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Revisiting the Income Inequality-Crime Puzzle

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ABSTRACT

The economics literature generally supports a positive theoretical link between income inequality and crime. However, despite this consensus, empirical evidence has struggled to yield definitive conclusions. To address this puzzle, I conducted a meta-analysis based on 1,341 estimates drawn from 43 studies in economics journals. The findings indicate a statistically significant but economically insignificant true effect of inequality on crime, ranging between 0.007 and 0.123 using UWLS FAT-PET and advanced methods. In essence, if there is an impact of inequality on crime, it is, at best, minimal. Additionally, there is some limited evidence suggesting positive publication bias. Results from Bayesian model averaging reveal that inequality does not affect exclusively property crime, as predicted by the rational choice models. Moreover, this analysis shows that inequality measures which are sensitive to changes in income at the middle and top of the distribution are associated with higher coefficients. The study also underscores the biases arising from the exclusion of relevant variables. The implications of this research suggest that inequality may not be the primary motivator for criminal behaviour, with other factors potentially playing more significant roles. Lastly, if inequality does affect crime, it might do so in different ways than those discussed by the majority of the existing empirical studies.

1. Introduction

According to the economic theory of crime, the incentives for individuals to commit a crime depend positively on the differential between illegitimate and legitimate returns (Ehrlich, 1973). An increase in inequality might widen such a differential, by lowering the opportunity cost of those at the bottom of the income distribution and/or increasing the gains of crime, due to richer potential targets. Following this cost–benefit analysis, most of the theoretical literature has formulated a positive relationship between income inequality and crime (Chiu & Madden, 1998; Merlo, 2004).[1] Despite these theoretical predictions, the empirical evidence presents contradictory results. Fajnzylber, Lederman, and Loayza (2002) found that income inequality is associated with higher levels of murder and robbery, while Demombynes and Özler (2005) found that in South Africa inequality is a strong predictor of property crime but a much weaker predictor of violent crime. Kelly (2000) found that inequality is not related to property crime but is related to violent crime. In a study of US counties, Brush (2007) showed that a cross-sectional approach produced a positive association but a negative association for time series. The empirical studies included in this work demonstrate that about 43.1% of the total number of estimation coefficients are positive and significant.[2] How can such evidence be reconciled with theoretical predictions? This paper contributes to this topic by critically discussing and summarising the relevant empirical work in the economics literature within a meta-analysis.

Meta-analysis involves performing statistical analysis on data collected from multiple studies, to systematically organise the research findings (Stanley & Doucouliagos, 2012). Meta-analysis techniques are currently applied in many different fields, such as medicine, biology and political science (Borenstein, Hedges, Higgins, & Rothstein, 2021; Gurevitch, Koricheva, Nakagawa, & Stewart, 2018). In economics, meta-analysis is gradually becoming a common tool among researchers (Havránek et al., 2020). My work includes studies published in economic journals which report empirical estimates of the effect of inequality on crime. The main reason to focus exclusively on economics is that, to my knowledge, no meta-analysis exists in this literature, while there are several in other fields (Kim, Seo, & Hong, 2020; Nivette, 2011; Pratt & Cullen, 2005). By focusing exclusively

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[1] Only a few theoretical works explore the possibility that such a relationship might be more complex (Bourguignon, Nuñez, & Sanchez, 2003; Corvalan & Pazzona, 2022; Deutsch, Spiegel, & Templeman, 1992). Other powerful approaches, such as those based on strain or social disorganisation theory (Merton, 1938; Shaw & McKay, 1942), lead to a similar positive relationship (Blau & Blau, 1982; Messner, Raffalovich, & Shrock, 2002). Merton's strain theory suggests a direct link between inequality and crime. Social disorganisation theory assumes a channel mediated through lower social capital. These theories are also better equipped to explain violent crime than the rational choice model.

[2] See Table 1 for more detail.

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on economics, I can also limit the large differences in theoretical and methodological approaches with other sciences. The search has produced a total of 43 studies, and 1341 estimates, which I have converted into partial correlation coefficients (PCC) for comparison. Moreover, I collected precise information on several dimensions, including the crime and inequality measures for each study. By employing meta-analysis techniques I have two main objectives. The first is to estimate the average effect size of the inequality-crime relationship, net of publication bias (if present). The second is to identify the characteristics of the regressions and studies that affect the estimates.

Publication bias exists whenever some results have higher probabilities of being published compared to others (Brodeur, Cook, & Heyes, 2020). For example, statistically significant results might be more likely to find their way into publication, as well as those that confirm some initial hypotheses. Publication bias does not have a single source: it could depend on both the choice of researchers submitting a study for publication and the individuals involved in the publication process, such as referees or editors (Stanley & Doucouliagos, 2012). The extent of publication bias in economics is widespread (Blanco-Perez & Brodeur, 2020; Brodeur et al., 2020) but also in other fields (Franco, Malhotra, & Simonovits, 2014). The consequence of publication bias is to severely distort the understanding of a specific topic (Stanley et al., 2013). In my case, it is particularly relevant to inspect the presence of publication bias because the theoretical literature almost unambiguously points towards a positive relationship between inequality and crime (Chiu & Madden, 1998; Ehrlich, 1973; Merlo, 2004). To detect and correct publication bias in the meta-analysis, I use various linear and non-linear techniques, including advanced ones which have been proposed recently in the literature. I also present results using several estimation techniques, to account for model dependency. Using my preferred methods – the funnel asymmetry and precision effect tests (FAT-PET), calculated through unrestricted weighted least squares, and the advanced tests – I find two main results. Firstly, the true values of the partial correlation coefficients – net of publication bias – are statistically but not economically significant. They are in the range 0.007–0.123, which represents non-existent or small effects, according to the guidelines provided by Doucouliagos (2011). Secondly, I also find some limited evidence of positive publication bias (preference for positive results), but its presence is limited.

The estimated effects, albeit corrected for the presence of publication bias, can still display high level of heterogeneity depending on several characteristics of the models and studies. In the second part of Section 5, I consider how such characteristics affect the signs and magnitudes of the estimated effects. To guide this analysis, in Section 2, I provide a comprehensive literature review, which demonstrates that the empirical literature has examined various types of crime, while the economic theory provides solid insights only for activities that offer economic gains. Moreover, I note the lack of consensus regarding the choice of inequality measures, which derives from the difficulty of clearly representing the costs and benefits of committing a crime. Additionally, I argue that the typical regression is likely to suffer from the omission of variables that are simultaneously related to both inequality and crime. For example, failing to control for deterrence – whether public or private – might cause severe bias in the coefficients. To test the role of the regressions and studies characteristics in explaining the direction and statistical significance of the effect sizes, I used a Bayesian model averaging (BMA). This analysis confirms that inequality affects not only property but also violent crime. In addition, it shows that inequality measures which are sensitive to changes at both the middle and top of the income distribution have greater coefficients. Finally, it demonstrates that controlling for income and poverty reduces the importance of inequality, as predicted by Pridemore (2011).

This work relates and contributes to various strands of literature, and to two in particular. The first concerns the impact of inequality on crime in economics (Demombynes & Özler, 2005; Enamorado, López-Calva, Rodriguez-Castelán, & Winkler, 2016; Fajnzylber et al., 2002; Kelly, 2000). Given the contradictory results of this literature, my work contributes to it by systematically ordering and summarising the research findings. The findings that inequality does not affect exclusively property crime suggests that the rational choice model approach might not be the only theoretical background used in the economics literature. It should be accompanied by others, such as those based on strain or social disorganisation theories (Merton, 1938; Shaw & McKay, 1942), used in criminology and sociology.[3] This work also provides guidance concerning the variables to be included in regressions which evaluate the impact of inequality on crime. As an illustration, my research indicates that neglecting to account for deterrence, a factor overlooked in 47.7% of the models (as detailed in Table 2), results in a negative bias. This pattern also holds true for the influence of income and poverty.[4] The second strand of the literature is the multidisciplinary one which performs a meta-analysis on inequality and crime.[5] To my knowledge, this is the first meta-analysis to systematically summarise the economic literature on inequality and crime. Similar exercises, however, have been undertaken by criminology scholars. There are three studies closely related to mine: Kim et al. (2020), Nivette (2011), and Pratt and Cullen (2005). These works test the importance of several determinants which are mainly related to theories of deprivation and social disorganisation. They found correlation coefficients higher than the ones found in this research and no evidence of publication bias. There are methodological differences between this research and those studies. First of all, I focus exclusively on inequality, which allows me to explore in detail the heterogeneity in crime categories, income measures and regression characteristics. I also open the black box on property crime, whereas the existing literature focused mainly on violent crime (most often murder).[6] To detect publication bias, I always provide estimates which control for the presence of standard errors and employ advanced methods. Finally, I consider several moderators, in order to explain the heterogeneity of the coefficients, using Bayesian techniques. Indeed, my study could be useful as a comparison between different disciplines which study the link between income inequality and crime.

There are direct implications from my results. Firstly, the analysis underscores the necessity for theoretical models to provide more compelling explanations regarding the influence of inequality on non-property crime. Secondly, it also shows the importance of selecting inequality measures based on sensitivity to different parts of the income distribution. Finally, this work emphasises that scholars should further investigate the interaction between inequality and other variables related to the economic benefits and cost of crime, in order to obtain less biased estimates.

The paper is organised as follows. Section 2 provides a review of the main theoretical and empirical contributions of the literature on the relationship between inequality and crime. Section 3 then presents the dataset employed in the empirical exercises. Section 4 concerns the methodology, and Section 5 the empirical findings. Section 6 presents a discussion and concluding remarks.

2. Literature review

This section discusses the possible reasons for the misalignment between empirical findings and theoretical predictions, initially focusing on the measurement of crime and inequality.

[3] As done, for example, by Kelly (2000).

[4] It is noteworthy that Table 2 specifies the estimates that do include deterrence, i.e., 52.3%.

