investment θ is

$$C_S(\theta) = \lambda_P \psi[v(y_P, y_R - \theta; q_0 + q(\theta))] \tag{5}$$

The supply of crimes $C_S(\theta)$ is a decreasing function of private investment in protection θ, given that $v(y_P, y_R - \theta; q_0 + q(\theta))$ in (1) is decreasing in $y_R - \theta$ and increasing in $q_0 + q(\theta)$. More investment in protection decreases the level of crime through two channels. First, it decrease the disposable income of the victims used in protecting themselves. Most importantly, protection decreases the probability of crime success and thus the incentives for crime are lower.

Now we turn to the level of protection, i.e., the demand side. Individuals observe the crime rate C and decide whether to invest in protection or not. Given our assumption that q_0 is high enough that only rich individuals may be victims, only them may find optimal to invest in protection. Consider that one criminal commits only one offense on other individual, the probability that a rich is randomly attack if $C\lambda_P^{-1}$. Rich individuals maximize their utility as a function of protection θ, that is:

$$U_R(\theta) = log(y_R - \theta) + C\lambda_P^{-1}(1 - q(\theta) log(1 - \beta)$$

We compute the first order condition, and we solve for C in order to obtain the demand for crime $C_D(\theta)$:

$$C_D(\theta) = \frac{\lambda_P}{log(1-\beta)} \times \frac{1}{(y_R - \theta)q'(\theta)} \tag{6}$$

The demand of crimes $C_D(\theta)$ is an increasing function of private investment in protection θ, given that $q(\theta)$ is concave and thus $q'(\theta)$, and $(y_R - \theta)$, are decreasing in θ. The higher the crime rate, the higher the investment in protection.

An equilibrium (C^*, θ^*) is a level of private protection θ^* in which supply and demand of crime are equal to C^*. That is, $C^* = C_S(\theta^*) = C_D(\theta^*)$.

PROPOSITION. There exists a unique equilibrium (C^*, θ^*).

PROOF. On the supply side, function $C_S(\theta^*)$ is strictly decreasing in θ. For $\theta = 0$, $C_S(0) > 0$ given that $q_0 \leq q_H$. As an assumption, it exists a θ' such that $q_0 + q(\theta') = q_H$, we have that $C_S(\theta') = 0$ given that $v(y_P, y_R; q_H) = 0$. On the demand side, the function $C_D(\theta)$ is strictly increasing. For $\theta = 0$, $C_D(0) = 0$ given Inada condition. As we have that $C_S(\theta) > 0$ for any θ, in particular we have that $C_S(\theta') > 0$. The existence of equilibrium follows from continuity. As both function are strictly decreasing and increasing, the equilibrium is unique.

2.2.3. Inequality

Now we study the effect of inequality on the equilibrium. In this case, a change in inequality is moving the two curves that determines the equilibrium, changing the level of protection and the crime rate. We write the new equilibrium as:

$$C^* + \Delta C = C_S(\theta^* + \Delta\theta, i + \Delta i) = C_D(\theta^* + \Delta\theta, i + \Delta i) \tag{7}$$

where $i + \Delta i$ represent a simultaneous change in incomes to $y_p - \Delta i$ and $y_R + \lambda\Delta i$. As all functions are continuous, we extend at first order this expression to establish linear relations between the rate changes. For the change in (θ) we have the following derivatives:

$$\Delta\theta = -\left(\frac{C_{S,i} - C_{D,i}}{C_{S,\theta} - C_{D,\theta}}\right)\Delta i \tag{8}$$

The supply and demand on crime are decreasing and increasing, respectively, with respect to protection. That is $C_{S,\theta} < 0$ and $C_{D,\theta} > 0$. We also know for Proposition 1 that $C_{S,i} > 0$. Finally, it is direct from definition show that $C_{D,i} < 0$. From this relation, we conclude that the effect of Δi is unambiguously positive on $\Delta\theta$.

The derivative of crime is:

$$\Delta C = C_{S,\theta}\Delta\theta + C_{S,i}\Delta i \tag{9}$$

$$= C_{D,\theta}\Delta\theta + C_{D,i}\Delta i \tag{10}$$

In this case the effect of inequality is ambiguous. While $\Delta\theta$ and Δi move in the same direction, as we have just shown, their derivatives have opposite signs either in the demand or in the supply equation. Accordingly, the effect of inequality on the crime rate depends on the particular elasticities of the equilibrium point. fig:ModelFigureshow our findings.

[Figure 1 ABOUT HERE]

As we can see, an increase in inequality leads to an increase in the supply curve and a decrease in the demand curve. This leads to an unambigous increase in the level of protection, because victims reacts to the increase in crime by protecting more. However, the effect on crime are ambiguous.

2.3. Possible Extensions to the model

We could think of extending the model toward several directions, in order to check whether *Proposition 1* holds in other scenarios. For example, we could consider three groups: poor, rich and middle class. If the crime deterrence parameters are the same across groups, nothing very different from the two groups model occurs. Criminals attack only the richest individual in society given that there is not congestion. All groups may participate or not, depending on the parameters, but the poorer the group is, the higher will be the participation of its members in criminal activities. In this context, under very mild conditions, any negative progressive transfers would increase the crime rate, given that the incentives for more vulnerable people would increase. However, if protection is different across groups, the rich may be not the unique victims, given that they can invest in protection. The evidence shows that crime victims are more in the middle class, rather than among the richest (Gaviria and Pagés, 2002). Using an economic model of crime with continuous location, CC shows that victims of burglary are more often from the middle class. Therefore, a progressive transfer that decreases inequality, would have different effect on crime if the transfer was from rich to poor or from middle income to poor might. That is because in the first scenario, the crime rate decrease via increased opportunity costs, whereas in the second the crime rates would increase via rapacity ones.

3. Description of the Data

In section 2, we showed that with fairly common changes to a standard model of crime, the relationship between inequality and crime might result ambiguous. Now, we aim to make sense of the empirical literature that studied such relationship through the use of a meta-analyis, i.e. collecting the results from individual studies that study the inequality-crime relationship. In particular we have two separate objectives: first we want to evaluate the true inequality-crime relationship, net of publication bias. Secondly, we want to study whether and how the characteristics of the studies affect the magnitude of the inequality-crime relationship. In order to so, we first explain how we selected the individual studies and what effect sizes we recover from each of them.

