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Abstract[1]

Estimating the effect of inequality on crime is challenging due to reverse causality and omitted variable bias. This paper addresses these concerns by exploiting the fact that, as suggested by recent scholarly research, the legacy of slavery is largely manifested in persistent levels of economic inequality. Municipality-level economic inequality in Colombia is instrumented with a census-based measure of the proportion of slaves before the abolition of slavery in the nineteenth century. It is found that inequality increases both property crime and violent crime. The estimates are robust to including traditional determinants of crime (like population density, proportion of young males, average education level, quality of law enforcement institutions, and overall economic activity), as well as geographic characteristics that may be correlated with both the slave economy and with crime, and current ethnic differences. Policies aiming at reducing structural crime should focus on reducing economic inequality.

Keywords: Slavery, Inequality, Crime, Instrumental variables, Colombia
JEL: C26, D63, I24, K14

[1] The authors completed most of this paper while they both were Visiting Scholars at the Inter-American Development Bank. We thank Jorge Agüero for his useful discussion as well as seminar participants at the IDB and Universidad del Rosario. Buonanno: Università degli Studi di Bergamo, Department of Economics. Via dei Caniana 2 - 24127, Bergamo. Email: paolo.buonanno@unibg.it. Vargas: Universidad del Rosario, Department of Economics. Calle 12c No. 4-69, of. 315, Bogota. E-mail: juan.vargas@urosario.edu.co.

1. Introduction

Since the seminal contribution of Becker (1968), the relationship between economic inequality and crime has attracted the attention of social scientists, especially economists and criminologists. Becker argued that agents would engage in crime or illegal behavior as long the expected benefits offset the expected costs. Indeed, for a given probability of apprehension and expected punishment, higher levels of economic inequality would increase the expected benefit of committing a crime for the relatively disadvantaged. A reasoning of this fashion seems to be in the mind of many, as economic inequality is often considered in the public debate as a fundamental determinant of crime.

The idea that economic inequality causes crime finds support in several theoretical accounts besides Becker's paper (see, for example, Ehrlich, 1973; Chiu and Madden, 1998; Imrohoroglu, Merlo and Rupert, 2000). But a positive relationship between inequality and crime is not the only existing theoretical prediction: as long as inequality increases *the threat of* crime, it may be negatively related with actual crime rates. This may occur if private protection strategies are implemented to offset crime by the wealthy (e.g., alarm systems, private security, etc.). Allen (1996) and Chiu and Madden (1998) suggest this mechanism, and Chintrakarn and Herzer (2012) find a robust crime-reducing effect of inequality in a panel of U.S. states.

The empirical evidence is also inconclusive. While some have found support for the idea of a positive relationship between inequality and crime (e.g. Ehrlich, 1973; Kelly, 2000; Machin and Meghir, 2000; Gould, Weinberg and Mustard, 2002; Fajnzylber, Lederman and Loayza, 2002; Demombynes and Ozler, 2005), others have failed to find any significant relationship (Allen, 1996; Bourguignon, Nuez and Sánchez, 2003; Neumayer, 2005).

One limitation of most of the existing empirical literature is the lack of an empirical strategy to overcome the usual identification challenges.[2] In addition, the existing contributions are by and large based on cross-country evidence or other large levels of aggregation, thus the issue of data and institutional comparability is also likely to play an important role in the study of the relationship between inequality and crime. For instance, the lack of a large panel of homogeneous data on economic inequality may constitute a source of attenuation bias that is likely to explain, at least partially, why several studies have failed to find a significant

[2] See Bourguignon (1999), Eide (1999) and Freeman (1999) for thorough reviews on the relationship between inequality and crime.

relationship. The use of data at a smaller level of geographical aggregation is highly desirable, as it allows a more adequate measurement both of economic inequality and crime.

In this paper we investigate the effect of economic inequality on crime focusing on sub-national variation across Colombian municipalities. Besides the fact that this approach helps us mitigate some of the concerns regarding the institutional heterogeneity and data comparability present in cross-country set-ups, we argue that Colombia is a good laboratory to study this relationship for several reasons. First, it is one of the most unequal and most violent countries in the world. According to the 2013 World Development Indicators,[3] Colombia ranks 143 among 154 countries in terms of economic inequality.[4] Likewise, according to the 2013 UNODC Homicide Statistics,[5] Colombia ranks 180 out of 190 countries in terms of the homicide rate.[6] Second, high-quality data on various types of crime are available in Colombia for academic research. The Criminal Research Division (DIJIN) of the Colombian National Police maintains monthly-level municipal-specific datasets on the incidence of various types of crimes. In particular, Colombian crime statistics allow us to overcome under-reporting issues that typically affect crime statistics. Third, Colombia's history of slavery, combined with the availability of pre-abolition population censuses, allows us to construct a municipal-level instrument of current economic inequality, thereby overcoming identification concerns.

Indeed, as mentioned, the identification of the causal effect of inequality on crime is challenging due to the existence of reverse causality and omitted variable bias. However, recent research has established that the legacy of slavery is manifested in persistent levels of economic inequality. Engerman and Sokoloff (1997 and 2000) argue that ecological and climate characteristics present at the earlier colonial period determined the economic activity adopted by the colonizers. There, where the soil and the geography were ideal for establishing large-scale, labor-intensive plantations, the use of slave labor became more prevalent. In turn, the establishment of this institution determined large initial levels of land concentration and economic inequality. Finally, initial inequality shaped the choice of certain types of institutions

[3] Available online at: http://wdi.worldbank.org/tables. Last accessed 12/09/2013.
[4] The 11 countries that are more unequal than Colombia are, from the most to the least unequal: Seychelles, Comoros, Namibia, South Africa, Botswana, Micronesia, Haiti, Honduras, Zambia, Bolivia and Central African Republic.
[5] Available at: http://www.unodc.org/unodc/en/data-and-analysis/homicide.html, last accessed 12/09/2013.
[6] The 10 countries that have higher homicide rates than Colombia are, from the most violent to the least violent: Malawi, South Africa, Trinidad and Tobago, Saint Kitts and Nevis, Guatemala, El Salvador, Venezuela, Cote d'Ivoire, Jamaica and Honduras.

(what Acemoglu and Robinson, 2012, would call *extractive* institutions) that generated further inequalities and thus made them persist over time. One implication of this argument, which has found support in the work of Lagerlof (2005), Easterly (2007), Nunn (2008) and Soares, Assunção and Goulart (2012), both cross-country and within the United States, is that the use of slaves in colonial times has explanatory power in regard to current economic inequality.