[5] There are very few meta-analytic studies – of any type – in the literature on the economics of crime (Higney, Hanley, & Moro, 2022).

[6] Kim et al. (2020) was the first to consider non-violent crime, although 73% of the studies in the meta-analysis considered homicides.

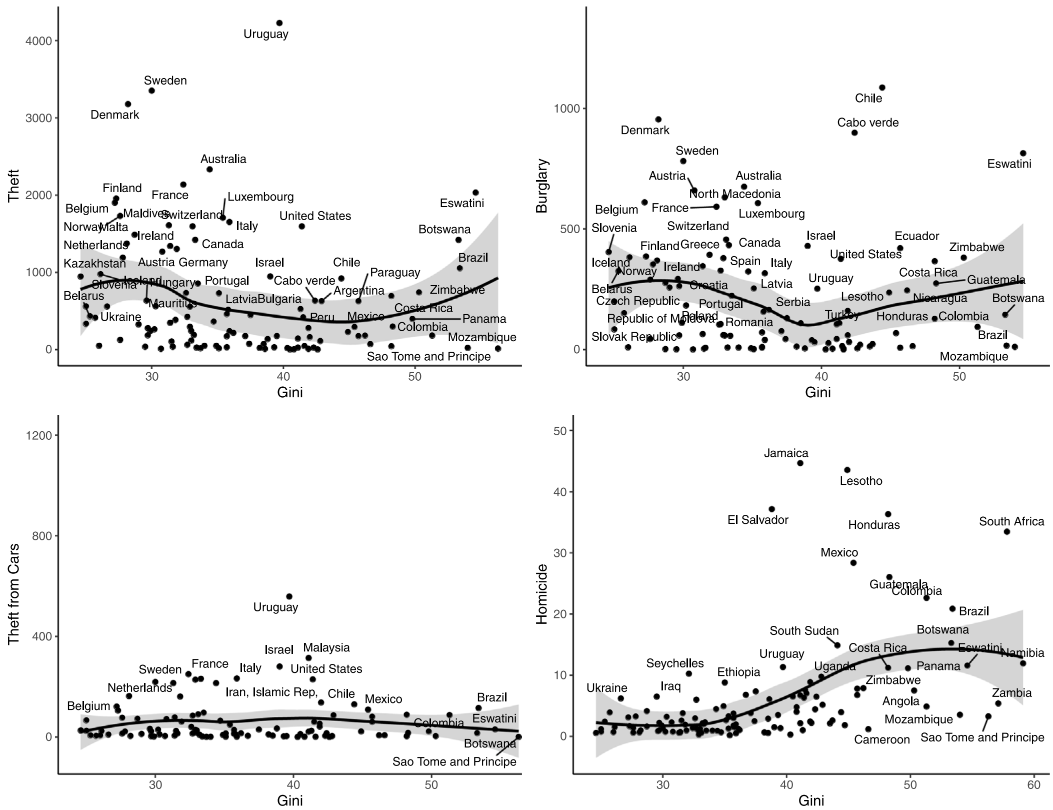

Fig. 1. Crime categories vs. Gini coefficient, international comparison. This figure shows four scatter plots with the Gini coefficient (x-axis) versus the rate of crime (y-axis, x100 inhabitants) for different categories. The solid line represents the smoothed conditional mean, and the shaded area is the 95% confidence interval. Data for crime have been taken by the UNODC, while the data for inequality from the World Bank. The latest available year for each variable is considered.

2.1. Crime

In his seminal economic theory of crime, Becker (1968) considered all types of crime. The pay-off that individuals receive from committing a crime should not be interpreted exclusively as pecuniary, but also as psychological. However, most of the subsequent theories focus on crimes that lead to economic gains. An example is the statement of Josten (2003), p.440, that "The only purpose of crime is to obtain the monetary reward". In a discussion of the positive correlation between inequality and crime, Chisholm and Choe (2005) argue that it is more valid for property crime – such as burglary – than violent crime. Bourguignon (2000), Chiu and Madden (1998), Corvalan and Pazzona (2022), Deutsch et al. (1992), Imrohoroglu, Merlo, and Rupert (2000) and Merlo (2003) have all considered property crime. When the theory was first tested with data, only offences leading to economic gains were considered (Ehrlich, 1973).

However, some works suggest that violence is also consistent with the economic model of crime, albeit more mildly. According to Bourguignon (2000), p.206: "it certainly cannot be ruled out that homicides, intentional or not, are more common among poor and less educated people and in areas where police are less present". Some murders and acts of physical violence are directly linked to property crimes (Dix-Carneiro, Soares, & Ulyssea, 2018). The murder rate in a given area may be determined by many of the same variables as the rate of property crime. However, given the exceptional nature of murder, the relationship between variables is likely to be weaker than for property crime.

There are several types of crime, and inequality could indeed have different effects on each of them. To illustrate this point, Fig. 1 displays simple international correlations between inequality – measured with the Gini coefficient – and several types of crime. While inequality shows a positive correlation with murder rates, the relationship with property crimes is typically non-linear.

The empirical literature employs several crime variables, which are often aggregated. As discussed in the introduction, the results are mixed. For example, Kelly (2000) found an effect on assault and robbery but not on murder, auto theft, burglary, rape or property theft. However, Demombynes and Özler (2005) found no effect for assault but a positive and statistically significant effect for auto theft and burglary. Enamorado et al. (2016) and Fajnzylber et al. (2002) found evidence of a positive and statistically significant effect for murder.

When measuring crime, most empirical studies employ official crime data, usually recorded by the police, even though this is affected by under-reporting. The resulting measurement error would not be problematic if it was randomly related to crime determinants, but this is seldom the case (MacDonald, 2001). The existing literature tries to overcome such an issue by using those categories of crime that are least affected by under-reporting, such as murder and auto theft. A further solution to the measurement problem could be the use of victims-of-crime surveys, which ask directly about individual experiences. A study by Gibson and Kim (2008) showed that measurement errors in official crime data underestimate the role of inequality in crime. When data from surveys is used, the role of economic inequality in crime becomes more pronounced.

2.2. Inequality

The choice of an appropriate measure of inequality, for both theoretical and empirical works, is not trivial: there are many different measures, each with particular features (Cowell, 2011). At least three decisions regarding the theoretical modelling of inequality have implications which relate to the empirical applications.

Firstly, inequality typically considers income, but can also be calculated for consumption, expenditure or wealth. Economic theory suggests that inequality of income is the main factor driving crime. As a result, most of the studies work directly with income distribution or related measures. For example, many authors use the underlying distributions of human capital (Josten, 2003) or ability (Imrohoroglu et al., 2000), which in turn determine individual incomes. Few authors consider non-income inequality, such as Deutsch et al. (1992) who focused on wealth.

Secondly, a major difference between inequality measures lies in their sensitivity to changes in different parts of the income distribution (Shorrocks & Foster, 1987).[7] While there is consensus about the use of income inequality, there is ambiguity regarding the part of the income distribution which is most relevant to proxy the net gains from crime. The incentives for committing a crime depend on the benefits of legal activities (the opportunity costs of engaging in crime) and the gains (illegitimate wages or pay-offs). Consequently, there is no theoretical consensus on whether the bottom, middle or top-weighted inequality is most likely to influence the rate of crime in a given society (Bourguignon et al., 2003).

The seminal contribution of Ehrlich (1973) assumed that individuals well below the median income have greater incentives to undertake crime. Consequently, median income level and relative inequality (measured as the income of the poor divided by the median income) are positively related to the incidence of property crime. Bourguignon (2000) used the same measure, the so-called relative poverty "shortfall". These theories favour the use of bottom-weight measures of inequality. Some other works focus on middle-weight measures. Chisholm and Choe (2005) state that the net gains from crime can be expressed as a product of the Gini coefficient with the mean income of the given society. Imrohoroglu et al. (2000) use the variance of the distribution. Several theoretical works rely on the general properties of inequality indexes: Corvalan and Pazzona (2022) and Deutsch et al. (1992) consider inequality to be a principle of transfer between two groups. Josten (2003) assumes that a more unequal distribution is a mean-preserving spread in the distribution of endowments. Chiu and Madden (1998) consider relative differential inequality, which is associated with Lorenz worsening of any sub-interval of incomes.[8]

To summarise, theoretical contributions do not offer a clear indication of which inequality measures should affect crime, or how they should do this.

2.3. Omitted factors

Many variables could simultaneously affect income inequality and levels of crime. As mentioned earlier, the rational choice model interprets income inequality as capturing the average differential returns from illegal activity. However, a single measure is unlikely to be able to include the full range of criminal costs and benefits. As a result, controlling for other variables that affect the incentives of crime is recommended. For example, poverty, unemployment rates and unskilled wages could be considered proxies for the cost of crime.[9] On the other hand, some income measures related to the targets of the criminals should be included. Demombynes and Özler (2005) state that the inequality measure should not necessarily be related to crime if all the costs and incentives of crime are taken into consideration.

A similar argument could be made for another key variable, deterrence, which is likely to be negatively associated with the level of crime (Di Tella & Schargrodsky, 2004). Deterrence is also likely to be related to income inequality: an increase in the incomes of the rich provides incentives to invest in public or private protection (Chiu & Madden, 1998; Jayadev & Bowles, 2006; Merlo, 2003). Corvalan and Pazzona (2022) show that the relationship between crime and inequality may be ambiguous if private protection is not controlled for. However, regressions that include it should recover a positive coefficient.

In general, as pointed out by Brush (2007), there may 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 biases (Gibson & Kim, 2008). Relatedly, Brush (2007) found that cross-sectional studies may report higher coefficients than panel data or time series. Another issue would be reverse causality (Barenboim, 2007). Finally, many authors noted how race and family variables may both be correlated with inequality and crime (Blau & Blau, 1982; Glaeser & Sacerdote, 1999).

3. Meta-dataset

In this section, I present the data employed in the meta-analysis exercises.

