

Empirical Issues in the Homicide-Income Inequality Argument

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Abstract

The argument that income inequality increases homicide rates has provoked scholarly debate, with some studies not supporting this position and providing evidence to the contrary. We identify several empirical issues with the current body of evidence, as well as their underlying problems. We challenge these issues by using more robust techniques than those typical of this literature. Based on the case of Mexican municipalities, we provide evidence that in fact, homicide rates correlated negatively with income inequality between 1990 and 2015, and that this relationship was moderated by levels of socioeconomic marginality. Likewise, we show evidence of spatial dependence in the relationship, thus challenging the assumption that geospatial units of analysis are probabilistically independent of each other.

Keywords

homicide, income inequality, poverty, Mexico

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Introduction

Scholars contend that income inequality increases homicide due to strains, anomic pressures, and/or social disorganization. In this regard, the income inequality argument for spatial variation in homicide found in the literature is popular for its simplicity and ease of generalization, as well as due to the fact that income inequality is a well-defined social problem, oftentimes associated with race and gender. Furthermore, as it informs a wide variety of policies, the argument itself facilitates effective communication between researchers of different disciplines (Heimer, 2019). Perhaps as a result, there is little dispute about the validity of the argument, especially when much evidence exists showing that income inequality correlates positively with homicide (Hsieh & Pugh, 1993; Messner et al., 2002). However, there is cross-sectional evidence that does not conform to the argument (Pare & Felson, 2014; Pridemore, 2008, 2011) and panel evidence that contradicts it (Brush, 2007; Messner et al., 2002; Neumayer, 2003), though some studies, also using panel data, do provide evidence in support (Fajnzylber et al., 2002; Loureiro & Silva, 2012; Siegel et al., 2013).

Clearly, there are empirical issues with the argument that require examination. Among these are whether evidence suffers from endogeneity bias from different sources such as measurement error, omitted variable bias, and/or simultaneity bias (Enamorado et al., 2016; Fajnzylber et al., 2002; Jencks & Leigh, 2006; Kelly, 2000; Neumayer, 2003; Rufrancos et al., 2013; Stucky et al., 2016); whether collinearity was considered in the testing of models (Land et al., 1990; McCall et al., 2010; Pare & Felson, 2014; Parker et al., 1999); whether results are time and spatially dependent, thus indicative of substantive differences in cultural norms and local circumstances or the result of nuisance dependence (Baller et al., 2001; Messner et al., 1999; Vilalta, Lopez-Ramirez, et al., 2021); and/or whether the effect of income inequality is constant throughout the homicide distribution (Rennó Santos et al., 2018). These issues are central as they may suggest that current evidence is misleading, and that the argument needs to be reformulated.

Besides the empirical issues, there are also conceptual issues that need to be addressed. One of these is that most studies treat income inequality and poverty, that is, relative and absolute deprivation, separately, though they seem complementary for predicting homicide (Burraston et al., 2018, 2019; Messner, 1982; Messner & Tardiff, 1986; Pare & Felson, 2014; Pridemore, 2008, 2011; Pridemore & Trent, 2010). If complementary, they are the core of the more general deprivation-homicide argument and should be studied in combination.

Using the case of Mexico between 1990 and 2015, we further previous work by addressing three of the empirical issues mentioned above, namely,

endogeneity, collinearity, and spatial dependence, while controlling for absolute deprivation.¹ Our empirical strategy is designed to minimize the effects of these issues as much as possible. By doing so, we provide robust evidence that municipal-level income inequality has been negatively associated with homicide rates in Mexico.

We are aware that Mexico falls in the upper tail on the world homicide rates distribution and is currently facing a war on organized crime. Mexico is thus an interesting case in which to hard test the homicide-income inequality argument given its *ad-hoc* conditions—conditions which may be either unobservable in most countries or are unobserved in cross-national studies.