3.1. Selection Criteria

The academic works used in the meta-analysis have been selected using the following criteria:

Title of Paper. The title must include both the word inequality and crime. It is accepted the use of words which echoes such concepts, such as income dispersion or concentration, for inequality, or criminal activity, illegal behavior or violence, for crime. For example, Brzezinski (2013)'s title ”Top income shares and crime” is accepted. We also include works that refers to a specific crime, as Menezes et al. (2013), with homicide. We exclude all those studies that include inequality as a regressor but which principal focus, using the title perspective, is not inequality, or income dispersion. For the same reasons, we do not consider the abstract of the works, as it might mislead our main focus on inequality.

Crime Measures. We consider any type of crime, whether it is purely motivated by an economic reason or not. Whenever the authors use crime index, we write down the crime categories that compose it. We do so because some authors follow the FBI classification which consider as property crime all types of crime where violence is not involved and violent when violence is involved. According to this classification, a theft would be a property crime whereas a robbery a violent one. Other authors, instead, consider a crime as a property one whenever there is a tangible economic loss and violent when there is not. Accordingly, a robbery would be considered as property one. [10] In section 5.1, we will divide crime categories according to this latest definition.

In the section 5.1, we will separate between property and violent crime.

Search Engines & Discipline. We searched studies through academic search engines. We entered keywords as "inequality/inequitable development/income distribution" and "crime/criminal activity(ies)/illegal behavior". We employed engines as *Google Scholar*, ISI web of science and *Econlit*. We first collected primary works, i.e., those mostly known, in terms of citations or relevance of the authors. We thoroughly read these studies' literature review sections to find cited works which satisfied our criteria. We also reviewed the studies that cited these primary works, using *Google Scholar* and *Social Science Citation Index*. We did not restrict the search to any period and we concluded such search on the twenty-second of February 2018. We collected studies published in refereed journals but also on working paper series and published in non-referred journals. Since many of the published works first appear in working papers series we apply the rule that "publication dominates over working papers". Therefore, we always consider a published work over its working paper version, whether it exists. Finally, we also consider books.

We decided to include only works published in economic journals as identified with IDEAS/RePEc (2018a). [11] That means not considering important works published in other fields, as criminology, sociology, law, politics, geography and medicine.[12]Similarly, we decided to consider only the

[10]Of course any type of crime implies a cost, even pure violent ones as rape and assault (**?**.

[11]The only two exceptions to this rule are Astarita (2013) and Izadi and Piraee (2012) which are published in the *Journal of Public Finance and Public Choice* and *Journal of Economics and Behavioral Studies* respectively.

[12]For example Blau and Blau (1982) for sociology or Patterson (1991) for criminology.

works that appear in economics working paper series, as defined by IDEAS/RePEc (2018b), or that are written by academics working in an economic department at higher education institutions. We therefore excluded phd thesis or economic papers written only by graduate students. For books, we consider those that are written by economists.

Language. We considered only works written in English, as it is the mainstream language in economics. By doing so, we exclude works written in the national language of countries that have high level of inequality, as those from Latin America. Interestingly, in our database, after the USA, Mexico and Colombia are the most represented countries.

Measures of Inequality. We consider only income based inequality measures, as on wages and consumption/expenditures. In such a way, we exclude other types of inequality, based on education for example. If a study reports income and non income related inequality measures, we will retain only the results from the former and discard the second [13]. Whether a variable represents inequality is decided only by the researcher. We will report the variable that he/she identify as inequality. In the case that the regressions include more than one measure of inequality, we consider each separately. For example, this occurs in works that employ decomposable inequality measures, as general entropy ones. In time series analysis that report a large number of lags, we report up to the third lag. Further details on the effect sizes we report are given below.

The search has produced thirty-seven works: 29 in referred journals, 2 in non-referred journals, 5 in working papers series and 1 book. A complete list of the of works included can be seen in Table 1.

[Table 1 ABOUT HERE]

3.2. Effect Size

The most basic, cross section, econometric regression, i, employed in a typical study, j, on inequality and crime takes the following form:

[13] This is the case of Kelly (2000).

$$Crime_{i,j} = \alpha + \theta Inequality_{i,j} + \gamma X_{i,j} + \varepsilon_{i,j} \tag{11}$$

Some works considers *Crime* in rate, whereas other in level; those who report rates consider them per 10,000 inhabitants and other per 100,000; some reports 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. Therefore we cannot readily compare the θs among all the studies and, in order to do so, we transform 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:

$$Part\ Corr\ Coeff_{i,j} = \frac{t - statistics_{i,j}}{\sqrt{t - statistics_{i,j}^2 + Degrees\ of\ Freedom_{i,j}}} \tag{12}$$

Standard errors are calculated in this way: $Standard\ Error_{i,j} = 1 - \sqrt{(1 - Part\ Corr\ Coeff_{i,j}^2)/Degrees\ of\ Freed}$ Whenever the tables did not report the test statistics, we calculated through the information in the tables with the results. Whenever p-value was not reported , but only the level of significance, we assigned 0.01, 0.05 and 0.1 if the coefficients were significant at the 1%,5% and 10% respectively.

We would have preferred to employ elasticities, as they represent an economic measure rather than a statistical one as partial coefficients. However, only thirty percent of all estimates in our analysis have the crime and inequality measure in log form.[14] The use of partial correlation coefficient is widespread in meta-anlysis Stanley and Doucouliagos (2012).

We report all θ coefficients from all the models in the 37 works that regress crime on an inequality measure. We do so to increases the number of observations and model heterogeneity in

[14]For the remaining 70 %, we could have calculated the elasticities using the means and standard deviations of the dependent and independent variables. However, many studies do not report them. Alternatively, we could have used z transformation, Fishers z-transform and t-test, but again, these measures are also statistical ones and are more flawed then partial correlation coefficient.

(5). By reporting only one coefficient per study, we would have lost important within study heterogeneity. Moreover, Bijmolt and Pieters (2001) showed that using multiple measurements per study is to be preferred for detecting the "true" underlying population effect size. Indeed, using all effects causes within study dependence which we will address with particular statistical methods. As a robustness, we will consider a model retaining only coefficient per study. We will also consider a weighted least square regression, using the number of studies as weights to avoid over-representing studies with many estimates. The final number of effect sizes we have is 1,130 which means an average a bit higher than thirty per study. 806 of the 1,130 estimates are positive, about 71.33 %. If we consider the median estimate per study, we have that 31 out of 37 are positive, about 83.8 %. Nevertheless, there is high heterogeneity in terms of effects among study as can be see in Figure 4.