3.1. Search criteria & effect sizes

I searched for published studies reporting empirical estimates of the effect of inequality on crime. I employed search engines including Google Scholar, Research Gate, ISI Web of Science and Econlit and entered keywords such as "inequality/inequitable development/income distribution" and "crime/criminal activity(ies)/illegal behaviour". I searched only for studies written in English and published in economics journals, as defined in IDEAS/RePEc (2023).[10]

To be included in the meta-analysis, a study must report standard errors or other statistics that allow their computation, such as t-statistics or p-values. In the Appendix, I specify all the selection criteria employed, which follow the suggested guidelines for meta-analytic studies in economics (Havránek et al., 2020).[11] The search produced a total of 43 studies, listed in Table 1.

The most basic cross-section econometric regression (i) employed in a typical study (j) on the effect of inequality on crime takes the following form:

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

where *Crime* might be any crime variable, *Inequality* refers to any income-related measure of inequality, such as wages, wealth, and consumption/expenditures. X represents a series of control variables employed in the regressions. Within the retrieved 43 studies I obtained a total of 1341 effect sizes – $\hat{\theta}$ – slightly more than 32 per work. Table 1

[7] For a discussion on how the measurement of bottom incomes affects inequality measures, look at Hlasny, Ceriani, and Verme (2020).

[8] Other characteristics of inequality measures may have an impact on crime. For example, it might be relevant to differentiate between absolute or relative inequality (Kolm, 1976). Ehrlich (1973) states that the crime rate is a positive function of the absolute differential returns of crime. As already mentioned, Chisholm and Choe (2005) implicitly considers absolute inequality. Nevertheless, other works state that relative inequality may affect crime. Chiu and Madden (1998) present a theory about relative differential inequality, while Bourguignon et al. (2003) consider the distribution of relative incomes. I do not test differences in this dimension because almost all studies report relative measures of inequality. Similarly, I do not explore the role of non-linearities in the effect of inequality on crime. A few works explore these issues, including Buonanno and Vargas (2019) and Thornton, Bhorat, Lilenstein, Monnakgotla, and van der Zee (2023).

[9] For example, Pridemore (2011) found that inequality becomes not statistically significant, once poverty is controlled for.

[10] The search was concluded on the 5th of July 2023. I did not consider unpublished studies because it has been shown that the inclusion of unpublished studies does not reduce the level of publication bias and makes it more difficult to distinguish between economics and other fields (Rusnák, Havranek, & Horváth, 2013). If anything, Brodeur, Carrell, Figlio, and Lusher (2021) showed that editorial decisions decrease bias.

[11] This work details several points related to all the stages of a meta-analysis, from the definition of the research question and effect sizes to the reporting and interpretation. For example, the authors should report "the exact databases or other sources used; the precise combination of keywords employed; and the date that the search was completed" (Havránek et al., 2020, p.471).

Table 1
Summary of the studies in the meta-analysis.

Study	Crime variables	Inequality measure	Numb regressions	Numb Pos. & Signif
Adekoya (2019)	Property	Gini	4	3
Adeleye and Jamal (2020)	Violent	Gini	26	24
Ahad (2018)	Aggregate	Gini	2	2
Anser et al. (2020)	Violent	Gini	1	1
Astarita (2013)	Violent, Property	Gini, Quintile Ratio, Theil	26	0
Atems (2020)	Aggregate	Gini, Top Share, Theil	29	19
Brush (2007)	Aggregate	Gini, Top Share	4	2
Brzezinski (2013)	Violent, Property	Top Share	36	2
Buonanno and Vargas (2019)	Violent, Property, Aggregate	Atkinson, Gini, Theil	90	86
Cheong and Wu (2015)	Aggregate	Gini, Quintile	14	14
Chintrakarn and Herzer (2012)	Property	Gini, Top Share	5	0
Choe (2008)	Violent, Property	Gini	40	0
Coccia (2018)	Violent	Gini	6	6
Costantini, Meco, and Paradiso (2018)	Property	Gini, Top Share	35	35
Dahlberg and Gustavsson (2008)	Property, Aggregate	Gini, Variance	20	6
Demombynes and Özler (2005)	Violent, Property	General Entropy	25	12
Di Matteo and Petrunia (2022)	Violent	Gini, Quintile	35	4
Distefano, Ferrante, and Reito (2019)	Property	Gini	4	4
Doyle, Ahmed, and Horn (1999)	Property	Gini	36	0
Enamorado et al. (2016)	Violent, Property	Gini	134	71
Fajnzylber et al. (2002)	Violent, Property	Gini, Quintile, Polarisation	24	24
Gibson and Kim (2008)	Violent, Property	Gini	36	25
Goh, Kaliappan, and Ishak (2018)	Violent	Gini, EHII	3	2
Harris and Vermaak (2015)	Violent	Gini	3	3
Hauner, Kutan, and Spivey (2012)	Violent, Property, Aggregate	Variance	7	5
Hicks and Hicks (2014)	Violent, Property	Gini	225	33
Izadi and Piraee (2012)	Property	Atkinson, Gini	9	0
Kang (2016)	Violent, Property	Gini, Theil	149	4
Kelly (2000)	Violent, Property	Gini	15	6
Li, Wan, Wang, and Zhang (2019)	Property	Gini, Polarisation	123	85
Maddah (2013)	Aggregate	Quintile	2	0
Manea, Piraino, and Viarengo (2023)	Violent, Property	Factor Analysis	7	5
Menezes, Silveira-Neto, Monteiro, and Ratton (2013)	Violent	Gini	3	3
Neumayer (2005)	Aggregate	Gini, Quintile	14	6
Poveda (2011)	Violent	Gini	9	4
Sachsida, de Mendonça, Loureiro, and Gutierrez (2010)	Violent	Gini	10	10
Scorzafave and Soares (2009)	Property	Gini	4	4
Song, Yan, and Jiang (2020)	Aggregate	Quintile	70	31
Syed and Ahmed (2013)	Property	Gini	5	5
Thornton et al. (2023)	Property	Gini	8	5
Witt, Clarke, and Fielding (1998)	Property	Quintile	5	4
Wu and Wu (2012)	Violent, Property	Gini	22	13
Zhu and Li (2017)	Violent, Property, Aggregate	Quintile	28	10

This table classifies the crime measures based on the monetary loss criteria (see the text for more detail). As such, this classification does not necessarily reflect the original crime categories employed by the authors of the individual studies.

provides a general description of the crime and inequality variables employed. It also displays, in the last two columns, the total number of $\hat{\theta}$s per study, and the ones that are positive and significant at the 10% level.

3.2. Characteristics of regressions and studies

Based on the reading of the literature – described in Section 2 – I gathered information about several factors that were likely to affect the estimates of inequality on crime.

Firstly, I classified the dependent variable, i.e. the crime measures. I grouped them into three binary variables: *Property*, *Violent* and *Mixed Crime*. The first takes value one if the crime involves a tangible economic loss (from a victim's perspective), independently of the use of violence. Accordingly, a robbery is considered to be a property crime (Hernández, Hunt, Pazzona, & Vásquez Lavín, 2017). I decided to classify crimes according to such criteria in order to capture economically motivated offences, which should adhere better to the rational choice model. However, it is difficult to draw a neat line between different types of crime. *Mixed Crime* includes crime indexes, or groups of specific crimes, that are violent and/or economically motivated. As shown in Table 2, the relative majority – 41% – of all the crimes are *Property Crime*. Additionally, the Appendix categorises crimes following the approach used by the FBI: a crime is a property crime if an economic gain was obtained without the use of violence. The Appendix also presents separate categories of crime: *Homicide*, *Auto Theft*, *No Auto Theft*, *Robbery* and *Burglary*. As described in Section 2, the first two categories represent types of crime with high reporting rates, which, according to Gibson and Kim (2008), may produce higher estimates. The last three represent the most typical property crimes which – together with *Auto Theft* – allow me to further evaluate the hypothesis that inequality affects property crime more than violent crime. To test whether regressions that employ victims-of-crime surveys – rather than officially reported data – are associated with higher estimates, I also code a variable that takes value one if the crime data employed is from a survey and value zero otherwise. Unfortunately, only 2.7% of the regression employs this data. To assess the role, and direction, of measurement errors on the estimates, I created a binary variable that takes value one if the regression employs instrumental variables techniques. In the Appendix, I will also consider crime categories that are less likely to suffer from measurement error.

Secondly, I classify the inequality variables. Based on the discussion in Section 2, I test whether measures sensitive to changes to different parts of the income distribution provide heterogeneous incentives to potential criminals. Accordingly, I create three binary variables: *Inequality Bottom Weight*, *Inequality Middle Weight* and *Inequality Top Weight*. As the names of the variables suggest, the first gives more

Table 2
Description of the variables and summary statistics.