The Homicide-Income Inequality Argument

As stated in the introduction, the homicide-income inequality link has been explained from three social-structure theoretical perspectives, namely, Strain theory, Institutional anomie, and Collective efficacy. In terms of the argument, these three theoretical perspectives are in fact compatible rather than redundant, given that the origins of the first two perspectives can be attributed to the seminal works of Merton (Agnew, 1997; Featherstone & Deflem, 2003; Messner, 1988).

The main contribution of strain theory to the argument is the idea that individuals sometimes respond to anger and frustration with crime (Agnew, 1999, 2013; Agnew & Brezina, 2019; Pratt & Godsey, 2003). Anger and frustration are key to the homicide-income inequality argument as violence results from blaming others for income inequality induced adversity (Agnew, 1992). People may demand equality of income based on living in the same place as others (Agnew, 1999; Burraston et al., 2019) or feeling similar to them (Hipp, 2007). Thus an individual may commit a crime either because economic resources have been inequitably distributed or as a means of retribution (Chamberlain & Hipp, 2015). As a result, strain theory predicts more crime where social relations are strained due to income inequality (Cao & Zhang, 2017; Kawachi et al., 1999; Nivette, 2011; Ouimet, 2012).

Strain theory has been expanded from the individual to the community level of analysis (Agnew, 1999, 2011; de Beeck et al., 2012; Botchkovar et al., 2018; Brezina et al., 2001; Eitle & Eitle, 2016). This extension of strain theory to the community has been called the macro-level or multi-level version of strain theory (de Beeck et al., 2012; Botchkovar et al., 2018; Brezina et al., 2001). This extension is not surprising as environmental criminological theories frequently extend individual-level criminogenic predictors to communities or other aggregated levels of analysis, given that the criminogenic

characteristics of a community are oftentimes the aggregate of the individual criminogenic response to crime (Vilalta, Lopez-Ramirez, et al., 2021).

Clearly, where competition for inequitably distributed yet highly desired goods is severe, violent tactics of competition, such as homicidal violence, may seem justified to some (Daly, 2017). In this regard, it is easy to see a logical relationship between strain and anomie in Mertonian terms, basically in that “structural strain is [just] one way to explain why deviance occurs in the context of anomie” (Featherstone & Deflem, 2003). Thus, Anomie theory enters easily into the homicide-income inequality argument when proposing that profit-driven societies push social relations towards utilitarian ends, creating sets of normative values conducive to anomie (Messner & Rosenfeld, 2012). Furthermore, it is contended that Anomie may develop faster in places where income inequality limits the accomplishment of socially desirable goals (Bjerregaard & Cochran, 2008; Maume & Lee, 2003; Messner & Rosenfeld, 1997), such as in Mexico (Vilalta, 2013b, 2014; Vilalta & Muggah, 2016). Thus, given that income is a traditional source of status and respect, a low income in areas with high income inequality may create feelings of shame (Nadanovsky & Cunha-Cruz, 2009; Wilkinson et al., 1998).

If strain and anomie were the only mechanisms influencing homicidal violence, then the interactive effects of the community context and the individual would be incomplete in terms of our descriptive models, thereby affecting the empirical adequacy of our theories. In such a case, it may be expected that communities with high levels of collective efficacy shield the structural pressures created by high levels of strain and anomie. Indeed, Collective efficacy theory argues that collective efficacy, as a central mechanism for crime control in communities, mediates the correlation between concentrated disadvantage (i.e., absolute deprivation) and income inequality (i.e., relative deprivation) with homicide (Kawachi et al., 1999; Morenoff et al., 2001; Nadanovsky & Cunha-Cruz, 2009; Sampson et al., 1997; Vilalta & Muggah, 2016; Wilkinson, 2002). The causal pathway is that both types of deprivation erode social trust, thus reducing the willingness to create collective efficacy for a safer community (Elgar & Aitken, 2011; Morenoff et al., 2001; Pabayo et al., 2014). Importantly, this theory demands the examination of the combined effect of absolute and relative deprivation on homicide.