In Table 2 we reported the weighted and unweighted averages, and standard deviations, of the partial correlation coefficient. As we can see, the unweighted average is 0.17, whereas using the inverse of the standard errors as weights it lowers this to 0.2. If we employ the impact factor as weight, the partial coefficient is about the same value as the unweighted one. The median value per study is higher, at 0.146, whereas the median weighting for precision (inverse of standard error) is 0.164. Although such values are small, we should be cautions in interpreting them in the likely presence of publications bias, which we will address in the next section.

[Table 2 AND Figure 4 ABOUT HERE]

4. True Effect & Publication Bias

4.1. Graphical Analysis

We start exploring graphically whether publication bias is an issue in the inequality-crime literature. In such way we can also start having an idea on what the true effect is, i.e. net of publication bias. As we said in the introduction, publication bias means favoring to report certain results based on the theoretical predictions. A standard way to do so is through a funnel graph. This consists in plotting the partial correlation coefficient in the horizontal axis and its precision, i.e., the inverse of the 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 of 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 will need to try harder to find statistically significant results that support their theory. When no publication bias exists, 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 on one of the sides of the most precise coefficients, the researcher has a preference over some results.

We reported the funnel graph for the inequality-crime relationship in Figure 2. The graph is roughly funnel-shaped, although there is more concentration of points in the right side, suggesting some degrees of publication bias. The most precise estimates are very close to zero, which anticipates that the true effect of inequality on crime is lower than thought, net of publication bias. The twenty most precise coefficients average 0.015. For simplicity, we report two lines: the solid line represents the unweighted average, whereas the dashed ones the weighted one, using the inverse of the standard error as weights. We exclude from the graph, but not from the analysis, the estimates belonging to Andrienko (2002), which are the most precise ones. The average measure of precision for Andrienko (2002) is 317, well above the highest point in the graph , which is 80. The other outliers are the points which have precision level around 40 and shape a curved line to the right. These represent the estimates by Costantini et al. (2016). As a robustness exercise, we will run the formal tests for publication bias excluding the two studies.

[Figure 2 and Figure 3 ABOUT HERE]

We also report the funnel graph considering only the median estimate for each study, in Figure 3, which also displays the name of each study. The presence of positive publication bias looks neater in this graph compared to the previous one.

4.2. Formal Tests

We now formally test for publication bias, i.e. whether there is a relationship between the partial correlation coefficient and their standard errors (Card and Krueger, 1995). The regression model is the following:

$$Part\ Corr\ Coeff_{i,j} = \lambda_0 + \lambda_1 Standard\ Error_{i,j} + \varepsilon_{i,j} \tag{13}$$

Where i stands for the *ith* estimates in the *jth* study. The coefficient λ_1 represents the publication bias, whereas λ_0 represents the true effect size after correcting for publication bias. As Stanley and Doucouliagos (2012) pointed out, such regression suffers from heteroskedasticity because the conditional distribution of the errors with respect to the standard errors is not constant. However, we know that the source of such heterogeneity is the estimates' variances itself. Therefore these authors suggest to use weighted least squares, using the the inverse of each estimates variance, as weights. After dividing by the standard errors, we have the following regression model:

$$t - statistics_{i,j} = \lambda_1 + \lambda_0 \left(\frac{1}{Standard\ Error_{i,j}} \right) + \upsilon_{i,j} \tag{14}$$

Where $t - statistics_{i,j}$ is the t-statistics of each empirical effect, $1/StandardError_{i,j}$ its precision and $\upsilon_{i,j}$ is $\varepsilon_{i,j}/StandardError_{i,j}$. λ_1 represents the direction and magnitude of selection bias and λ_0 the true effect of inequality on crime, i.e., net of publication bias. This is the well-known Funnel Asymmetry Test, or FAT (Egger et al., 1997). If the literature on inequality and crime is free of publication the coefficient λ_1 should not be statistically significant. Column (1) of Table 4 reports the results for the weighted least squares exercise. Given the likely within-study correlation we employ clustered standard errors at the study level. However, standard errors might be correlated with residuals. For example, we could think that the precision of the partial correlation coefficient to be dependent on some characteristics of the papers. In order to take this into account, we employ an instrumental variable approach in column (2). More specifically, we instrument the standard error with the inverse of the number observation [15]. We believe this is a good instrument because the degrees of freedom are generally little correlated with method choices. In the next two columns, we exploit the panel structure of the database, still using the transformed model as in (14). In column (3) we consider a mixed effect model, estimated through maximum likelihood methods. Mixed effects model allow both fixed and random effects. They are particularly useful in our case because they allow a hierarchical structure given that estimates are nested into studies. In column (4) we consider a within, fixed-effect, model. Such technique allows for some degrees of correlation between the error with the regressors, the standard error in our case. The cons of using such model is that it does not allow to study the impact of those variables that are study-invariant. In (5) we consider a random effect model. Finally, in column (6) we consider an ols model, without weighting for the inverse of the estimates' variances. Even though all these techniques are theoretically correct, our preferred ones are those in the first two columns. Stanley and Doucouliagos (2015) showed that weighted least square is always as good to fixed and random effects models and, in some circumstances, they perform better. [16]

[Table 4 ABOUT HERE]

Looking at column (1), the publication bias (λ_1 in Equation 14), is 1.18 and statistical significant at the 5 % level. According to Stanley and Doucouliagos (2012), being the coefficient greater than 1, reflects some relevant degree of selection bias. The true effect , net of the publication bias is 0.01, but not significant at conventional statistical significance level. Using instrumental variables techniques, in column (2), we find similar results, with greater publication evidence of publication bias, although less precise. Considering the panel data results, columns (3) and (5), we confirm the presence of pistive publication bias. However, using mixed models and fixed effects, we also a significant true effects, (λ_0). Both are 0.02, much smaller than the unweighted average. Using the Cohen (1988) s well-known guidelines, this partial correlation is to be consider quite small [17].

[15]Such relationship is negative, as higher estimates are associated with lower standard errors.

[16]More specifically, weighted least square perform better than fixed effects when there is heterogeneity and to random effects when there is selective reporting.

[17]Cohens guidelines establish that 0.1 is small, 0.3 is medium, and anything larger than 0.5 is large.

Finally, results with unweighted ols are somehow similar to the one in column(1).