Variable	Definition	Mean	Std. Dev.	WM
PCC	Partial Correlation Coefficient	0.08	0.15	0.06
SE(PCC)	PCC Standard Error	0.06	0.03	0.03
Property Crime	Property Crime, Monetary Loss	0.41	0.49	0.44
Violent Crime	Violent Crime, Monetary Loss	0.32	0.47	0.36
Mixed Crime	Total Crime, Monetary Loss	0.27	0.45	0.20
Theft (excl. auto)	Crime is Theft, except Motor Vehicle	0.14	0.35	0.11
Auto Theft	Crime is Motor Vehicle Theft	0.00	0.05	0.00
Robbery	Crime is Robbery	0.07	0.26	0.05
Burglary	Crime is Burglary	0.08	0.27	0.06
Homicide	Crime is Homicide	0.23	0.42	0.41
Crime Victimisation	Data are from Victimisation Survey	0.03	0.16	0.00
Inequality Bottom Wt	Measure sensitive at the Bottom of Income Distribution	0.28	0.45	0.15
Inequality Top Wt	Measure sensitive at the Top of Income Distribution	0.07	0.26	0.70
Inequality Middle Wt	Measure sensitive at the Middle of Income Distribution	0.65	0.48	0.15
Gini	Gini Inequality Measure	0.58	0.49	0.66
Theil	Theil Inequality Measure	0.13	0.34	0.08
Decile/Quintile Ratio	Any Decile/Quintile Ratio Inequality Measure	0.11	0.32	0.04
Income Inequality	Inequality Measure using Income	0.70	0.46	0.80
Cons/Exp Inequality	Inequality using Consumption and Expenditures	0.22	0.41	0.15
Unemployment	Control for Unemployment	0.59	0.49	0.50
GDP/Income	Control for GDP or Income	0.79	0.41	0.83
Poverty	Control for Poverty	0.47	0.50	0.56
Deterrence	Control for Deterrence	0.52	0.50	0.47
IV-Inequality	Inequality has been instrumented	0.13	0.33	0.19
Cross Section	Data are Cross Sectional	0.12	0.32	0.10
Single Country	Study with Data from one Country	0.91	0.28	0.98
Time FE	Time Dummies are included	0.46	0.50	0.57
Race	Control for Race	0.38	0.48	0.29
Female Head	Control for Female-headed households	0.31	0.46	0.20
Article Influence	Article Influence of Journal of Publication	0.98	0.80	182.25
Google Citations	Total Number of Google Scholar citations	142.15	274.96	1.39
USA	Data are from the USA	0.41	0.49	0.36
China	Data are from China	0.18	0.38	0.07
Mexico	Data are from Mexico	0.09	0.29	0.34
Years	Years Since Publication (2023 is 0)	8.55	5.24	7.77

weight to changes at the bottom of the income distribution, the second to changes in the middle, and the last to changes at the top. *Inequality Bottom Weight* includes the Generalised Entropy (GE) indexes with α equal to zero and one. α represents the sensitivity of the index to different parts of the income distribution, with lower values indicating more weight given to lower incomes. When α is zero, the GE index is known as the mean log deviation, and when it is one as the Theil index. When all individuals have the same wealth, the indexes are zero and they get bigger as inequality increases. Continuing, I include the Atkinson Index with ε equal to one and two (Atkinson, 1970). ε indicates the level of aversion in the social welfare function, with higher values – such as one and two – implying greater weight to transfer at the lower end of the distribution. Finally, I include in this category any type of decile dispersion ratio – such as the 90–10 ratio – and all the poverty measures. *Inequality Middle Weight* includes the Gini coefficient; the general entropy with α equal to two; Atkinson with ε equal to 0.5, and income polarisation.[12] Finally, *Inequality Top Weight* includes the income share held by the richest and the variance of incomes. I also code individual inequality measures. The Gini coefficient is the most used metric in the empirical literature (57.8%), followed by *Theil* (13.2%), and *Decile/Quintile Ratio* (11.4%). I also classify the inequality measures based on the use of income (69.6% of the whole sample) and consumption/expenditure (15.3% of the whole sample).

The role of omitted variables may be particularly relevant to the empirical studies of the impact of inequality on crime. According to the previous discussion, I code the regressions and studies, based on many additional characteristics which could help to explain the directions and magnitudes of the $\hat{\theta}$s. To capture the economic benefits and costs of crime, I created three binary variables which take value one if the model includes a particular regressor and value zero otherwise. These are *Unemployment*, *Poverty* and *GDP/Income*. I specifically distinguish between poverty and income, because (Pridemore, 2008) notes that measures of economic development do not measure poverty. *GDP/Income* is the most frequent control variable, employed in 79.2% of the regressions. Interestingly, only 46.6% of them include a measure of poverty, which is recognised as being strongly related to inequality. About 90.8% of the regressions contain at least one of these three variables.

I also create a binary variable that equals one if there is a measure of deterrence. Around 52.3% of all the studies include this, usually referring to the (public) police. About 60.8% of all the regressions that control for deterrence employ a measure of police spending. As mentioned earlier, deterrence may be related to both inequality and crime (Corvalan & Pazzona, 2022). I deal indirectly with the problem of omitted variables – but also reverse causality and measurement errors – by classifying regressions that use an instrumental variable approach for inequality, such as Buonanno and Vargas (2019) who employ the share of slaves in 19th century Colombia as an instrument.

To take into consideration that cross-sectional studies may report higher coefficients, I created three variables. The first is a binary variable which takes value one if data are cross-sectional and zero otherwise. The second variable is *Single Country* which takes value one if the study is based on a single country, and zero for multiple countries. Finally, I consider whether the time-fixed effects are included.

Continuing, I include a binary variable that is equal to one if the regression controls for race. I also categorise the variable *Female Head* which is equal to one if a measure related to female-headed households is included. The quality of the journal in which the study was published may also be relevant. I, therefore, control for the article-influence score, provided by *Eigenfator.org* (Bergstrom, 2007). To measure the success of the study within the academic community, I include the number

[12] Of course, income inequality and polarisation are different, although related, concepts (Esteban & Ray, 1994).

of citations, retrieved from *Google Scholar*.[13] Demombynes and Özler (2005), Fajnzylber et al. (2002) and Kelly (2000) are the most cited works in the relevant literature. I finally calculate the number of years since publication, *Years*. I have done this to determine whether there is any time trend in the literature regarding the effect sizes.

3.3. Standardisation of effect sizes

Given that I cannot readily compare the $\hat{\theta}$s – the estimated coefficients – among all the studies, I transformed them into partial correlation coefficients (PCC), which provide measures of association between variables, ceteris paribus (Stanley & Doucouliagos, 2012). The formula to calculate $PCC_{i,j}$ is $t-stat_{i,j}/\sqrt{t-stat^2_{i,j}+df_{i,j}}$, where $t-stat_{i,j}$ is the conventional t-test for the statistical significance of θ in Eq. (1). If the test statistic was not reported, I calculated it. The formula also includes *df*, the number of the degrees of freedom for that particular regression. As with any correlation, PCC is bounded between −1 and 1. The standard errors – $SE(PCC)_{i,j}$ – are calculated as $\sqrt{(1-PCC^2_{i,j})/df_{i,j}}$.. To avoid having a few outliers which affect the analysis, I winsorize the top, and bottom, 1% of the *PCC* and *SE(PCC)*, as frequently done in recent meta-analysis studies (Chletsos & Sintos, 2022).[14] The hbox – Fig. 2 – shows a high degree of heterogeneity of PCC within and between studies.

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, the Appendix reports the results with the Fisher's z units.[15] The use of partial correlation coefficients, and Fisher's z statistics, is widespread in meta-analyses (Cazachevici, Havranek, & Horvath, 2020; Havránek, 2015). Finally, Table 2 provides the unweighted mean and standard deviation of each characteristic, alongside the mean weighted by the inverse of the variance of the *PCC*, i.e., $1/V(PCC)$.

4. Methodology

In this section, I present the meta-analysis techniques that are employed to calculate the true effects, net of publication bias, and to evaluate the role of heterogeneity in the crime-inequality relationship.

4.1. Estimating the true effect, net of publication bias

A first approach to assess whether the presence of publication bias is affecting the estimation of the inequality-crime relationship is through a funnel graph. This consists in plotting the studies' effect size on the x-axis, partial correlation coefficients in my case, and a measure of standard errors on the vertical axis, in descending order. The effect sizes associated with the smallest standard errors are the most precise ones, and also those less likely to be susceptible to publication bias. This is because, with such high precision, it is almost guaranteed to find significant effects and researchers will report smaller coefficients. Less precise effect sizes are distributed at the bottom of the graph, and are more likely to be widely dispersed because less precision should lead to more variability/sampling errors in the estimates. In the absence of publication bias, the less precise estimates should be distributed symmetrically – as a funnel – around the most precise effect sizes. If there is publication bias – i.e. preference for effect sizes which are statistically significant and/or of a particular sign – the graph is asymmetrical, with more estimates concentrated on one side, and in areas which provide statistical significance. That is why publication bias is also referred to as a small study effect.

Nevertheless, I need to rely on the formal tests to assess the presence of publication bias, and provide an estimate of the average effect sizes of the inequality-crime relationship. The standard way to do so is through the funnel asymmetry and precision effect tests (FAT-PET) (Egger, Smith, Schneider, & Minder, 1997; Stanley & Doucouliagos, 2012), which estimates the (linear) relationship between the effect sizes and standard errors. Formally, the regression model is:

$$PCC_{i,j} = \lambda_0 + \lambda_1 SE(PCC)_{i,j} + \varepsilon_{i,j}4 \tag{2}$$

where, again, *i,j* stands for the *ith* estimates in the *jth* study. *PCC* is the partial correlation coefficient, or the estimated coefficients, $\hat{\theta}_{i,j}$, in Eq. (1). *SE(PCC)* is the standard error. $\varepsilon_{i,j}$ is the random error term with mean 0 and variance σ^2. In the absence of publication bias, the standard error is independent of its effect size and – accordingly – the coefficient λ_1 should not be statistically significant. If publication bias is present, this coefficient is statistically significant with the sign representing its direction, either positive or negative. λ_0 – the intercept – represents the true PCC of inequality on crime, once publication bias has been considered. Alternatively, we can think of it as the value of PCC as the standard error approaches 0, i.e., the infinite precision.

Several methods have been proposed to estimate Eq. (2), with the fixed (FE) and random effects (RE) models being the earliest. The former assumes that all estimated coefficients in the meta-analysis – $\hat{\theta}_{i,j}$ – come from the same population, θ, and that the differences between them are due mainly to sampling error, $\varepsilon_{i,j}$. Abstracting from the presence of publication bias, the estimated coefficient in a fixed effects model can be expressed as $\hat{\theta}_{i,j} = \theta_{i,j} + \varepsilon_{i,j}$. On the other hand, the random effects model assumes that, along with sampling error, single effect sizes vary because of the presence of between-study heterogeneity. In other words, there is not a unique true effect but a distribution of effects. The coefficients can diverge because regressions employ different measures of intensity of the treatment, or because they are based on different countries. Accordingly, the random effects model assumes that there is an extra source of error, $\eta_{i,j}$, which is generated from the effect μ. This model could be expressed as: $\hat{\theta}_{i,j} = \mu + \eta_{i,j} + \varepsilon_{i,j}$.