Colombia is not an exception: Acemoglu, García-Jimeno and Robinson (2012) show that places with a higher concentration of slaves in colonial times, due to the establishment of gold mines exploited by the conquistadors, are more unequal and less developed today.

Building on this political economy literature, we exploit the heterogeneous slave legacy across Colombian municipalities to construct an instrument of current economic inequality. In particular, using a historical population census, we compute the proportion of slaves to the total municipal population in 1851, right before the abolition of slavery that took place with the 1853 Constitution.

We find that inequality increases both property and violent crime (e.g., car theft and murder). Our estimates are robust to including traditional correlates of crime like population density, the proportion of young males, the average education level, the quality of law enforcement and the overall economic activity. This helps mitigating potential threats to our identification strategy that stem from the fact that past slavery may have had an independent effect on other social outcomes different from inequality (as shown by Acemoglu, García-Jimeno and Robinson, 2012). Importantly, we also show that our results are not driven by fixed geographic characteristics that may be correlated both with the slave economy in the nineteenth century and with current crime, or by current ethnic differences across municipalities. Moreover, our results are robust to the use of different measures of economic inequality, such as the Gini coefficient, the Theil index and the Atkinson index. Last but not least, our results are robust to relaxing the assumption that the exclusion restriction hold unviolated. More specifically, following (Conley, Hansen and Rossi, 2012) and (Berkowitz, Caner and Fang, 2012), we conduct some sensitivity analysis to test the extent to which our estimates survive after allowing for plausible amounts of imperfect exogeneity of the slavery instrument.

That we find an effect of economic inequality on property crime is consistent with the framework of Becker (1968) as criminals attempt to obtain material benefits from their actions. However, it is less clear why should there be an effect of inequality on violent crime. While it

may be the case that a fraction of violent crimes starts as nonviolent attempts to illegally obtain someone else's property, this fraction seems rather marginal. Instead, the result on violent crime is consistent with recent epidemiological research. In particular, Wilkinson and Pickett (2009) provide evidence that inequality has a pernicious effect on society by eroding trust and social capital, and by creating a range of pathologies ranging from anxiety and drug abuse to mental and physical illness. This is the mechanism that according to the authors links inequality with social ailments like the formation of gangs and the resort to violent crime.

Our paper is not the first to study the relationship between inequality and crime in Colombia. Bourguignon, Nuez and Sánchez (2003) propose a structural model that suggests that different parts of the income distribution are more likely to affect property crime and test it using a panel of the seven largest Colombian cities. In a similar vein, but studying the relationship between inequality and violent *conflict*, Fergusson and Vargas (2011) develop a model that distinguishes between the overall inequality and the inequality within poor and rich segments of the population. They argue that the relationship between inequality and conflict depends on the type of inequality analyzed. Using a cross-section of municipalities, they find support for this idea. Neither of these papers, however, deals with identification issues.

The remainder of the paper is organized as follows. Section 2 describes the data and their sources. Section 3 presents our identification strategy. Section 4 describes the main results and robustness checks, and finally Section 5 concludes.

2. Data

2.1 Main Variables

We estimate the effect of economic inequality on crime using the proportion of slaves in the nineteenth century as instrument of current inequality. Thus, the main variables in our analysis are the historic proportion of slaves, and the contemporaneous economic inequality and crime rates, all at the municipal level.

The proportion of slaves comes from the 1851 Census of Colombia (then called New Granada), which took place just two years before the abolition of slavery with the 1853 Constitution. Despite all the potential challenges of carrying out a comprehensive population census in a relatively backward country like New Granada some 150 years ago, the 1851 census is believed to have been the most reliable demographic account of the century. Indeed, while

historians like Gómez (1969) mention as one of the hurdles of the 1851 census the fact that some people tried to hide from census officials, fearing that it was a way to extend that tax base, they also conclude that, compared to other censuses carried out in the nineteenth century in Colombia, the 1851 census was the one with the fewest problems (Gómez, 1969).

The census includes most municipalities (then called "parochial districts") existing at the time in Colombia. Each municipality mayor, together with the governor of the municipality's state (then called "canton") were in charge of delimiting the borders and choosing two local "commissioners" to administer the census within that area. The commissioners had to record each household's information by hand writing in strict order the names and ages head of the household, his wife, the offspring, dependents and servants and finally the slaves (both single and married).[7] For each municipality we record the number of counted slaves as well as the total municipal population, allowing us to construct slave shares at the most disaggregated administrative level. Critically, before the abolition the settlement of slaves was determined by exogenous factors, like the presence of mineral deposits (Acemoglu, García-Jimeno and Robinson, 2012).

The administrative division of the New Granada territory differs from the contemporaneous administrative division of Colombia. Today there are over 1,100 municipalities, which is about double the number of parochial districts in the 1850s. Current municipalities were either founded from settlements on frontier lands that did not belong to any administrative unit, or split from pre-existing municipalities. Using data on the date of foundation of each municipality as well as its origin (whether a vacant land or one or more pre-existing municipalities) we aggregate the current administrative division to that existing in 1851, when the census was administered. We end up with 541 parochial districts, which constitute our estimation sample.

In the absence of municipal-level measures of income-inequality in Colombia, we use the Gini coefficient of the current distribution of the value of landholdings as our proxy of economic inequality. These data, that were previously used by Offstein (2005) and Fergusson and Vargas (2011), come from the 2002 cadastral update of IGAC, the Colombian government institution in

[7] Presidential Decree of July 18 of 1842.
Accessed from: http://www.banrepcultural.org/blaavirtual/economia/estadcol/estadcol38.htm

charge of maintaining cadastral records at the municipal level.[8] IGAC, however, does not keep the records of large cities like Bogota and does not keep the records of the whole state of Antioquia, which maintains its own (confidential) cadastre. Thus municipalities from Antioquia are not included in our main sample.[9] Using the individual label data on land tenancy and value we computed current municipal-level Gini coefficients of the value of land for the municipalities existing in the geo-administrative division of the New Granada in 1851. For robustness purposes we also compute other inequality measures, namely the Theil index and the Atkinson index.