[Table 5 ABOUT HERE]

In Table 5, we perform various robustness tests. We report only the results, employing WLS, our preferred model. [18] We start by considering only one estimate per study, namely the median partial correlation coefficient and standard error per study, in column (1). This is basically the formal test for Figure 2. We still find evidence of publication bias and the λ_0, true effect, is practically zero, and not statistically significant. In column (2) and (3) we consider all estimates, and only the median, but we remove the two outliers, Andrienko (2002) and Costantini et al. (2016). We still find evidence of publication bias and practically zero true effects. The true effect is slightly negative but, again, not significant. In (4) we weight by the number of citations rather than precision. previous results are confirmed. In column (5) we only consider the studies with more than 10 citations. We do not find any sign of publication bias but also of a true effect. In (6) and (7), we consider only the works published in journal or working paper series with impact factor above 1 and 10, respectively. We find evidence of publication bias only for those above 1 but not 10. We never find evidence of a significant true effect. These results, along the one found in Table 4, evidence an almost inexistent relationship between inequality and crime. The model we presented in section 2 is helpful in making sense of such results. We show that with fairly simple changes to the standard model, for example the introduction of private protection, inequality ceases to have an unambiguous positive effect on crime. We also find some evidence of publication bias although we foresee how studies characteristics are important in explaining its presence.

5. Modelling Heterogeneity

As Figure 4 shows, there is a high degree of heterogeneity of effect sizes between and within works. This might depend on misspecification biases, such as omitted variables or not adequate

[18]The rsults with other techniques are available upon requests.

econometric methods. Heterogeneity might also depend on genuine differences in the this relationship across countries. In order to explore this issue deeper, we model the source of heterogeneity by running the following regression:

$$Part\ Corr\ Coeff_{i,j} = \lambda_0 + \lambda_1 Standard\ Error_{i,j} + \lambda_k Moderators_{i,j} + \varepsilon_{i,j} \quad (15)$$

Moderators are the variables that help explain the sign and direction of the effect sizes. We included the standard error to control for the presence of publication bias. We weight the regression by the inverse of the coefficients' variance as we have done in equation 15. Notwithstanding that we can still interpret λ_1 as the publication bias, we cannot do the same for λ_0 as the true effects is now replace by λ_0 + $\lambda_k Moderators_{i,j}$ (Stanley and Doucouliagos, 2012).[19]

We divided the moderators variables into the following categories:

Characteristics. We included several variables that capture the studies' characteristics as the number of years since the work came out. In such way we can capture whether there has been trends over the years, especially considering that the most important papers were published at the beginning of the year 2000. Moreover, we include a dummy equal to one if the study is published or not. Finally we also consider the impact factor of the journal or working paper series.

Data. We consider the number of observations and the total number of regressors. The inclusion of this last variable is useful to account for the presence of (possible) omitted variable. We also include three dummies for the country of study. We consider only the three countries with more effect sizes: *USA*, *Colombia* and *Mexico*. In such a way we can verify whether there are differences in the inequality-crime relationship between countries.

Econometric Issues. We consider a dummy equal one whether the estimation technique is OLS and zero otherwise. We also include a dummy that takes value one whether endogeneity of the inequality measures was taken care of. Again, this strategy is useful to take into account the likely endogeneity of inequality with respect to crime.

Crime Variables & Inequality Variables. We consider a dummy variable taking value one if

[19]The coefficient λ_0 would represent the true effect if all the moderators variables were zero.

the dependent variable in the regression was a property crime and zero otherwise. We classify as property crime all types of crime that involve loss of property, irrespective of the use of violence. [20]. Property crimes represent 54.3 % of all estimates. We also consider a dummy variable equal one if the regression used a Gini coefficient as a measure of inequality. We decided to to so because more the half of the inequality measure employ Gini.

Controls. We coded the control variables included in each regression. Such variables are dummies equal one if the control is included and zero if it was not. We consider whether any kind of deterrence variable was considered. It is worth noting that deterrence variables almost exclusively refer to what we define as public protection in the model. We also consider whether time dummies and trends were included. Other relevant controls are the percentage of female heads of family, a standard measure of deprivation in the economics of crime literature. Finally, we also included three income variables: unemployment rate, gdp or a proxy and poverty. These variables are likely to be related with inequality and their inclusions might reduce omitted variable bias.

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

5.1. Heterogeneity Regressions

We are going to run the model (15) using the unrestricted weighted least squares.[21] Table 6 reports the results by slowly adding the block of variables presented earlier once at that time. In column (6) we include all the variables. We analyze the results from this last column for simplicity. As we can see, *Year* is negative and significant : each additional year since publication decreases the partial correlation coefficient by 0.01 points. Indeed, there was a tendency to overstate the link between inequality and crime in the past. This might also explain the general perception that inequality strongly affects crime. Interestingly, whether the work is published seems to reduce the effect size, although the effect is not statistically significant. The number of Google scholar

[20]Therefore, we do not follow the FBI classification which considers a crime as violent crime even if there is a loss of property. According to the FBI a robbery would be considered as violent crime, whereas for us is a property one.

[21]We also run all the other models. Results are available upon requests.

citations and impact factor have generally positive effects but are not statistically significant.

Works with more observations, that include more control variables tend to have lower estimates, possibly for omitted variable bias. Interestingly, estimates from study using data from only a country analysis report lower estimates. It is likely that cross-country analysis are dominated by the presence of south American country that present high level of inequality and crime. Continuing, panel data are associated with smaller estimates, compared to cross sectional studies (the baseline category). A possible explanation is that inequality changes slowly over time and that the main differences are between. Turning to the importance of country, we do not find evidence that studies based on the USA, Colombia or Mexico have higher coefficients, at least at conventional statistical level.

Regarding the estimation technique, we do not find that regressions that take care of the endogeneity of inequality have higher estimates. Regressions that employ standard OLS techniques have also higher coefficients.

Continuing, the coefficient for property crime is negative and significant. That means that, on average, inequality has more effect on crimes which do not involve economic loss. Our model might help explaining this result. It is likely that income inequality is affecting more violent crime that are not explained by economic rationality. As for the inequality measures, we find that *Gini* is associated with a higher coefficient. This result is puzzling and call for a deeper analysis. In fact, Gini coefficient usually gives more weight to variation in the middle of the income distribution.

[Table 6 ABOUT HERE]

Continuing, the inclusion of deterrence increases the coefficients, as the inclusion of unemployment, suggesting a negative bias in models that exclude these variable. Whether the result for unemployment is easy to explain. [22] The former result for deterrence suggest that public protection, and not only private protection, is positively associated with inequality. If the regressions

[22] Unemployment and crime are negatively associated whereas unemployment and inequality are positively associated.

include time trends, local fixed effects, percentage of single mothers, race, poverty and income do not seem to be affecting the size of the coefficients.