Despite such differences, the accepted practice is to estimate both fixed and random effects by weighting observations using the estimates' inverse of the variance, $1/V(PCC)$ (Stanley & Doucouliagos, 2012).[16] These are also the only weights applied in the fixed effects model. On the other hand, the random effects model adds an extra term representing the variance of the between estimates heterogeneity – $\eta_{i,j}$ – which is usually labelled as τ^2. Accordingly, the weights employed in the random effects models are $1/[V(PCC)+\tau^2]$. There are various estimators of τ^2, but the most popular are based on unrestricted or restricted maximum likelihood (Viechtbauer, 2005).

In recent years, many scholars have highlighted several flaws in both the fixed and random effects techniques. The drawback of the former is that it assumes that all estimates are coming from the same population. This assumption is rather unrealistic, especially in a field such as economics that relies on observational studies. Moreover, Stanley and Doucouliagos (2017) suggested that the confidence interval might have poor coverage. Random effects – it has been argued – might be biased if the between study variance, τ^2 is improperly estimated, which is likely to happen (Stanley, Doucouliagos, & Ioannidis, 2022). Several authors have noted how random effects is more biased than fixed effects in the presence of publication bias (Bom & Rachinger, 2019; Stanley, 2017). Considering the FE and RE's pitfalls, Stanley

[13] Both measures were collected on the 5th of July 2023.

[14] In the appendix, I will also show the results using different levels of winsorization and also with unwinsorized values.

[15] I would have preferred to employ elasticities. However, only 28.2% of all the estimates in my analysis measure both the crime and inequality in log form.

[16] Although the FAT and PET are conceptually the same tests, the former runs the model $T-stat_{i,j} = \beta_0 + \beta_1 1/SE(PCC)_{i,j} + \varepsilon_{i,j}$. As such the β_0 represents the publication bias and β_1 the true effect. Using the inverse of variance as weighs is also important to take into account the heteroscedasticity of PCC which increases with the size of standard errors.

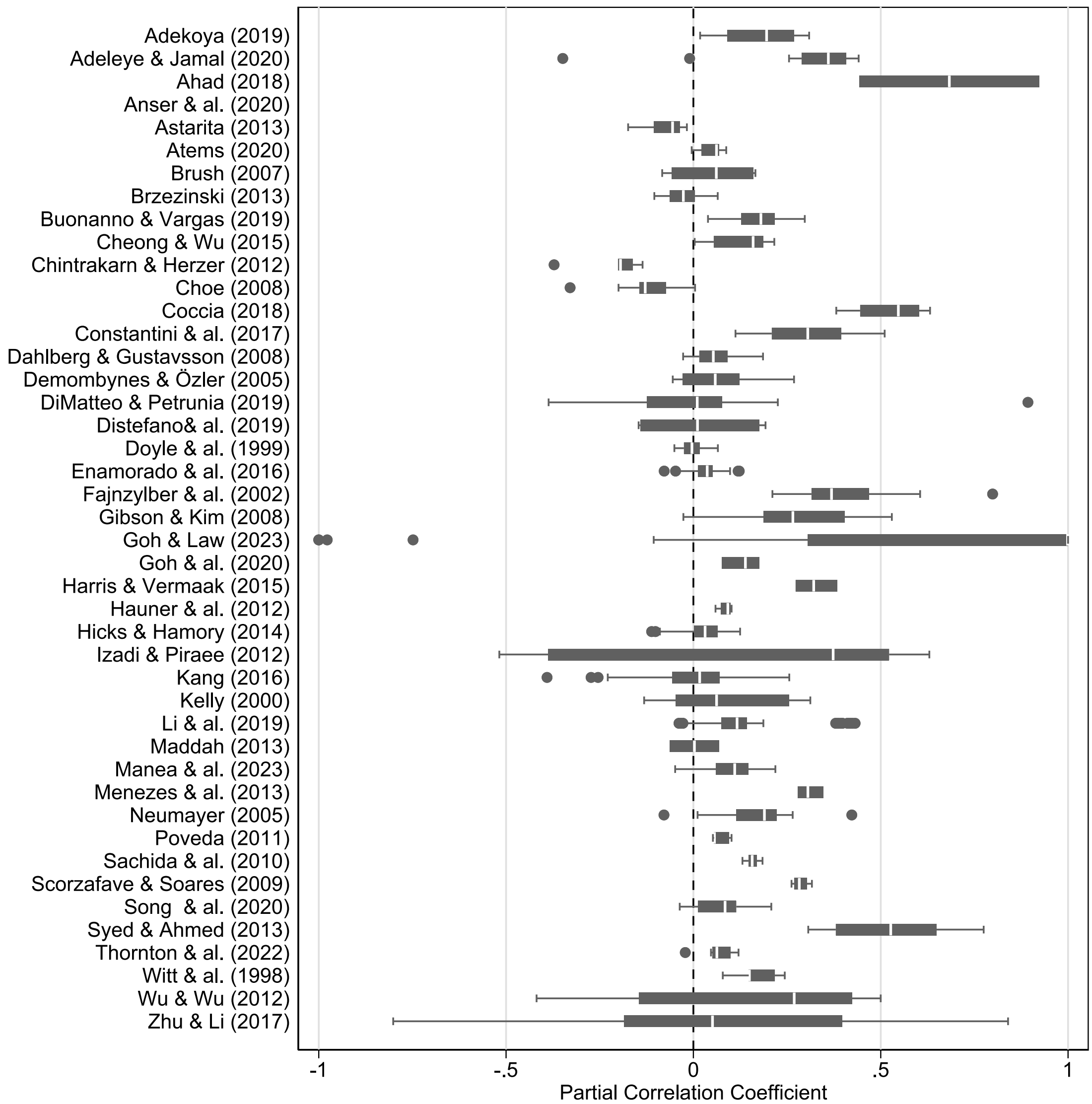

Fig. 2. Unweighted partial correlations per study. This figure depicts a box plot of the partial correlation coefficients (PCC) of inequality on crime reported in individual studies. The length of each box represents the interquartile range (P25-P75), and the dividing line inside the box is the median value. The individual point represents the highest and lowest data points within 1.5 times the range between the upper and lower quartiles.

and Doucouliagos (2017) and Stanley et al. (2022) suggested employing the unrestricted weighted least square (UWLS), which is also an inverse variance weighted method. The weights of the UWLS take the form $1/rV(PCC)$, which represents multiplicative heterogeneity, proportional (by the factor r) to the variance of each study. By allowing the heterogeneity variance to vary proportionally with the standard errors, the UWLS is more robust than the fixed and random effects. The former assumes no heterogeneity, while the latter an additive – and often biased – heterogeneity. It follows that UWLS and FE will produce the same point estimate but different standard errors. It is common practice in the meta-analysis literature to display the results using different techniques to avoid model dependency and provide robustness. Accordingly, I will report the results with all techniques, but the preferred one is the UWLS, given the reasons explained above.

Continuing, all three models, FE/RE/UWLS, assume independence of the effect size, i.e. lack of correlation. This condition is likely to be violated because effect sizes within a certain group (g) share characteristics that make them correlated. For example, estimated coefficients from the same study often share similar data, and research design. It could also be that coefficients using the same treatment effect are also correlated. The introduction of an extra level of heterogeneity – within group – is treated in a similar way to the random effects model presented earlier. Formally, we have that $\hat{\theta}_{i,j} = \mu + \eta_{i,j} + \kappa_j + \varepsilon_{i,j}$, where κ_j is the within group heterogeneity, in this case relative to study j.

In this paper, I adopt several econometric remedies to take into account this extra level of heterogeneity. Firstly, I employ cluster robust standard errors at the study level in all regressions. Secondly, I provide a multi-level (ML) hierarchical model, which consists in estimating the variance of the within cluster heterogeneity, κ_j. Similarly to the estimation of the between study heterogeneity for the random effects model, I need to estimate the within group variance and include it in the weights. Stanley and Doucouliagos (2017) and Stanley et al. (2022) show how the UWLS is also superior to the ML model, especially if the heterogeneity is correlated with the standard errors. As a third solution for the presence of heterogeneity, I weigh sizes by the inverse of the number of effect sizes per study, as to give more importance to studies with fewer estimates. Continuing, I also report a UWLS regression employing one observation per study, taking the averages PCC and SE(PCC). The drawback of doing so is that I lose information included in the universe of estimates. Moreover, as it has been shown by Bom and Rachinger (2020), using one observation per study does not solve completely the issue of dependency. Relatedly, heterogeneity might cause the standard errors to be correlated with residuals if the precision of the PCC is dependent on some characteristics of the papers.

To take this into account, I employ an instrumental variable approach, using the UWLS model. I instrument the standard error with the inverse of the degrees of freedom, as they are not generally correlated with the method choices (Cazachevici et al., 2020; Irsova, Bom, Havranek, & Rachinger, 2023).

The models deriving from Eq. (2) assume that the relationship between the estimated effects – the PCC in our case – and their standard errors is linear. Recently, some authors have questioned such an assumption, claiming that non linear – or more advanced models – are more appropriate to detect publication bias and retrieve the true effects (Stanley & Doucouliagos, 2014). An alternative to the FAT-PET is the precision-effect estimate with standard error (PEESE), which consists in estimating the model:

$$PCC_{i,j} = \lambda_0 + \lambda_1 SE(PCC)^2_{i,j} + \varepsilon_{i,j} \tag{3}$$

$SE(PCC)^2$ is used instead of $SE(PCC)$. The intuition is that publication bias – the relationship between PCC and SE(PCC) – might increase more than proportionally as the SE(PCC) gets bigger.