The data on crime comes from the Colombian National Police. We take the average of the monthly incidence of various types of crime in each municipality for the period 2005-2007. However, for reasons of data quality we focus on two specific crimes: the murder rate and the car theft rate. The first is a proxy of violent crime and the second a proxy of property crime. We focus on these measures because other categories of crime (e.g., mugging and larceny) have severe underreporting problems.[10] This is the case both in Colombia and internationally: people do not report minor crimes because the opportunity cost of doing so is high given the low probability of detection and the small losses involved. They also are unlikely to report crimes that make them feel ashamed, like rape. In turn, while in cases of murder there is a body that makes the crime difficult to hide, in cases of vehicle theft people do not have incentives not to report it, even in cases in which the vehicle is uninsured, because the loss is very valuable and so the expected gain of reporting the crime is relatively high, even if the probability of detection is low. In the specific case of Colombia, General Naranjo, the chief of the National Police from 2007 to 2012, confirms that, for this country "the public figures of murder and vehicle theft are by far the closest to the real phenomena, much more so than those of mugging and larceny."[11]

Table 1 shows descriptive statistics for these variables. Panel A focuses on the three outcomes described, which are normalized by the municipal population. Panels B and C describe respectively our independent variable of interest and the instrument. The mean value of the land

[8] More recent updates of these data are not publicly available.

[9] However, because Antioquia does have public data on crime as well as data on the proportion of slaves in the nineteenth century, we can estimate the reduced-form specifications including this state. In this case the results are unchanged in terms of magnitude and significance compared to the reduced form estimated over the baseline sample.

[10] In addition to murder and vehicle theft, we construct a crime index of both property and violent crimes using principal components analysis.

[11] General Oscar Naranjo in his address on street crime, Center for Latin American Studies, School of Foreign Service, Georgetown University, 2006.
Available at: http://pdba.georgetown.edu/Security/citizensecurity/Colombia/evaluaciones/delitocajellero.ppt.

Gini coefficient in Colombia is rather high: 0.68, with the least unequal municipality having a Gini of 0.21 and the most unequal having one of 0.91. The alternative inequality measures show a similar picture. Indeed, as mentioned in the introduction, Colombia is one of the most unequal countries in the world. In turn, the proportion of slaves in 1851 parochial districts is not too high: on average municipalities had 3 percent slaves, with some having no slaves and one having as much as 11 percent slaves.

2.2 Controls

Panel D of Table 1 describes the controls included in our estimations. We focus on variables that have been identified in the literature as robust determinants of the incidence of crime. These include demographic variables like population density (crime is more likely to occur in more densely populated, urban areas), and the proportion of young males in the age bracket 15-29 (young males in that age range are the subset of the population identified as most prone to commit crimes). We also include the average years of education, a measure of the efficiency of the judicial system at the local level and, in the absence of municipal-level GDP data for Colombia, per capita tax revenues as a measure of economic activity. We also look at the robustness of our results to controlling for the current proportion of blacks and geographical variables that are correlated with the slave economy. We describe these controls in detail below.

The demographic variables and the average level of education are census-based variables that we computed from the 2005 Census. To compute the density, we use data from DANE (the Colombian statistics office) on the area of each municipality. The mean density in our sample is 0.14 people per squared Km. The average proportion of young males aged 15 to 29 is 25 percent of the municipal population. However, the variation of this variable is somewhat large, from 17 percent to 52 percent. The average education is almost 5 years of schooling, with municipalities with as low as 3 average years of schooling. The proportion of blacks is based on self-reported ethnicity in the 2005 Census. We call "blacks" the proportion of people that identify themselves as either "Afro-Colombian" or "Palenqueros."[12]

[12] Palenqueros is the term that identifies the inhabitants of Palenque, a municipality in the state of Bolivar to which fugitive slaves would flee to before the emancipation.

As for our proxy of judicial efficiency, we use the index computed by Fergusson, Vargas and Vela (2013). The index uses all homicide cases that entered the criminal justice system from 2008 to 2010, from the National Office of the Attorney.[13] The index is computed as follows:

$$Efficiency\ Index_m = \frac{Cases\ Closed_m}{Total\ Cases_m} \times \frac{Total\ Cases\ Resolved_m}{Cases\ Closed_m}$$
$$= \frac{Total\ Cases\ Resolved_m}{Total\ Cases_m}$$

Hence, our measure can be thought of as the efficiency of judges, adjusted for quality. Indeed, the first ratio in the first line of the expression measures the share of cases entering the judicial system that are resolved (efficiency). However, cases are often closed without resolution, meaning that either no one is found guilty, or terms expire and the judge is forced to close the case with no definite action. Thus, we adjust by the second ratio (quality): the share of closed cases that were actually resolved. Low values of the first ratio indicate congestion of cases and slow decision-making, and low values of the second translate to high levels of impunity.

The data on tax revenues come from the fiscal database of the National Planning Department, and the geographic variables (the average municipal elevation and the average soil quality) come from the database of the CEDE, Universidad de Los Andes.

3. Empirical Strategy

Estimating the effect of inequality on crime using a simple linear regression is likely to produce biased estimates for several reasons, including reverse causality, omitted variable bias and measurement error. The incidence of crime (especially property crime) may be redistributive to the extent that perpetrators are economically disadvantaged with respect to victims. If that was the case then, assuming that the true value of β_1 in the model below is positive, the potential for reverse causality would be less worrisome for our purposes as the estimated coefficient would go against our finding any relationship between the two variables. Omitted variable bias is a

[13] Colombia started implementing a new criminal justice system in 2005, before which municipal judges were not forced to report systematically on the investigations carried out by them. Hence data on criminal judicial outcomes is sparse and unreliable for the years when the old system was in place. Because the adoption of the new system was staged in several years across different geographic regions, there are consistent data for all the municipalities starting from 2008.

concern to the extent that economic inequality and criminal behavior are jointly determined by unobserved factors. One such factor could be an ill-functioning broad institutional apparatus that, on the one hand, promotes the accumulation of wealth in the hands of a small elite and, on the other, lacks the law enforcement capacity to prevent and punish crime. This seems to be the case in many developing countries that are both highly unequal and prone to crime and violence, as recently noted by Acemoglu and Robinson (2012). Finally, the scope for measurement error is not negligible in our context, as our proxy of economic inequality is based on the cadastre-recorded value of land property, which may suffer from severe under-reporting.

For these reasons, in order to study the effect of economic inequality on crime we estimate the following regression model by 2 SLS:

$$crime_m = \alpha + \beta_1 \widehat{EI_m} + \delta' X_m + \epsilon_m \qquad (1)$$

where, EI_m are the predicted values of the economic inequality that are computed from the estimation:

$$EI_m = \phi_1 Slavery_m + \lambda' X_m + u_m \qquad (2)$$

We also report the results from the reduced-form equation:

$$crime_m = a + b_1 Slavery_m + p' X_m + e_m \quad (3)$$

which, in addition to testing the direct effect of past slavery on current crime, allows us to include the state of Antioquia, for which there is no publicly available micro-level cadastral data to compute our proxy of economic inequality.