If we consider the coefficient of the standard error, we see that the point coefficient is smaller than than one column (1) in 4, and it is insignificant but in column (3). That means that the publication bias depends on the characteristics of the studies.

6. Discussion & Conclusions

Too often, in the academia or in the public debate, it is assumed that high levels of inequality are associated with high levels of crime. Existing empirical contributions seem, on average, to support such view. However, some works also report negative or no effects at all. In our paper we re-examine the inequality crime nexus and provide two contributions to the existing literature.

First we consider a standard economics of crime model where we confirm that an increase in inequality is associated with higher level of crime. However, when we endogenize protection, we obtain that the effect is ambiguous. The intuition is that when inequality increases, private protection by the rich also increases which reduces crime.

Following the predictions of the model, it comes natural to question whether the generally positive effects found in the empirical papers are genuine impact or suffer from publication bias. In order to explore such issues further, we conduct a meta-analysis. This tool has been ignored by the economics of crime and has been little employed by mainstream economics. We gather thirty-seven studies: thirty-one papers published in referred journals, five papers appeared in working paper series and one book. Given the impossibility of comparing estimates coming from models that employ different measures and specifications, we calculated the partial correlation coefficients. The total number of effect sizes we employ is 1,130. Figure 4 and **??** confirms the high degree of heterogeneity between and within study in the inequality-crime literature. The unweighted average is 0.067, whereas the weighted one is 0.023, almost one third. Graphical introspection provides some evidence of positive publication bias. Most importantly, Figure 3 shows that the most precise estimates are centered around the zero. Figure 2 provides even clearer evidence of an asymmetrical funnel.

Formal tests, reported in Table 4, show robust evidence of a non-existent genuine effect of inequality on crime. However, they also reveal the publication bias depends on the study's characteristics, as in the last columns of Table 5. As a next step, we investigate the sources heterogeneity among estimates in our database. We consider a set of variables that might influence the signs and magnitudes of the effect sizes. Results, in Table 6, show that more recent papers have higher coefficients. Moreover, models that employ panel data structure of data are associated with lower effect sizes. Regression with property crimes tend to give lower estimates. The use of Gini measures is associated with higher coefficients. The evidence of publication bias seems less strong in this analysis, which reveals that high level of heterogeneity of results.

Concluding, the present work suggests that the effect of inequality on crime is likely to be modest, if not non-existent. This result calls for future research to analyze in greater detail this link. Indeed, it will be necessary to harmonize theoretical predictions with empirical evidences.

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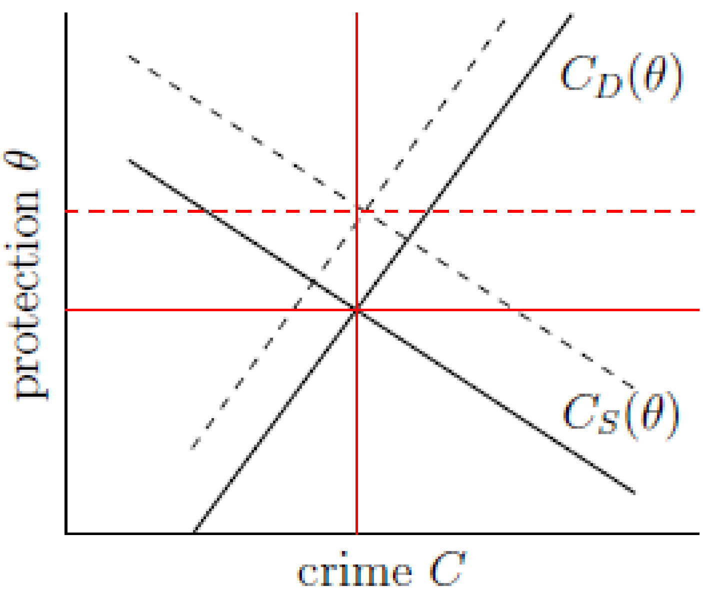
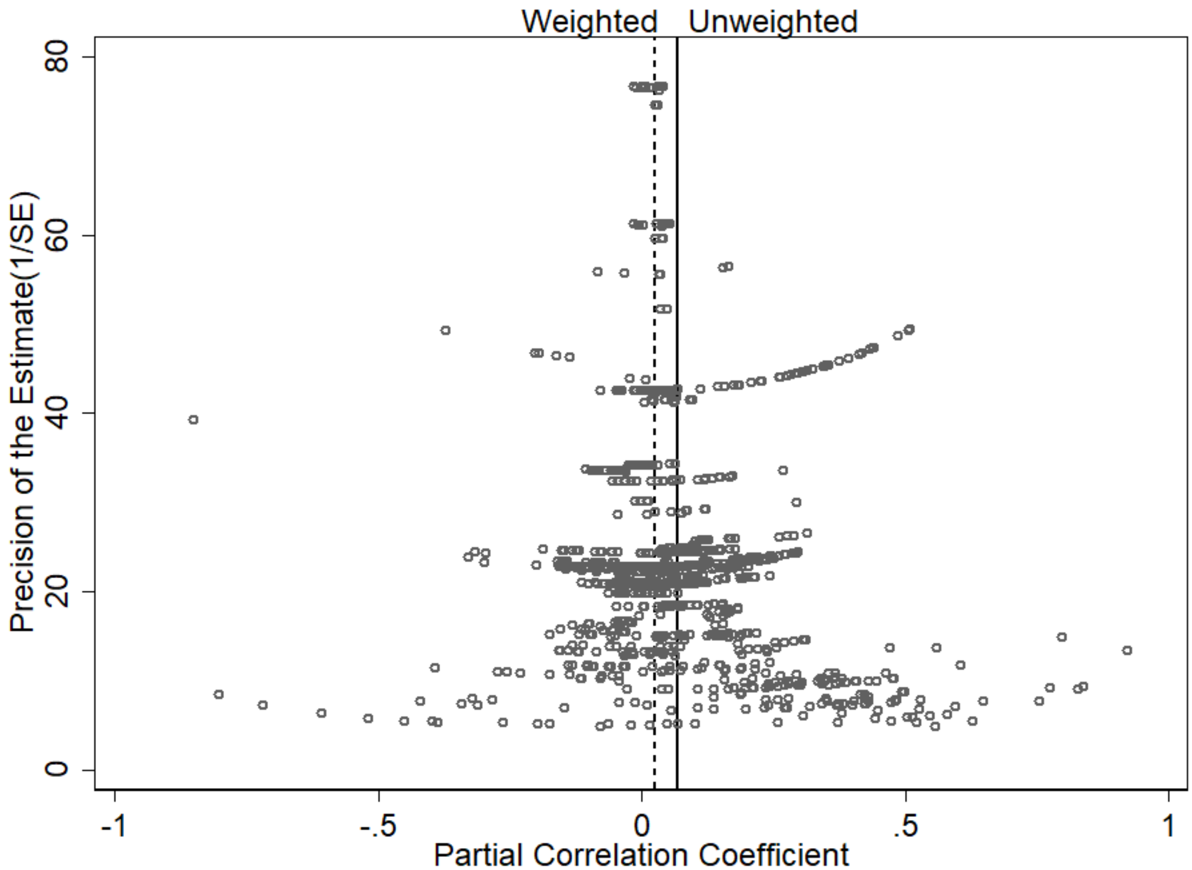
Figure 1: Model Figure