Continuing, several authors argued that the FAT-PET-PEESE tests present some drawbacks. For example, Stanley, Doucouliagos, and Ioannidis (2017) argued that these models could incorrectly estimate the true effect, even in the absence of publication bias. Accordingly, several advanced tests have been proposed. The first that I will employ has been proposed by Stanley, Jarrell, and Doucouliagos (2010) – labelled *Top 10* – which considers only the 10% most precise estimates. The idea is that these are less likely to be severely affected by publication bias. The second method is the weighted average of adequately powered estimates, *WAAP* (Ioannidis, Stanley, & Doucouliagos, 2017). This test considers only the effect sizes that have a statistical power above 80%, while providing no weights to those below this threshold. The included estimates are weighted by the inverse variance. Ioannidis et al. (2017) has shown that this method can severely reduce bias, compared to traditional ones. Another popular method is Endogenous Kink developed by Bom and Rachinger (2020), labelled *EK*. This is based on the intuition that for very small standard errors there is no relationship between the effect size and the standard errors. However, once the standard errors cross an endogenously determined threshold, the relationship does exist and it is calculated in a linear way as the FAT-PET model. A fourth advanced test is *AK*, developed by Andrews and Kasy (2019). This test starts from the recognition that the likelihood that an estimate is reported jumps at standard cut-offs for p-values (0.01, 0.05, 0.1). The authors build a selection model that estimates the probability that an insignificant effect size (or with the wrong sign) will be reported and then assigns more weight to intervals that are underrepresented. Continuing, the *Stem* method – developed by Furukawa (2019) – optimises the trade-off between bias and variance. Bias is low for the most precise estimates but discarding too many estimates leads to more variance. This method removes all estimates that do not contribute to this trade-off, and the true effect is calculated as the average value based on the remaining estimates. Finally, I will consider the p-uniform star technique, developed by van Aert and van Assen (2021), which is based on the principle that the distribution of p-values should be uniform at the true mean effect size.

4.2. Heterogeneity

The tests presented so far are useful to highlight the true effect of inequality on the crime net of publication bias. Still, these estimates represent averages among all effect sizes, irrespective of different studies' characteristics. A further exercise is to evaluate how the heterogeneity in the studies' design helps explain the sign and significance of the estimated coefficients.

One possible route to evaluate the determinants of the effect sizes' heterogeneity is to simply add a series of moderators to Eq. (2), which capture specific characteristics of the original regressions and studies. This consists in running the following model:

$$PCC_{i,j} = \lambda_0 + \lambda_1 SE(PCC)_{i,j} + \sum_{k=2}^{22} \lambda_k Moderators_{i,j} + \varepsilon_{i,j} \tag{4}$$

A drawback of such an approach is that it relies on a single selected model for inference. As clearly shown in Section 2, many competing empirical and theoretical approaches aim at explaining the impact of income inequality on crime. It is not clear, a priori, which variables should be included. By simply regressing Eq. (4), there is a risk of underestimating the role of model uncertainty.[17] A solution to infer the role of different moderators when there is no certainty about the best data-generating process is to average the estimates across some, or all, possible models (Leamer, 1978). Given the setting of this study, a model is a regression with the estimated effect (PCC) as the dependent variable and any combination of moderators on the right.[18] The idea of model averaging is to calculate the likelihood that each model represents the underlying data-generating process and use the resulting probabilities as weights to compute the average estimate for each coefficient. In my meta-analysis, there are potentially 2^{22} models, as 22 is the number of moderators.[19]

It has become common practice in the meta-analysis literature to apply model averaging using the probabilities obtained through the Bayes theorem (Cazachevici et al., 2020; Chletsos & Sintos, 2022), known as posterior model probabilities (PMP). This technique is called Bayesian model averaging (BMA). To fix ideas, let us define the model space $\mathcal{M} = \{M_1, M_2, M_3, ..M_{2^k}\}$, and M_j a representative model.[20] The PMP for model M_j conditional on the data, $p(M_j|PCC, Moderators)$, can be expressed by the following formula: $p(M_j|PCC, Moderators) =\propto p(M_j)$ $p(PCC|M_j, Moderators)$. That means that the posterior model probability is proportional to $p(M_j)$ times its marginal likelihood $p(PCC|M_j, Moderators)$. Accordingly, the computation of the PMPs entails making two choices related to the priors. One is for the model, and the other is for the regression coefficients.[21]

In the baseline specification, I use the uniform model prior which assumes an equal probability for each model (Eicher, Papageorgiou, & Raftery, 2011). Continuing, the choice over the priors on the model parameters is delicate as these enter directly into the calculation of the marginal likelihood, i.e. in $p(PCC|M_j, Moderators)$. It has been shown that it is more convenient to use a prior centred at zero, with the variance structure of the coefficient given by Zellner's g prior (Zellner, 1986). This means that I only need to define a scalar g that is then multiplied by the prior covariance to obtain the posterior covariance. Still, a $g-prior$ can be fixed or random. I decided to use the fixed *uip* prior which sets $g = n$, i.e. it assigns the same amount of information to the prior for regression coefficients as is contained in one observation (Kass & Wasserman, 1995).[22] The choice of priors – the uniform one for the model and the *uip* for the parameters – implies agnosticism about the relevance of the individual explanatory variables. Nevertheless, The results might be sensitive to the choice of the priors, so in the appendix, I use different ones.

Given the high number of explanatory variables, it is not feasible to run all possible models and it is common practice to use Markov chain Monte Carlo to identify the most likely of all, which are then

[17] As Steel (2020) pointed out, uncertainty "affects virtually all modelling in economics".

[18] Except the intercept which is included in all models.

[19] I include the SE(PCC) among the moderators.

[20] This discussion is based on Zeugner and Feldkircher (2009). For a detailed analysis on the use of BMA in economics, refer to Steel (2020).

[21] In a linear framework, it is typical to assume non-informative priors for the constant – always included – and the variance of residuals.

[22] As it is well understood, the g-parameter is directly related to the shrinkage of regression coefficients towards zero (Fernandez, Ley, & Steel, 2001). This corresponds to the shrinkage parameter $g/(1+g)$.

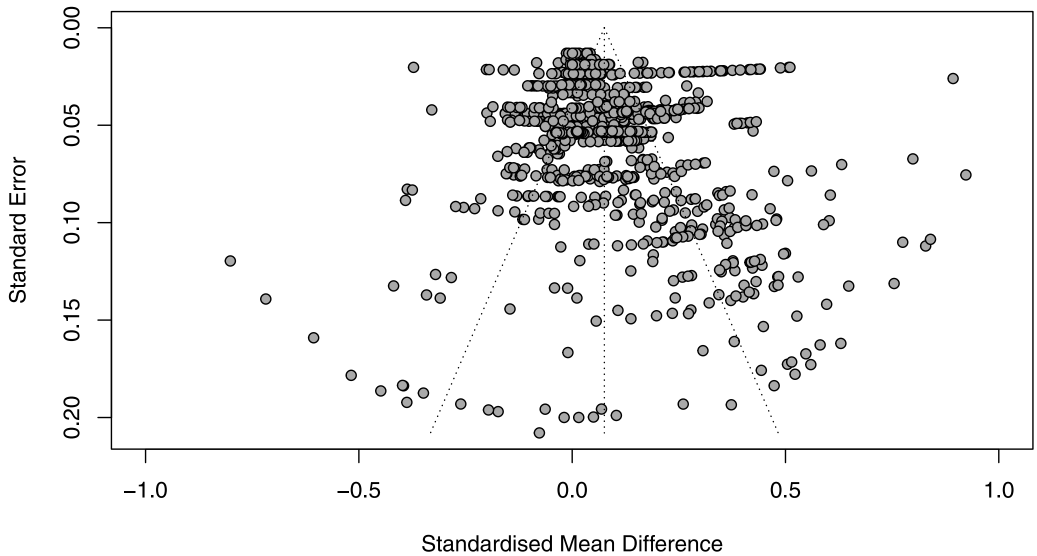

Fig. 3. Funnel plot. This figure is a funnel plot. The partial correlation coefficients are on the x-axis and the standard errors are on the y-axis. The graph includes all the 1341 estimates from the 43 studies. The line represents the unweighted average partial correlation estimates.

estimated.[23] I use the *Birth–death sampler* for the Markov chain Monte Carlo.

Once the PMPs have been assigned, they are used, as weights, to get the posterior means and standard deviations for each moderator. The PMPs are also employed to get the predictors' estimated posterior inclusion probabilities (PIPs), which are given by the sum of the posterior model probabilities of all the models where the moderator is included. The PIPs inform on how likely it is for each moderator to be included in the model. They are strictly related to the p-value in a frequentist setting.

5. Results

In this section, I present the main empirical findings,

5.1. Estimating the true effect, net of publication bias

First, I report the funnel graph for the effect of inequality on crime in Fig. 3. It is roughly funnel-shaped, although there is more concentration of points on the right side, suggesting some degrees of positive publication bias.[24] The most precise estimates are very close to zero, which anticipates that the true effect of inequality on crime is lower than previously thought. The dotted vertical line represents the average effect size. The diagonal line measures the precision of individual studies with a 95% confidence limit.

Table 3 reports the results of the methods presented in Section 4.1. Panel A presents the results of the linear models, all weighted by the inverse of the variance of the PCC, except the last one, *OLS*. In six out of eight models, I find evidence of publication bias. For example, in UWLS – the preferred model – the coefficient of the standard error is 0.568 and statistically significant at the 5% level. This indicates some moderate level of publication bias towards positive estimates. With other models – such as *RE* and *IV* – the coefficient is larger. Turning to the true effect, the λ_0, it ranges between −0.001 (*IV*) to 0.082, excluding the outlier *Mean*. The coefficients are significant in 4 out of the 7 models. Using *UWLS*, the coefficient is 0.044 and statistically significant.[25] Doucouliagos (2011) provided guidelines to analyse the magnitude of the effect. This author defines a strong effect for $|PCC| > 0.327$, a medium effect between 0.173 and 0.327, and a small effect for $|PCC| < 0.173$. There is no effect at all if $|PCC| < 0.070$. According to the results in Table 3, in 6 out of 8 models, there is no effect. Additionally, there is a medium effect in one of the least preferred specifications, *Means*.