The outcome $crime_m$ measures the incidence of crime at municipality *m*. We look at variables associated with both property (vehicle theft) and violent crime (murder), as well as at a principal components index of both types of crime. *EI* is our proxy of economic inequality, namely the municipal-level Gini coefficient of the distribution of the value of landholdings. $Slavery_m$ is the proportion of slaves to the total municipal population according to the 1851 Census, carried out just two years before slavery was abolished in Colombia, with the 1853 Constitution. X_m is a vector of municipal specific controls that are likely to affect the local dynamics of crime. These are described in Section 2.

We are interested in the estimate of β_1. A positive estimate suggests that higher level of inequality produce higher criminal rates. The exclusion restriction implies that the settlement of

slaves before the abolition of slavery does not affect current crime through a channel different than economic inequality. We believe this is very likely to be the case for two main reasons. First, we rely on the recent political economy literature that shows that the legacy of slavery is mainly manifested in persistent levels of economic and political inequality (Engerman and Sokoloff, 1997 and 2000; Nunn, 2005; Soares, Assunção and Goulart, 2012). Second, our set of controls includes municipal level characteristics that net out the effect of other potential channels. Indeed, as we will show in the next section, our estimates are robust to controlling for average education levels, the quality of local institutions, a proxy of economic performance, the current proportion of black population and geographic characteristics that may be correlated with the slave economy, among other considerations.

In any case, to address any remaining concern about the validity of the instrument conditional on the aforementioned controls, we investigate the robustness of our baseline estimates in circumstances in which the instrumental variable does not perfectly meets the exclusion restriction. In particular, Conley, Hansen and Rossi (2012) propose a way to construct conservative confidence intervals of the structural coefficient of interest (in our case β_1) in the face of an instrument that is only plausibly exogenous. To this end, consider the following simultaneous equation model:

$$crime_m = \beta \widehat{EI_m} + bSlavery_m + \delta' X_m + \epsilon_m \qquad (4)$$

$$EI_m = \phi_1 Slavery_m + u_m \qquad (5)$$

Following Conley, Hansen and Rossi (2012), interval estimates of β, our coefficient of interest, are obtained by taking the *union* of all confidence interval estimates across all plausible values of b, the direct effect of the instrument on the outcome (which for a perfectly met exclusion restriction should be equal to zero).[14]

[14] As an alternative approach, we follow (Berkowitz, Caner and Fang, 2012) who propose a fractionally resampled Anderson-Rubin test (FAR) to assess the significance of endogenous regressors in an instrumental variables estimation. The FAR test allows us to conduct reliable inference in presence of a plausible violation of the exclusion restriction.

4. Results

4.1 Initial Results

Table 2 reports the results from estimating a naïve linear regression of the crime rates on our measure of economic inequality. We do so for comparison purposes with the IV coefficients, reported in Table 3. The tables presented hereafter have all the same structure except for Tables 7 to 9 in the Appendix. They include 6 columns. The first column includes no controls. The subsequent columns include, additively, a standard set of controls in the economics of crime literature: Column 2 includes population density and column 3 includes the proportion of males in the age range 15-29, which captures the supply of (the most common) crime perpetrators. Column 4 adds as control the average years of education in each municipality. The idea is that education increases the opportunity cost of engaging in criminal behavior, as it increases one's chances in the legal labor market. Column 5 controls for the efficiency of the criminal justice system at the local level, and column 6 includes the per capita tax revenues as a proxy for local economic activity, in the absence of municipal-level GDP data.[15]

Table 2 includes three panels, respectively showing the results for the murder rate, the rate of car theft and the crime index. The estimated coefficient of the effect of economic inequality on the per capita murder rate is very stable across specifications. Using the most conservative specification in column 6 it implies that a one standard increase in the land Gini (equivalent to 0.09, see Table 1) is associated with an increase in the murder rate of 0.068 (0.09 × 0.755) assassinations per capita, equivalent to a fourth of a standard deviation. This effect seems somewhat small. However, recall that the naïve OLS estimates are likely to be downward biased, both because of the potential redistributive impact of crime as well as for the attenuation driven by the likely measurement error.

Regarding the effect of inequality on the rate of car theft the magnitude of the estimated coefficient is not so stable. It drops as controls are added progressively, in particular when the average municipal education is accounted for. The estimated coefficient of column 6 is the smallest of all, and it implies that a one standard deviation increase in inequality is associated with an increase in the rate of car theft of 0.03 (0.09 × 0.277) thefts per capita, equivalent to a sixth of a standard deviation.

[15] Adding the judicial efficiency index reduces our sample from 541 municipalities to 538.

The estimated coefficient of interest in the regression involving the crime index is much more stable. A one standard deviation increase in our measure of economic inequality is associated with an increase in the crime index of 0.07 (0.09 × 0.730), equivalent to a fourth of a standard deviation.

Table 3 reports the IV coefficients, estimated by 2SLS. In addition to the three panels of Table 2, it includes a fourth panel (D), which reports the first stage estimates (which is the same for the three outcomes). The effect of past slavery on current economic inequality is, as expected, positive and significant across specifications. A one standard deviation increase in the proportion of slaves as measured by the 1851 Census (which is equivalent to a 1 percent increase), is associated with a fourth of a standard deviation increase in current economic inequality. In addition, the F-statistic of the Kleibergen-Paap Wald rk test of weak instruments is very large throughout (= 46.1 in the most demanding specification).

The estimated IV coefficients are larger than the OLS ones reported in Table 2. This is consistent with our prior regarding the downward bias of the OLS coefficients, both because of the potential redistributive effect of conflict and because of measurement error. It is also reassuring that the magnitude of the coefficients is very stable across specifications, even for car theft, which presented very unstable OLS estimates (Panel B, Table 2).

Using the most complete specifications (column 6), we find that a one standard deviation increase in inequality causes a 0.32 increase in the per capita murder rate, equivalent to 1.15 standard deviation. In the case of car theft, an equivalent increase in our measure of economic inequality causes an increase in this outcome of about two-thirds of a standard deviation. Finally, the IV estimates of the effect of inequality on our crime index are reported on Panel C of Table 3. As in the case of the murder rate, a one standard deviation increase in inequality causes an increase in the index equivalent to 1.15 standard deviations.