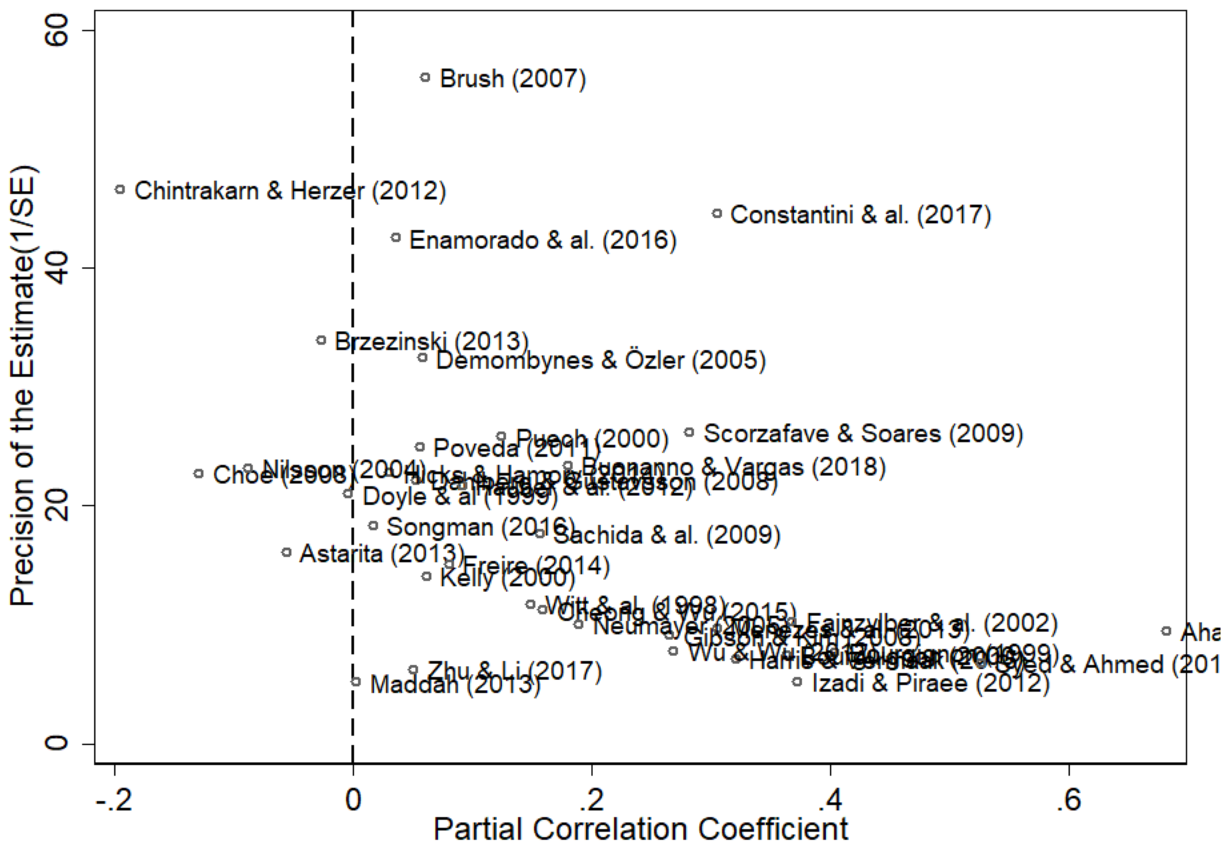
This figure represents the supply and demand curve of crime as a function of protection. Solid line represents the curves before the increase in inequality whereas the dashed ones after its increase.

Figure 2: Funnel Graph: All Estimates



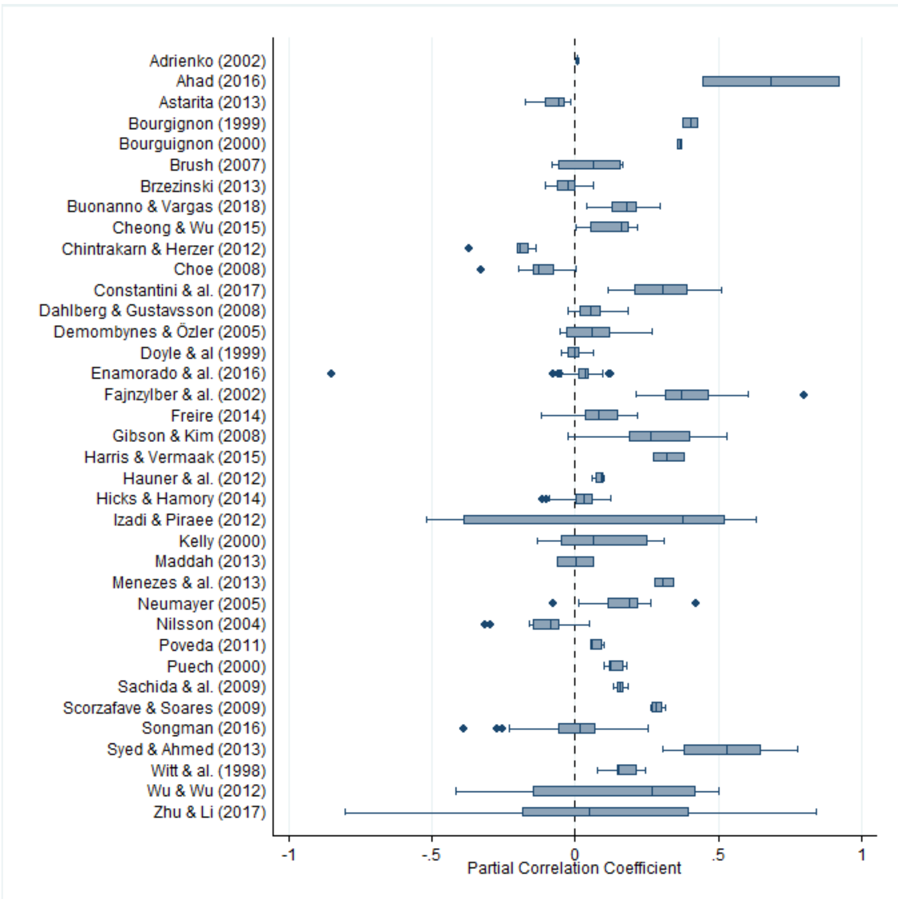
This figure shows a graphical analysis of the publication bias. It includes all the 1,130 estimates from the 37 studies. On the x axis there is the partial correlation coefficients and the y axis there is the inverse of their standard errors, which proxy for the precision of estimates. The solid vertical lines represent the unweighted average of the partial correlation coefficient. The dotted one represent the weighted average, using the inverse of the standard errors as weights.