I report the same battery of results for the PEESE model as for the FAT-PET, in Panel B of Table 3. The majority of models – 6 out of 8 – provide a positive and statistically significant coefficient for $SE(PCC)^2$. The coefficients are bigger than the ones reported in Panel A. This suggests that small studies might be the ones with over-estimated effects. The average PCCs – net of publication bias – are somewhat similar to the ones provided with the FAT-PET model. Six out of eight models produce a no-effect, according to Doucouliagos (2011)'s guidelines. Which results to choose between FAT-PET and PEESE? (Stanley & 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. Results in both Panel A and B of Table 3 indicate that this is most likely the case.

Panel C of Table 3 shows the results of the advanced test, presented in the same order as the exposition in Section 4.1. All the models, but *Stem*, retrieve positive and significant effects that range between 0.007 (*Stem*) to 0.123(*p-uniform**). Interestingly, the model developed by Bom and Rachinger (2020) also retrieves publication bias. In conclusion, my findings indicate that the actual values of the partial correlation coefficients, accounting for publication bias, are statistically significant but lack economic significance. Additionally, I observe limited evidence of positive publication bias, suggesting a preference for positive results, although its occurrence is limited.

5.2. Heterogeneity

The evidence presented so far points to a small, or inexistent, effect of inequality on crime. Still, the sign and size of the estimated effects might vary depending on several regressions and studies' characteristics. To take this into account, I employ Bayesian model averaging with the moderators presented in Section 3, which are based on the theoretical considerations developed in Section 2. I also include *SE(PCC)*,

[23] All BMA models are run using the R command, developed by Zeugner and Feldkircher (2015).

[24] This could be seen in the Appendix where I report the kernel distribution of PCC.

[25] Although not reported, the first stage of *IV* is highly statistically significant, with a Kleibergen–Paap rk Wald F statistic equals to 1117.197.

Table 3
Test to detect the true effect and publication bias.

Panel A: FAT-PET								
	UWLS	FE	RE	ML	Study	Mean	IV	OLS
True Effect	0.044***	0.044***	0.011	0.082*	0.027**	0.190*	−0.001	0.014
	(0.010)	(0.002)	(0.008)	(0.034)	(0.009)	(0.088)	(0.008)	(0.011)
Pub Bias	0.568**	0.568***	1.238***	0.477	0.533**	0.113	1.453***	1.188***
	(0.185)	(0.056)	(0.134)	(0.434)	(0.198)	(0.886)	(0.126)	(0.230)
N. obs.	1341	1341	1341	1341	1341	43	1341	1341
Panel B: PEESE								
	UWLS	FE	RE	ML	Study	Mean	IV	OLS
True Effect	0.054***	0.054***	0.048***	0.209***	0.042***	0.229***	0.046***	0.056***
	(0.006)	(0.001)	(0.004)	(0.030)	(0.004)	(0.052)	(0.005)	(0.006)
Pub Bias	6.504***	6.504***	7.717***	−1.039***	3.733**	−1.529	8.377***	5.852***
	(1.463)	(0.512)	(0.837)	(0.161)	(1.429)	(3.462)	(0.736)	(1.504)
N. obs.	1341	1341	1341	1341	1341	43	1341	1341
Panel C: Advanced								
	Top 10	WAAP	EK	AK	Stem	p-uniform*		
True Effect	0.099***	0.080***	0.044***	0.032***	0.007	0.133***		
	(0.013)	(0.010)	(0.007)	(0.011)	(0.073)	(0.010)		
Pub Bias			0.569***					
			(0.178)					
N. obs.	134	183	1341	1341	1341	1341		

Notes: This table shows the results of various tests aimed at detecting the true effect of inequality on crime, net of publication bias. In all models, the response variable is PCC, winsorized at the top, and bottom, 1%. Panel A reports the results of the FAT-PET tests using different estimation techniques. UWLS: unweighted weighted least squares; FE: Fixed Effects; RE: Random Effects; ML: Multi-Level Hierarchical; Study: UWLS using the number of estimates per study as weights; Mean: UWLS using only one estimate per study; IV: 2SLS using the inverse of the degrees of freedom as an instrument for SE(PCC). OLS is an unweighted model. Panel B reports the results using PEESE, i.e. controlling for $SE(PCC)^2$, instead of $SE(PCC)$. All models but OLS are weighted by the inverse of the variance of PCC. The true effect is the intercept – λ_0 in Eq. (2) – and λ_1 is Publication Bias. Panel C reports the results for the advanced techniques. *Top 10* is the model suggested by Stanley et al. (2010); *WAAP* by Ioannidis et al. (2017); *EK* is the Endogenous Kink model developed by Bom and Rachinger (2020); *AK* by Andrews and Kasy (2019); *Stem* by Furukawa (2019); and *p-uniform** by van Aert and van Assen (2021). Standard errors, clustered at the study level, are reported in parentheses. The total number of studies is 43.

* Significance at the 10% level.

** Significance at the 5% level.

*** Significance at the 1% level.

to check for publication bias. Moderators have been demeaned to provide a more meaningful interpretation of the constant term (also reported).[26] I report the visual representation of the BMA results in Fig. 4. Each column represents an individual regression model, ordered on the horizontal axis according to the posterior model probabilities. The vertical axis represents the explanatory variables listed in the descending order of their posterior inclusion probabilities. Blue colour indicates a positive coefficient and red is a negative one. A blank cell means that the corresponding explanatory variable (listed on the left) was not included in the model. Table 4 represents the numerical results of Bayesian model averaging. I report three columns: one with the variables' PIP, another with the posterior mean – the weighted coefficients – and finally the standard errors.

According to Raftery, Madigan, and Hoeting (1997) a PIP equal to 1, indicates a decisive variable; a PIP between 0.95 and 0.99 is a strong variable; substantial between 0.75 and 0.95; and weak between 0.5 and 0.75. If the PIP is below 0.5, the variables should not be included in the model. Following this classification, out of the 22 moderators, 18 pass the 0.5 threshold. There are decisive variables — *Unemployment*, *GDP/Income*, *Poverty*, *Cross Section*, *Single Country*, *Race*, *USA*, *China*, *Mexico*, *Years* and *Publication Bias*. Continuing, *Inequality Middle Weight* and *Crime Victimisation* are strong. Finally *Property Crime*, *Inequality Top Weight*, *Deterrence*, *Citations*, and *IV-Ineq* are substantial.

Starting from the crime variables, I note how the coefficient for *Property crime* is negative and relatively small. This result does not imply that *Violent Crime* has higher coefficients. Rather it is the third group – *Mixed Crime* – which has significantly higher estimates, ceteris paribus. As explained earlier, *Mixed Crime* includes both violent and property crime, which makes it difficult to interpret the results. To better understand this conundrum, in the Appendix I consider individual crime categories: four property crimes (*Auto Theft*, *Not Auto Theft*, *Burglary* and *Robbery*) and one for the main violent crime, *Homicide*. This exercise also does not reveal any statistically significant difference between violent and crime categories. Furthermore, I consider a model without the category *Mixed Crime*, to directly evaluate property versus violent crime. No difference with *Violent Crime* is retrieved. The lack of a positive and statistically significant impact on property crime categories implies that inequality does not primarily influence economically motivated criminal behaviour as predicted by the rational choice model.

Continuing, the variable *Crime Victimisation* has a positive and sizeable coefficient. As predicted by Gibson and Kim (2008), the use of victimisation surveys retrieves bigger estimates than recorded police data.[27] The coefficient for *IV-Ineq* points in the same direction, as the coefficient is also positive. Turning to the inequality variables, I find evidence that measures that are more sensitive to changes in income at the top and middle of the distribution have higher coefficients than the excluded category, *Inequality Bottom Weight*. This provides some evidence that crime incentives are the highest when criminal payoff increases, rather than when the opportunity cost decreases. In the Appendix, I consider three individual inequality measures – *Gini*, *Theil* and *Decile/Quintile Ratio* – which confirm such results. Continuing, whether the inequality measure is based on income or other measures seems not to matter to explain the size of coefficients. All

[26] All BMA and UWLS models in this work have been calculated using variables weighted by the inverse of the variance of the PCC. More specifically, the model run is $T-stat_{i,j} = \beta_0 + \beta_1 1/SE(PCC)_{i,j} + \sum_{k=2}^{22} \beta_k Moderators_{i,j}/\mathrm{SE(PCC)}_{i,j} + \varepsilon_{i,j}$.

[27] Also Stucky, Payton, and Ottensmann (2016) used surveys data and found high estimates.