As mentioned, the magnitude of the coefficients is larger in the IV specifications relative to OLS. While this is consistent with the direction of the bias we expected, IV coefficients are about four times larger in the most demanding specifications (column 6). A gap this size could also arise if the exclusion restriction was not met perfectly. In Table 4 we allow for such possibility and, following Conley, Hansen and Rossi (2012), report the lower and upper 95 percent confidence intervals of the coefficient of interest (β in equation 4) for *non-zero* values of b (the direct effect of the instrument on the outcome) in different ranges, from (−0.1,0.1) to

(−1,1). Importantly, regardless of the magnitude of the (nonzero) plausibly endogenous relationship between the instrument and the outcome, the confidence bound of the estimated relationship of interest is quite consistent and does not overlap with zero in any of the cases. This confirms that the impact of inequality on criminal activities is positive and significant, even if one thinks that the instrument is only plausibly exogenous.[16]

Table 5 shows the results from estimating the reduced-form regression. The association between the share of slaves in the nineteenth century and current crime is positive and significant throughout specifications, and the magnitude is fairly stable. In the case of murder, a one standard deviation increase in the share of slaves translates into an increase in the homicide rate of almost a third of a standard deviation. The substantive effect is very similar in the case of the crime index, whereas for vehicle theft a one standard deviation increase in the proportion of slaves in the mid-1800s is associated with an increase in this crime of about a sixth of a standard deviation.

4.2 Further Robustness

The effect of economic inequality on both property and violent crime is positive and very robust to controlling for a set of variables that are likely to affect the incentives for engaging in criminal behavior. These include demographic characteristics, education, economic conditions and institutional quality.

One limitation of our estimates has to do with the fact that they exclude the state of Antioquia, one of the largest of Colombia, and with crime rates that are higher than the national average, largely inherited from the time of the Medellin Cartel and the war against drugs. Unfortunately, Antioquia maintains its own cadastre and its information is not publicly available. This prevents us from being able to interpret our results as national averages.[17] Nevertheless, we do have information for Antioquia on both crime rates and the share of slaves in the nineteenth century. Indeed, the former data are provided by the National Police and the latter were obtained from the 1851 population census, and thus none of it can be withheld by the state's governor's office.

[16] We also conducted, following (Berkowitz, Caner and Fang, 2012), a fractionally resampled Anderson-Rubin test. The null hypothesis of the FAR test is that the impact of inequality on crime is equal to zero. This is rejected for all variables under different scenarios, providing further evidence of the robustness of our empirical findings. These results are available upon request.

[17] Of course, the IV estimates of Table 3 have to be interpreted as local average treatment effects.

Thus, in Table 6 we report again reduced-form estimates of Table 5, this time including Antioquia. Our sample is now 74 municipalities larger.[18] It is reassuring that the estimates are not only still positive and significant across outcomes, but also that their magnitudes are virtually unchanged (though slightly smaller for the case of murder and slightly larger for the case of vehicle theft). We are then confident that the exclusion of Antioquia is very unlikely to be biasing out IV results of Table 3 in any systematic way.

Our argument hinges on the long-term legacy of slavery in terms of economic inequality. It is not a story about the effect on conflict of current ethnic differences across municipalities, as proxied by slavery over 150 years ago. This point is important because, as pointed out by Lagerlof (2005) for the case of the United States, slavery in 1850 is highly correlated with the current proportion of black population at the county level. This is also the case of Colombia. Our data suggest that the municipal-level correlation between the proportion of slaves in 1851 and that of self-reported blacks in the 2005 census is 0.55.

It is then important to show that our results are robust to controlling for the current proportion of blacks. If this were not the case, then our argument about the legacy of slavery in terms of inequality would be weakened. Table 7 looks at this issue. In addition to the complete set of controls included in the previous results (as of column 6 of the previous tables), columns 1 to 3 of the table include the current proportion of (self-identified) blacks, as registered by the 2005 Colombian population census. Column 1 reports the naïve OLS estimates for comparison; column 2 the reduced form and column 3 the IV results. As with the previous tables, the three outcomes are investigated in three different panels, respectively murder (Panel A), vehicle theft (Panel B), and the crime index (Panel C).

The effect of inequality on crime is positive and significant across outcomes and econometric specifications. The results are also somewhat larger in magnitude. Focusing on the IV results of column 3, a one standard deviation increase in the land Gini coefficient, increases the murder rate by 1.3 standard deviations. It also increases the rate of vehicles theft by just over one standard deviation, and the crime index by 1.5 standard deviations.

An additional robustness check is motivated by the argument of Engerman and Sokoloff (1997, 2000) that the use of slaves was determined by exogenous geographical characteristics that were adequate for large-scale crops like cotton, sugar or tobacco. This hypothesis finds

[18] Recall that the estimation uses administrative divisions as of the mid-nineteenth century.

support in the works of Easterly (2007), who shows that current inequality is correlated with the geographical suitability for sugarcane production cross-country, and Lagerlof (2005), who shows that historical slavery in U.S. counties is correlated with geographic conditions including elevation and other variables associated with soil quality. If the geographic characteristics that favored the acquisition of slaves in the nineteenth century in Colombia can also predict current crime rates at the municipal level to some extent, then failing to control for these would bias our results.

Table 7 includes the average municipal elevation and the average soil quality as controls, in addition to all the other controls (including the current proportion of black population). Columns 4 to 6 report the OLS estimates, the reduced-form, and IV results respectively. The estimated coefficients are again positive and significant across these specifications and for the three outcomes, and their magnitude is virtually unchanged with respect to the effects computed for columns 1 to 3. We conclude that geography does not drive our results.

Acemoglu, García-Jimeno and Robinson (2012) investigate the effect of slavery on long-run development in Colombia by comparing municipalities with gold mines during the colonial period with neighboring municipalities without mines. The idea is that gold mines during the seventeenth and eighteenth centuries, now depleted, were a major source of demand for slaves. The authors conclude that slavery is associated with worse current development indicators. While this paper is the closest to ours, the empirical strategy differs. We rely on Engerman and Sokoloff (1997 and 2000) to argue that slavery was the seed of large initial economic and political inequalities that persisted through the centuries. This motivates our first stage, which is hence not limited to the places that had gold mines, but generally to all the places that had slaves before the abolition of this institution and hence before slaves could freely decide where to settle. As Engerman and Sokoloff noted, beyond mining these were places with large-scale plantations. Hence, to show that our results are not driven by the towns that had gold mines during the colony, in Table 8 we repeat our exercise on a sample that excludes these municipalities (columns 1 to 3) as well as on a sample that excludes both the mining towns and their neighbors (columns 4 to 6). Throughout this table we include the entire set of controls, inclusive those explored in Table 7 as robustness (i.e., the ethnicity and geography controls).

The baseline results are robust to excluding these subsamples, which implies that our results are not driven by the towns included in the analysis of Acemoglu, García-Jimeno and

Robinson (2012). The estimated coefficients are positive throughout and the magnitude remarkably similar, with the IV coefficients (columns 3 and 6) only slightly larger than in the previous table. Also, the significance of the coefficients is not reduced by the efficiency loss that comes from reducing the estimation sample.