Figure 3: Funnel Graph: Median Estimates



This figure shows a graphical analysis of the publication bias. It includes only the median estimate from each the 37 studiy. On the x axis there is the partial correlation coefficients and the y axis there is the inverse of their standard errors, which proxy for the precision of estimates. The dotted one represent the weighted average, using the inverse of the standard errors as weights.

Figure 4: H-Box



This figure shows an h-box that shows the heterogeneity of the partial correlation coefficients between and within study.

Table 1: List of Studies entering the Meta-Analysis

Ahad (2016)	Demombynes and Özler (2005)	Menezes et al. (2013)
Andrienko (2002)	Doyle et al. (1999)	Neumayer (2005)
Astarita (2013)	Enamorado et al. (2016)	Nilsson (2004)
Bourghignon (1999)	Fajnzylber et al. (2002)	Poveda (2011)
Bourguignon (2000)	Freire (2014)	Puech (2005)
Brush (2007)	Gibson and Kim (2008)	Sachsida et al. (2010)
Brzezinski (2013)	Harris and Vermaak (2015)	Scorzafave and Soares (2009)
Buonanno and Vargas (2017)	Hauner et al. (2012)	Syed and Ahmed (2013)
Cheong and Wu (2015)	Hicks and Hicks (2014)	Witt et al. (1998)
Chintrakarn and Herzer (2012)	Izadi and Piraee (2012)	Wu and Wu (2012)
Choe (2008)	Kang (2016)	Zhu and Li (2017)
Costantini et al. (2016)	Kelly (2000)	
Dahlberg and Gustavsson (2008)	Maddah (2013)	

Table 2: Mean Partial Correlation Coefficient

Variable	Mean	SE
All unweighted	0.069	0.167
All weighted by inverse SE	0.024	0.080
Median	0.150	0.187
Median weighted by inverse SE	0.168	0.214
All Weighted by inverse Impact Factor	0.141	0.232

This table shows the average of the partial correlation coefficients. The first row is the raw mean using all 1,130 estimates; the second is the weighted mean using the inverse of the standard errors as weight; the third is the average of the median partial correlation coefficients for each study; the fourth is the average of the median partial correlation coefficients for each study , weighted by the average inverse standard errors; the fifth is the average of all coefficients weighted by the inverse of the number of estimates per study.

Table 3: Summary statistics

Variable	Definition	Mean	Std. Dev.
Part Corr Coeff	Partial correlation Coefficient	0.067	0.163
Standard Error	S.E. of Partial correlation Coefficient	0.055	0.035
Years	Years since paper was published or uploaded in working paper series	5.590	5.101
Published	=1 if paper has been published in an academic journal	0.919	0.269
Google Citation	Total number of Google Scholar citations	61.068	161.454
Impact Factor	RePEc impact factor of the journal and working paper series	11.050	7.789
Observations	Total number of observations	2125.442	12388.988
Regressors	Total number of regressors	7.065	3.535
Single Country	Study based on one country	0.918	0.269
Panel Data	=1 if the data are panel	0.827	0.176
Time Series	=1 if the data are time series	0.034	0.371
Mexico	=1 if Mexico is examined	0.119	0.317
USA	=1 if USA is examined	0.482	0.5
Colombia	=1 if Colombia is examined	0.088	0.277
Property Crime	=1 if the crime variable belongs to the FBI property crime definition	0.543	0.476
crime Victimization	=1 if the crime variable is taken from a victimization survey	0.046	0.205
Gini	=1 if the inequality measure is GINI	0.643	0.487
Endogeneity	=1 if the endogeneity of inequality is taken care of	0.152	0.352
OLS	=1 if an OLS estimator is used	0.154	0.354
Time Dummies	=1 if time dummies are included	0.513	0.499
Reg/Prov/Block FE	=1 if regional/provincial or block fixed effects are included	0.053	0.297
Female Head Controls	=1 if the regression controls for the percentage of female head of family	0.385	0.482
Deterrence Controls	=1 if the regression controls for deterrence	0.478	0.498
Poverty Controls	=1 if the regression controls for poverty measures	0.550	0.5
Race Controls	=1 if the regression controls for race	0.448	0.5
Unemployment Controls	=1 if the regression controls for unemployment	0.652	0.471
GDP/Income Controls	=1 if the regression controls for GDP or any other income measure	0.821	0.413

Table 4: FAT-PET-MRA for detecting publication bias I, main results

	(1) WLS	(2) 2SLS-WLS	(3) MIXED	(4) FE	(5) RE	(6) OLS(no weights)
True Effect(λ_0)	0.01	0.00	0.02***	0.02*	0.02	0.01
	[0.01]	[0.00]	[0.01]	[0.01]	[0.01]	[0.04]
Publication Bias(λ_1)	1.18**	1.61*	1.36**	0.79**	1.36**	1.07*
	[0.50]	[0.88]	[0.61]	[0.35]	[0.56]	[0.55]
Random Effects (sd)			3.47			
			[0.43]			
No. Studies	37	37	37	37	37	37
Observations	1,130	1,130	1,130	1,130	1,130	1,130
Instrument		-0.96***				
		[0.15]				
F statistic		38.45				

This table shows the formal test for testing publication bias using various econometric techniques. Column (1) is the unrestricted weighted least square model as in 14. In column (2) we instrument the standard error by the square root of the degrees of freedom. The F.-statistics below is the Kleibergen Paap weak instrument statistic. In column (3) we use mixed model estimated through maximum likelihood. We specified random effects at the paper level. We also report the standard deviation of the study random effects. Column (4) reports a fixed effect model, whereas a standard random effects one in (5). Column (6) consider an OLS without any weighting. All regressions employ clustered standard errors at the study level to control for within study dependence.