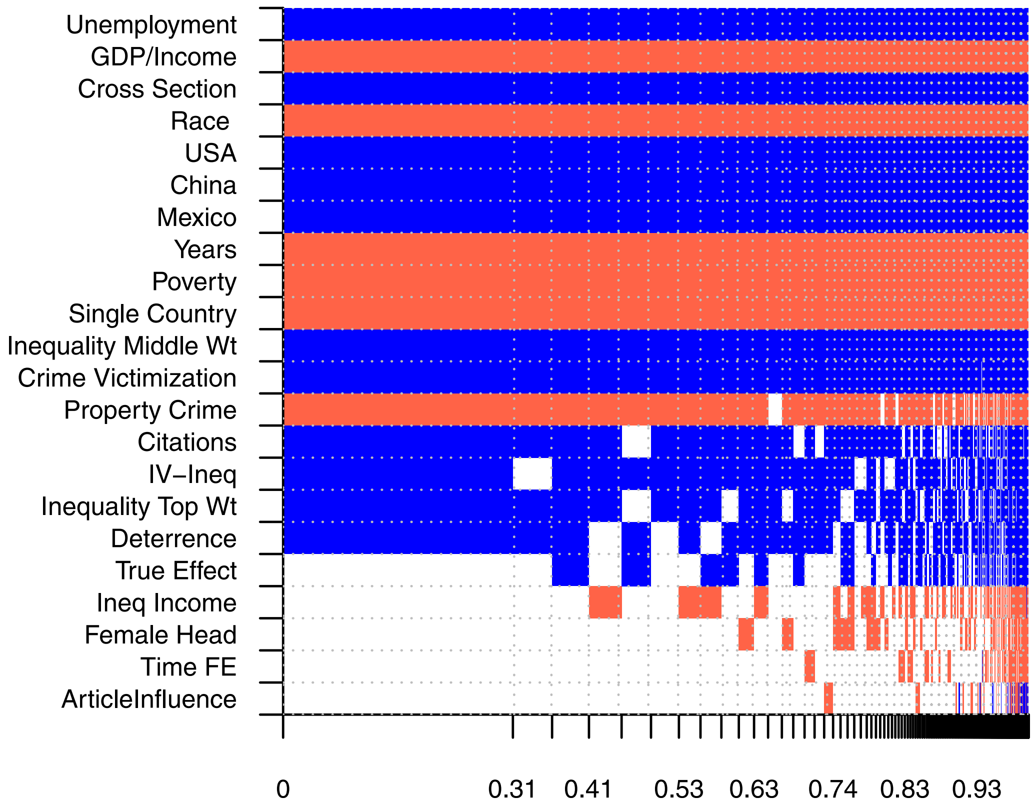

Fig. 4. Model inclusion in Bayesian model averaging. Notes: This figure visualises the result of the Bayesian model averaging (BMA). The vertical axis reports all the regressors ordered from the highest to the lowest posterior inclusion probabilities (PIP). A description of the included regressors is specified in the text and Table 2. Each column reports a single regression ordered – from the left to the right – according to their posterior model probabilities (PMP). The horizontal axis label reports the cumulative PMP. A blue-coloured cell represents a variable with a positive effect; a red-coloured one a negative effect; a blank cell means that the variable was not included in the model. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

the variables that capture the economic costs and benefits of crime have a PIP above 0.5. *Unemployment* has a negative sign, whereas *Income/GDP* and *Poverty* positive. The signs of the latter two indicate that inequality loses importance when these variables are controlled for. This confirms the intuition by Demombynes and Özler (2005) and several studies in criminology and sociology (Pare & Felson, 2014; Pridemore, 2011). However, I expected the sign of *Unemployment* to be also negative but I find that inequality is greater when this variable is included. To my knowledge, there are no studies that could help explain such a counter-intuitive result. *Deterrence* is associated with greater coefficients. Continuing, I do find support for the claim made by Brush (2007) regarding the bigger effects of inequality on crime in cross-sectional studies.

Relatedly, I do find that *Single Country* enters with a negative sign, as it is more likely to be employed in panel data studies. Regressions that include controls for *Race* are associated with lower PCC, because of the negative omitted bias created by its exclusion. This result is in line with the findings retrieved by Hipp (2007).[28] The number of citations has a very high PIP and a positive sign, although it is not clear the direction of causality. I do not find that article influence matters to explain the coefficient. Having greater coefficients does not open the door to better quality journals. Finally, regression using data from the USA, China and Mexico are all related with greater coefficients. These are countries where inequality is very high, so it is likely to be more salient. The negative coefficient of *Years* is likely to depend on the recent availability of microdata which leads to more precise – although smaller – coefficients. Kim et al. (2020) found similar results in his meta-analysis. The BMA results also reveal that, once we control for all the studies' characteristics, there is still some presence of publication bias. Finally, in the right columns of Table 4, I report the results with UWLS including only the variables with the PIP above 0.5. These results are similar to the ones just described for BMA.

6. Discussion and conclusion

Rational-choice models predict that higher income inequality should lead to more crime, because it increases the pay-off and/or reduces the opportunity costs of crime (Chiu & Madden, 1998; Ehrlich, 1973; Merlo, 2004). Although this argument is compelling, the existing empirical literature finds only ambiguous effects. To better understand this old puzzle, I conduct a meta-analysis, which is intended to quantitatively review the existing literature.

To detect and correct any publication bias in meta-analysis, I use various linear and non-linear techniques, including advanced ones recently developed in the literature. Such analysis reveals two main results. Firstly, the true effect of inequality on crime is statistically significant but economically insignificant. Using *UWLS* FAT-PET, and the advanced methods, points to a true effect which is comprised between 0.007 and 0.123. It is safe to say that, if inequality affects crime, its effect is – at best – small. Secondly, there is some limited evidence of positive publication bias. How do my results compare to the other meta-analyses that evaluated the effect of inequality on crime? In general, these studies found bigger coefficients than mine. Kim et al. (2020) found a weighted coefficient of 0.436, using a random effects method and 44 studies. Nivette (2011) estimated the mean effects sizes for homicides using a sample of 54 studies and 316 effect sizes. The author separated inequality in ratio and indexes, mainly the Gini coefficient. The estimated correlation coefficient was 0.416 for the former and

[28] This research studies the change in the coefficients of race variables with and without inequality.

Table 4
Determinants of the estimated effects: BMA and UWLS results.

	BMA			UWLS		
	PIP	Post.Mean	Post.SD	Coef	SE	p-val
True Effect	0.307	0.026	0.044			
Publication Bias	1.000	0.987		1.598	0.065	0.000
Property Crime	0.936	−0.020	0.008	−0.022	0.008	0.007
Crime Victimisation	0.996	0.201	0.047	0.207	0.043	0.000
Inequality Middle Wt	0.999	0.047	0.012	0.050	0.010	0.000
Inequality Top Wt	0.842	0.036	0.020	0.042	0.016	0.008
Ineq Income	0.249	−0.006	0.011			
Unemployment	1.000	0.170	0.014	0.168	0.023	0.000
GDP/Income	1.000	−0.063	0.011	−0.061	0.015	0.000
Poverty	1.000	−0.049	0.010	−0.051	0.018	0.004
Deterrence	0.807	0.023	0.014	0.028	0.010	0.004
IV-Ineq	0.863	0.021	0.011	0.024	0.006	0.000
Cross Section	1.000	0.206	0.016	0.209	0.020	0.000
Single Country	1.000	−0.098	0.031	−0.087	0.029	0.003
Time FE	0.070	−0.001	0.005			
Race	1.000	−0.110	0.014	−0.113	0.019	0.000
Female Head	0.137	−0.004	0.011			
Citations	0.875	0.000	0.000	0.000	0.000	0.033
Article Influence	0.046	0.000	0.003			
USA	1.000	0.126	0.013	0.127	0.020	0.000
China	1.000	0.143	0.017	0.141	0.022	0.000
Mexico	1.000	0.173	0.022	0.172	0.034	0.000
Years	1.000	−0.009	0.001	−0.010	0.001	0.000
Observations	1341			1341		

Notes: The table reports the results referring to Section 5. The left hand part shows the results using Bayesian model averaging (BMA). PIP stands for posterior inclusion probability. Post.Mean is the weighted coefficient, and SD the posterior standard deviation. The model uses the "unit information" g-prior (UIP) and a uniform model prior recommended by Eicher et al. (2011). The right-hand part of the table presents the results with UWLS using variables with at least 50% PIP in the BMA exercise. Moderators have been demeaned. The estimated model is $T\text{-}stat_{i,j} = \beta_0 + \beta_1 1/SE(PCC)_{i,j} + \sum_{k=2}^{22} \beta_k Moderators_{i,j}/\mathrm{SE(PCC)}_{i,j} + \varepsilon_{i,j}$. Accordingly, the intercept represents the publication bias. For more details on the methodology and variables employed, see Table 2 and Section 4

0.224 for the latter. Pratt and Cullen (2005) found inequality to be moderately correlated with crime, with a coefficient of 0.212, higher than my upper bound for PCC.[29] Finally, Kim et al. (2020) does not find evidence of publication bias, whereas I do find a limited presence. Other studies did not systematically investigate the role of publication bias.

The heterogeneity analysis – based on Bayesian model averaging – reveals that inequality does not exclusively affect property crime, but also affects violent crime. I conclude that the rational choice model – focused on property crime – does not fully capture the incentives to commit other types of crime. Such results should not be surprising, especially for scholars in other disciplines: Kim et al. (2020) found that the effect sizes are greater for violent crime rather than property ones. Similar results have been found by Stucky et al. (2016). Secondly, my analysis shows that inequality measures that are sensitive to changes at the middle and top of the income distribution exhibit higher coefficients. My results are somehow different from the one retrieved by Nivette (2011), who found that measures based on ratios had higher coefficients than indices, mainly the Gini coefficient. Moreover, this analysis also reveals the impact of excluding important variables – such as economic ones and deterrence – from the regressions. The latter result is in line with the theoretical arguments provided by Corvalan and Pazzona (2022). Finally, I show how measurement errors and data structure affect the estimates. For example, I find greater coefficients for cross-sectional studies, similar to a previous meta-analysis conducted by Kim et al. (2020).

[29] Pratt and Cullen (2005) ranked inequality as the determinant number 12 (out of 31) in terms of importance.

There are additional implications from my results. First of all, the small values of PCC I retrieved should lead to reflection on the true role of inequality on crime. It may be the case that inequality does not provide the greatest incentive to commit crime, and that other factors – such as poverty, GDP or unemployment – may be more relevant. Secondly, if inequality does indeed affect crime, it may do so in different ways than those discussed by the majority of the existing empirical literature. For example, future research might examine non-linear effects or show how inequality interacts with other economic measures which capture the costs and benefits of crime.

CRediT authorship contribution statement

Matteo Pazzona: Conceptualization, Methodology, Software, Validation, Formal analysis, Investigation, Resources, Data curation, Writing – original draft, Writing – review & editing, Visualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.worlddev.2023.106520.

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