As an additional robustness check looks at whether the results are driven by using the Gini coefficient as proxy for economic inequality. There are several alternative measures other than the Gini coefficient. Two of the most popular are the Theil index and the Atkinson index. The Theil index is a special case and the most widely used version of the generalized entropy index. While the Gini coefficient is arguably more intuitive, as it is based on the Lorenz curve, the overall Theil index has the property of being additive across different subgroups, so it can be decomposed in the inequality between groups and the inequality within them. The Atkinson index is useful for determining which tail of the observed distribution contributes more to the overall inequality. It includes an "inequality aversion" parameter that, when varied, produces different values of the index that put different weights to different parts of the distribution. Here we use the index that sets the aversion parameter to 1, which makes the index more sensitive to changes at the left tail of the distribution. Our results are, however, robust to indices that use other values of the inequality aversion parameter.

Table 9 looks at the effect of economic inequality on our three outcomes of crime using as the measure of inequality the Theil index (columns 1 to 3) and the Atkinson index (columns 4 and 5). The entire set of controls, including the proportion of contemporaneous black population and the geographic controls, is included in all the regressions. Using either of the alternative economic inequality measures does not change our results in substantive terms. The estimated coefficients of the OLS regression, the reduced form and the IV regression are positive and significant for both inequality measures and all crime outcomes.[19]

The magnitude of the estimated coefficients when using the Theil index is between four and five times smaller than the estimates of the equivalent model using the Gini coefficient (Table 7, columns 4 to 6 that use the entire set of controls). This is because while the Theil index is left-bounded at zero, it can be greater than 1. Indeed, as can be seen in Table 1, the mean of the Theil index in our sample is five times larger than that of the Gini coefficient, and so is its maximum. In turn, the estimated coefficients using the Atkinson index (a measure that does vary

[19] The reduced form is not included under the Atkinson index label, as it is the same as the one reported in column 2.

between 0 and 1) are virtually unchanged with respect to the Gini benchmark. Thus, accounting for their measurement details, Table 9 shows that using alternative proxies of economic inequality leaves our results unchanged in terms of both significance and magnitude.

Our last robustness check uses as the treatment variable an indicator of municipalities with high economic inequality. This is done either by separating municipalities according to the median of the distribution of the Gini coefficient (columns 1 to 4 of Table 10), or according to its mean (columns 5 to 8). In each case we report the results of using two alternative IV specifications. First, we instrument the indicator of high economic inequality with an indicator of high proportion of slaves using the same statistic in both cases (that is the median in column 3 and the mean in column 7). Second, we instrument the indicator of high economic inequality with the entire distribution of the proportion of slaves in the nineteenth century (column 4 for the median and 8 for the mean).

The results are generally robust to this alternative measure of economic inequality, which may arguably be subject to less measurement error. With the exception of the first IV approach (that instruments the indicator of the upper median (mean) of economic inequality with the indicator of the upper median (mean) of the proportion of slaves), the estimated coefficient is in all cases positive and significant.[20]

5. Conclusion

Economic inequality is often considered a key factor in explaining criminal behavior. Building on the contribution of Becker (1968), a large theoretical literature suggests that inequality increases the incentives of (property) crime. More recently it has been argued that inequality causes anxiety, mental disorder and lack of trust, thus also increasing violent crime. The empirical literature is less conclusive, perhaps owing to methodological shortcomings and measurement error. Indeed, economic inequality is difficult to define and measure in a standardized way cross-nationally, and most of the existing literature suffers from endogeneity and omitted variable bias.

We investigate empirically the relationship between economic inequality and crime using subnational-level data for Colombia, a country that is both very unequal and that features very high crime rates. In order to address the usual identification challenges, we instrument economic

[20] The second IV approach is just short of being significant for the case of vehicle theft.

inequality at the municipality level with the share of slaves in 1851, just before the abolition of slavery in 1853. Our approach is motivated by the growing political economy literature on the long term legacy of slavery. Engerman and Sokoloff (1997, 2000) argue that pre-existing geo-ecological characteristics of colonies determined the economic activities of the colonizers and the use of slave labor. This resulted in differential levels of initial economic inequality that persisted overtime, and is correlated with current inequality. This hypothesis has found support in the empirical contributions of several authors.

Our findings point to the existence of a positive and significant effect of inequality, as measured by the Gini coefficient of the value of land, on both property and violent crimes. The results are robust to the inclusion of several of the usual determinants of crime (like the proportion of young males, the average education level and overall economic activity), as well as of other potential channels through which slavery can impact current crime (like the quality of local law enforcement institutions). We also show that our results are not driven by current ethnic differences in the population or by geographic characteristics that may be correlated with the slave economy in the nineteenth century and with current crime. Moreover, our results are robust to the use of different measures of economic inequality, such as the Gini coefficient, the Theil index and the Atkinson index.

Our results suggest that policy initiative to reduce crime (both property and violent) should deal with structural economic inequalities. Current policing efforts and hot-spots patrolling are unlikely to reduce crime in the long run if not accompanied by efforts to break persistent cycles of inequality. This can be done through a large number of policy instruments, like increasing the quality of education, to name only one. However, this paper is silent about the expected success of specific inequality-reducing policies, and this has to be informed by future research and by studying and adapting successful experiences.

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Table 1. Summary Statistics

	Mean	Std. Dev.	Min.	Max.	N
Panel A: *Outcomes*					
Murder per capita	0.28	0.28	0	2.09	541
Vehicle theft per capita	0.11	0.16	0	1.15	541
Crime index (PCA)	0.28	0.26	0	1.60	541
Panel B: *Economic Inequality*					
Gini coefficient	0.68	0.09	0.21	0.91	541
Theil index	1.07	0.45	0.09	4.37	540
Atkinson index	0.64	0.13	0.09	0.95	540
Panel C: *Instrument*					
Prop. slaves 1851	0.003	0.010	0	0.11	541
Panel D: *Controls*					
Pop. density	0.14	0.50	0.002	7.39	541
Prop. males age 15-29	0.25	0.03	0.17	0.52	541
Years education	40.86	0.97	2.59	8.39	541
Judicial efficiency	0.60	0.63	-2.27	5.56	538
Tax revenue per capita	0.07	0.09	0.002	0.84	541
Prop. blacks	0.07	0.18	0	0.97	541
Elevation	1,268	956	2	3,087	530
Soil quality	2.74	1.22	0	6.67	531

Note: Variables measurement and sources described in Section 2.