Table 5: FAT-PET-MRA for detecting publication bias II, alternative results

	(1) Median	(2) No Andrienko and Constantini	(3) Median NA	(4) N Studies	(5) Google	(6) IF>1	(7) IF>10
True Effect(λ_0)	0.00	-0.01	-0.07	0.03	-0.01	0.01	0.00
	[0.00]	[0.06]	[0.08]	[0.05]	[0.02]	[0.01]	[0.02]
Publication Bias(λ_1)	1.88***	1.15**	2.88***	1.42**	1.01	1.24**	1.29
	[0.52]	[1.06]	[1.09]	[0.60]	[0.68]	[0.58]	[0.87]
No. Studies	37	35	35	37	37	37	37
Observations	37	1,115	35	1,130	1,006	843	718

This table shows various robustness test for publication bias. All the models, but the one in (4), estimate an unrestricted weighted least square, as in 14. In column (1) we consider only the median estimate per study. In (2) we use all estimates but excluding Andrienko (2002), which the work with the smallest standard errors. In (3) we replicate the regression of column (1) excluding Andrienko (2002). In column (4) we consider a different weighting scheme, as the inverse of the number of studies per work. We do so to give less weights to estimates taken from studies with many reported coefficients. In column (5) we consider only the work with more than 10 citations, whereas in (6) only thos with impact factor greater than 1. Finally, in column (7) only thos with impact factor greater than 10. All regressions consider clustered standard errors at the study level to control for within study dependence.

Table 6: Modelling Heterogeneity I

	(1)	(2)	(3)	(4)	(5)	(6)
Study Characteristics						
Years	-0.02**					-0.01**
	[0.01]					[0.01]
Published	-0.06					-0.05
	[0.07]					[0.06]
Google Citations	0.00***					0.00
	[0.00]					[0.00]
Impact Factor	-0.01					0.00
	[0.01]					[0.00]
Data						
Observations		-0.00				-0.00**
		[0.00]				[0.00]
Regressors		-0.01				-0.01**
		[0.01]				[0.01]
Single Country Study		0.00				-0.38***
		[0.08]				[0.14]
Panel Data		-0.11***				-0.17***
		[0.03]				[0.04]
Time Series		0.14				0.09
		[0.18]				[0.16]
Mexico		0.02				0.10
		[0.05]				[0.13]
USA		0.07				0.07
		[0.06]				[0.06]
Colombia		0.03				-0.03
		[0.03]				[0.08]
Econometric Issues						
Endogeneity Inequality			0.01			0.02
			[0.03]			[0.01]
OLS			0.05			0.04**
			[0.04]			[0.02]
Crime & Inequality						
Property Crime (Monetary)				0.00		-0.01*
				[0.02]		[0.01]
Crime Victimization				-0.04		-0.14
				[0.05]		[0.14]
Gini				-0.00		0.07**

				[0.04]		[0.03]
Controls						
Deterrence					0.09***	0.07**
Time Dummies					-0.06***	-0.04*
					[0.02]	[0.02]
Reg/Prov/Block FE					-0.07**	-0.01
					[0.03]	[0.02]
Female Head					0.04	-0.04
					[0.04]	[0.04]
					[0.03]	[0.03]
Poverty_Control					-0.00	-0.02
					[0.03]	[0.03]
Race					-0.10	-0.09
					[0.06]	[0.05]
Unemployment					0.01	0.21***
					[0.05]	[0.06]
GDP/Income Controls					-0.06	0.00
					[0.05]	[0.04]
Standard Error	-0.47	0.14	0.97*	0.35	-0.26	0.15
	[1.11]	[1.30]	[0.54]	[1.24]	[0.78]	[0.61]
Constant	0.29**	0.19***	0.01	0.04	0.14*	0.53***
	[0.12]	[0.06]	[0.01]	[0.09]	[0.07]	[0.16]
Observations	1,130	1,130	1,130	1,130	1,130	1,130
R-squared	0.27	0.24	0.08	0.08	0.38	0.58

This table shows the impact of the moderator variables in explaining the heterogeneity of partial correlation coefficient. All the models estimate an unrestricted weighted least square, as in 14. In the column (1) through (5) we add each block of variables at the time. In column (6) we add all of them together. Definition of the moderator variables can be found in section 5.1. All regressions consider clustered standard errors at the study level to control for within study dependence.

9. Appendix of the Model

Individuals derive an utility u for this income. We assume that u is well behaved (continuous) and increase monotonically with respect to income at a diminishing rate. That is, $u' > 0$ and $u'' < 0$. The Arrow-Pratt measure of relative risk aversion $R(y) = -yu''(y)/u'(y)$ would be important in what follows. The net gain equation (1) for a generic utility function is

$$v(y,y') = (1-q)[u(y+\beta y') - u(y)] + q[u(y(1-f)) - u(y)] \tag{16}$$

The derivative on y' is trivial. Given that $u' > 0$, we have that $\partial v/\partial y' > 0$ for any y'. The question is whether this net utility decrease with y, or not. The following proposition gives a necessary condition for this.

Proposition. If $R(y) \leq 1$ for all y, then v is strictly decreasing with y. That is $\partial v/\partial y < 0$ for all y.

We take derivative of v. The first term is negative given that u' is decreasing. For the second term, we need to evaluate the $u'(y(1-f))(1-f) - u'(y)$. We notice that this expression is zero for $f = 0$. We take the derivative in f and we have $-u''(y(1-f))(1-f)y - u'(y(1-f)) = u'(y(1-f))[R(y(1-f)) - 1]$. If $R(y) \leq 1$ for all y, this derivative is less or equal to zero in all the y domain. As $u'(y(1-f))(1-f) - u'(y)$ is zero for $f = 0$ and non increasing for $f > 0$, then this expression is negative for any value of f and y. This prove the proposition.

The previous proposition gives a necessary condition to have a sign on the derivative. In the case described in the main text, we have that $u(y) = log(y)$ with $R(y) = 1$. Accordingly, we have that v is strictly decreasing with y and we have the result in (4).

One of the few papers that casts some doubts on the relation between inequality and crime, Deutsch et al. (1992) gives an argument based on this. In a model similar to ours, they are not able to assign a sign to incentives to crime for an arbitrary utility function. While we agree that this cannot be done for general utilities, the use a function which derivative is positive helps us to construct the basic argument, which is against our final claim.