Table 2. Effect of Economic Inequality on Crime, Ordinary Least Squares

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variable: *Murder rate*						
Ec. Inequality	0.772***	0.797***	0.738***	0.815***	0.784***	0.755***
	(0.158)	(0.162)	(0.174)	(0.185)	(0.185)	(0.185)
Panel B. Dependent variable: *Vehicle theft rate*						
Ec. Inequality	0.625***	0.534***	0.511***	0.319***	0.318***	0.277***
	(0.0864)	(0.0807)	(0.0774)	(0.0622)	(0.0626)	(0.0624)
Panel C. Dependent variable: *Crime index (PCA)*						
Ec. Inequality	0.988***	0.941***	0.883***	0.802***	0.779***	0.730***
	(0.142)	(0.143)	(0.151)	(0.152)	(0.152)	(0.152)
Controls:						
Pop. density		Y	Y	Y	Y	Y
% males aged 15-29			Y	Y	Y	Y
Education				Y	Y	Y
Judicial efficiency					Y	Y
Economic activity						Y
Observations	541	541	541	541	538	538

Notes: Ordinary Least Squares regression. Standard errors in parentheses. Regression sample given by municipalities as existing in 1851, before the abolition of slavery. Sample excludes department of Antioquia, which has its own independent cadaster that is confidential. * significant at the 10 % level, ** significant at the 5% level, *** is significant at the 1% level.

Table 3. Effect of Economic Inequality on Crime, Instrumental Variables

	(1)	(2)	(3)	(4)	(5)	(6)
			Second Stage			
Panel A. Dependent variable: *Murder rate*						
Ec. Inequality	3.103***	3.147***	3.395***	3.543***	3.472***	3.553***
	(0.525)	(0.532)	(0.651)	(0.632)	(0.636)	(0.673)
Panel B. Dependent variable: *Vehicle theft rate*						
Ec. Inequality	1.193***	1.149***	1.195***	1.143***	1.136***	1.109***
	(0.401)	(0.387)	(0.440)	(0.409)	(0.417)	(0.416)
Panel C. Dependent variable: *Crime index (PCA)*						
Ec. Inequality	3.038***	3.038***	3.246***	3.313***	3.258***	3.297***
	(0.425)	(0.429)	(0.519)	(0.556)	(0.559)	(0.587)
			First Stage			
Panel D. Dependent variable: *Economic Inequality*						
Share slaves 19th cent.	2.742***	2.709***	2.391***	2.295***	2.270***	2.179***
	(0.339)	(0.338)	(0.340)	(0.315)	(0.314)	(0.321)
(K-P) F-Stat.						
Controls:						
Pop. density		Y	Y	Y	Y	Y
% males aged 15-29			Y	Y	Y	Y
Education				Y	Y	Y
Judicial efficiency					Y	Y
Economic activity						Y
Observations	541	541	541	541	538	538

Notes: Ordinary Least Squares regression. Standard errors in parentheses. Regression sample given by municipalities as existing in 1851, before the abolition of slavery. Sample excludes department of Antioquia, which has its own independent cadaster that is confidential. * significant at the 10 % level, ** significant at the 5% level, *** significant at the 1% level.

Table 4. Confidence Bounds of the Effect of Inequality on Crime Relaxing the Exogeneity Assumption of the Instrument

Support interval for possible values of *b*	*Murder rate*		*Vehicle theft rate*		*Crime index (PCA)*	
	Lower	Upper	Lower	Upper	Lower	Upper
$b \in$ (-0.1,+0.1)	2.038	4.168	.371	2.015	2.168	3.906
$b \in$ (-0.2,+0.2)	2.001	4.203	.334	2.052	2.131	3.943
$b \in$ (-0.3,+0.3)	1.963	4.240	.297	2.088	2.094	3.979
$b \in$ (-0.4,+0.4)	1.926	4.276	.260	2.125	2.056	4.015
$b \in$ (-0.5,+0.5)	1.889	4.312	.223	2.162	2.019	4.051
$b \in$ (-1,+1)	1.701	4.494	.037	2.348	1.830	4.234

Notes: 95% confidence lower and upper bounds are estimated according to the approach proposed by Conley, Hansen and Rossi (2012). All estimates include the entire set of control as in column 6 of Tables 2 and 3.

Table 5. Effect of Economic Inequality on Crime, Reduced Form

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variable: *Murder rate*						
Share slaves 19th cent.	8.509***	8.525***	8.117***	8.130***	7.883***	7.743***
	(1.849)	(1.853)	(1.870)	(1.865)	(1.860)	(1.861)
Panel B. Dependent variable: *Vehicle theft rate*						
Share slaves 19th cent.	3.271***	3.112***	2.858**	2.622***	2.579***	2.417***
	(1.179)	(1.107)	(1.114)	(0.885)	(0.890)	(0.829)
Panel C. Dependent variable: *Crime index (PCA)*						
Share slaves 19th cent.	8.329***	8.229***	7.760***	7.603***	7.397***	7.184***
	(1.616)	(1.583)	(1.586)	(1.523)	(1.515)	(1.482)
Controls:						
Pop. density		Y	Y	Y	Y	Y
% males aged 15-29			Y	Y	Y	Y
Education				Y	Y	Y
Judicial efficiency					Y	Y
Economic activity						Y
Observations	541	541	541	541	538	538

Notes: Ordinary Least Squares regression. Standard errors in parentheses. Regression sample given by municipalities as existing in 1851, before the abolition of slavery. Sample excludes department of Antioquia, which has its own independent cadaster that is confidential. * significant at the 10 percent level, ** significant at the 5% level, *** significant at the 1% level.

Table 6. Effect of Economic Inequality on Crime, Reduced Form (including Antioquia)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variable: *Murder rate*						
Share slaves 19th cent.	8.324***	8.318***	7.922***	7.965***	7.680***	7.523***
	(1.800)	(1.800)	(1.816)	(1.808)	(1.801)	(1.798)
Panel B. Dependent variable: *Vehicle theft rate*						
Share slaves 19th cent.	3.511***	3.348***	3.041***	2.844***	2.834***	2.699***
	(1.216)	(1.123)	(1.125)	(0.897)	(0.904)	(0.855)
Panel C. Dependent variable: *Crime index (PCA)*						
Share slaves 19th cent.	8.368***	8.249***	7.752***	7.643***	7.434***	7.228***
	(1.632)	(1.576)	(1.574)	(1.522)	(1.513)	(1.479)
Controls:						
Pop. density		Y	Y	Y	Y	Y
% males aged 15-29			Y	Y	Y	Y
Education				Y	Y	Y
Judicial efficiency					Y	Y
Economic activity						Y
Observations	541	541	541	541	538	538

Notes: Ordinary Least Squares regression. Standard errors in parentheses. Regression sample given by municipalities as existing in 1851, before the abolition of slavery. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 7. Effect of Economic Inequality on Crime, Robustness Check 1 (including current share of blacks and geographical controls)

	OLS (1)	Red. form. (2)	OLS (3)	Red. form. (4)	OLS (5)	Red. form. (6)
Panel A. Dependent variable: *Murder rate*						
Ec. Inequality	0.693***		4.100***	0.633***		4.094***
	(0.178)		(0.990)	(0.182)		(0.989)
Share slaves 19th cent.		8.027***			7.875***	
		(2.412)			(2.309)	
Panel B. Dependent variable: *Vehicle theft rate*						
Ec. Inequality	0.290***		1.866***	0.276***		1.819***
	(0.0628)		(0.605)	(0.0633)		(0.600)
Share slaves 19th cent.		3.653***			3.500***	
		(0.930)			(0.903)	
Panel C. Dependent variable: *Crime index (PCA)*						
Ec. Inequality	0.695***		4.218***	0.643***		4.181***
	(0.149)		(0.903)	(0.151)		(0.894)
Share slaves 19th cent.		8.259***			8.043***	
		(1.931)			(1.821)	
Controls:						
Pop. density		Y	Y	Y	Y	Y
% males aged 15-29			Y	Y	Y	Y
Education				Y	Y	Y
Judicial efficiency					Y	Y
Economic activity						Y
Observations	538	538	538	538	538	538

Notes: Ordinary Least Squares regression. Standard errors in parentheses. Regression sample given by municipalities as existing in 1851, before the abolition of slavery. Sample excludes department of Antioquia, which has its own independent cadaster that is confidential. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 8. Effect of Economic Inequality on Crime, Robustness Check 2 (excluding municipalities with gold mines in the colonial times and their neighbors)

	OLS (1)	Red. form. (2)	OLS (3)	Red. form. (4)	OLS (5)	Red. form. (6)
Panel A. Dependent variable: *Murder rate*						
Ec. Inequality	0.598***		4.290***	0.515**		4.199***
	(0.188)		(1.232)	(0.200)		(1.035)
Share slaves 19th cent.		8.061***			10.15**	
		(3.075)			(4.300)	
Panel B. Dependent variable: *Vehicle theft rate*						
Ec. Inequality	0.284***		2.501***	0.284***		1.897***
	(0.066)		(0.730)	(0.0703)		(0.529)
Share slaves 19th cent.		4.700***			4.584***	
		(1.062)			(1.264)	
Panel C. Dependent variable: *Crime index (PCA)*						
Ec. Inequality	0.624***		4.802***	0.565***		4.311***
	(0.158)		(1.147)	(0.170)		(0.816)
Share slaves 19th cent.		9.023***			10.42***	
		(2.611)			(3.555)	
Controls:						
Pop. density		Y	Y	Y	Y	Y
% males aged 15-29			Y	Y	Y	Y
Education				Y	Y	Y
Judicial efficiency					Y	Y
Economic activity						Y
Observations	505	505	505	451	451	451

Notes: Ordinary Least Squares regression. Standard errors in parentheses. Regression sample given by municipalities as existing in 1851, before the abolition of slavery. Sample excludes department of Antioquia, which has its own independent cadastre that is confidential. [a] Including the current share of black population and geographic characteristics. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 9. Effect of Economic Inequality on Crime, Robustness Check 3 (alternative measures of inequality)

	Theil index			Atkinson index	
	OLS	Red. form	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)
Panel A. Dependent variable: *Murder rate*					
Ec. inequality	0.136***		0.730***	0.409***	3.263***
	(0.0370)		(0.163)	(0.124)	(0.817)
Share slaves 19th cent.		7.891***			
		(2.313)			
Panel B. Dependent variable: *Vehicle theft rate*					
Ec. inequality	0.0405**		0.322***	0.221***	1.440***
	(0.0180)		(0.111)	(0.0423)	(0.488)
Share slaves 19th cent.		3.482***			
		(0.902)			
Panel C. Dependent variable: *Crime index (PCA)*					
Ec. inequality	0.125***		0.744***	0.445***	3.325***
	(0.0350)		(0.154)	(0.102)	(0.746)
Share slaves 19th cent.		8.042***			
		(1.821)			
Previous controls	Y	Y	Y	Y	Y
Observations	527	527	527	527	527

Notes: Ordinary Least Squares regression. Standard errors in parentheses. Regression sample given by municipalities as existing in 1851, before the abolition of slavery. Sample excludes department of Antioquia, which has its own independent cadastre that is confidential. [a] Including the current share of black population and geographic characteristics. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 10. Effect of Economic Inequality on Crime, Robustness Check 4 (indicator of high inequality)

Indicator based on:	Median				Mean			
	OLS	Red. form	IV1	IV2	OLS	Red. form	IV1	IV2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Dependent variable: *Murder rate*								
Ec. inequality	0.0914***		41.63	2.437**	0.0993***		33.56*	1.828***
	(0.025)		(42.97)	(1.215)	(0.0244)		(17.64)	(0.704)
Share slaves 19th cent.		1.647***				3.027***		
		(0.540)				(0.906)		
Panel B. Dependent variable: *Vehicle theft rate*								
Ec. inequality	0.0184*		85.37	1.083	0.0202**		67.34*	0.813**
	(0.009)		(89.17)	(0.669)	(0.00973)		(36.89)	(0.412)
Share slaves 19th cent.		3.378***				6.073***		
		(1.269)				(2.124)		
Panel C. Dependent variable: *Crime index (PCA)*								
Ec. inequality	0.0777***		89.80	2.489*	0.0845***		71.34*	1.867**
	(0.021)		(93.41)	(1.272)	(0.0204)		(38.50)	(0.732)
Share slaves 19th cent.		3.553***				6.434***		
		(1.278)				(2.139)		
Previous controls	Y	Y	Y	Y	Y	Y	Y	Y
Observations	600	617	617	528	600	617	617	528

Notes: Ordinary Least Squares regression. Standard errors in parentheses. Regression sample given by municipalities as existing in 1851, before the abolition of slavery. Sample excludes department of Antioquia, which has its own independent cadaster that is confidential. IV 1 instruments the indicator of high inequality (as given by the median of the Gini distribution in columns 1-4 or by its mean in columns 5-8) with the indicator of high proportion of slaves in 1851 (median in columns 1-4 and mean in columns 5-8). IV 2 instruments the indicator of high inequality with the entire distribution of the proportion of slaves. [a] Including the current share of black population and geographic characteristics.

* significant at the 10% level, ** significant at the 5% level, *** significant at the 1 % level.