Despite its appeal and solid theoretical grounding, there are reasons to be wary of the homicide-income inequality argument. For one, it may be that in some societies, homicidal violence is more a response to disrespect and insult and less to income inequality (Nisbett, 1993). Also, some types of homicidal violence may be more or less (or not) associated with income inequality than others (Roberts & Willits, 2015). In Latin America, homicide has sometimes been associated with local conflictive conditions, such as guerrilla warfare or

paramilitary groups, rather than to socioeconomic conditions (Sandoval, 2018), although it may very well be that income inequality and poverty gave rise to the appearance of these groups in the first place. In the case of Mexico, the War on Organized Crime (WOC), launched by President Calderon in late 2006, consisted of the deployment of the armed forces to fight criminal organizations under the argument that parts of the country, particularly some municipalities in the state of Michoacan, were fully controlled by these organizations. Unfortunately, the strategy resulted in far more violence than expected, mostly due to an explosion of homicidal executions between criminal organizations, and multiple deaths from confrontations with the military. Homicide rates rose from 12.6 per 100,000 in 2000 to 22.5 in 2010 (i.e., an 80% increase).

Another source of caution is the seemingly implicit idea in many studies that homicide and income inequality do not co-evolve over time, that is, that they are exogenous. On the contrary, it should be expected that homicide and income inequality are endogenous with increases in homicidal violence resulting in a deterioration of local economic conditions through lost earnings, lost productivity, and outward-migration (Irvin-Erickson et al., 2016; Riascos & Vargas, 2011). This issue has empirical consequences, which we address below.

Empirical Issues in the Homicide-Income Inequality Argument

Land et al. (1990) maintained that differences in findings in the homicide-income inequality argument may be statistical or methodological artifacts. More recently, Pridemore and Trent (2010) warned of model misspecification problems in the literature. We would agree that one of the weaknesses of the homicide-income inequality literature is its overwhelming dependence on OLS cross-sectional estimator evidence (Hsieh & Pugh, 1993; Nivette, 2011; Rufrancos et al., 2013).

Another issue affecting the body of evidence is that income inequality data may be of poor quality, possibly resulting in biased estimators as it induces correlation with the error term (Messner et al., 2002). Among the typical measurement problems related to the quality of income inequality data are uneven coverage, unclear definitions of income, and differences in the unit of account (Forbes, 2000). The effect of poor income inequality data will be absorbed by the OLS error terms, resulting in attenuation bias. As the variance in the measurement error increases, estimator bias increases, meaning that positive estimators will become smaller towards zero, while negative estimators will also tend to zero, thus decreasing in magnitude—that is, it becomes more positive.

Homicide data may also be of poor quality due to underreporting. Measurement error in the dependent variable (DV), in our case, the homicide rate, will not lead to estimator bias, but will decrease precision by increasing the magnitude of the error terms. In general, while measurement error in the DV may be less critical than measurement error in the IV, both are problematic especially if both are present (Hausman, 2001).

An additional concern is that cross-sectional OLS estimates may be affected by omitted variable bias, thereby also resulting in biased estimators. In our study, income inequality, as measured by the Gini index, is negatively correlated with the Mexican Marginality index (a proxy measure of concentrated disadvantage or absolute deprivation) for 1990, 2000, 2010, but positively correlated for 2015, while at the same time the marginality index moderates the correlation between homicide rates and income inequality for the entire period under study. Thus, in addition to biased estimators, the effect of omitting the marginality index from the homicide-income inequality equation would be captured by the error term causing heteroscedasticity. In turn, this will result in unreliable significance tests on the estimators. Heteroscedasticity on its own does not bias OLS estimators, but omitted variable bias will result in biased estimators.

Furthermore, geographically aggregated data of analysis of homicide and income inequality are not spatially independent, but rather positively spatially dependent, that is, values in one location are similar to those from nearby locations (LeSage & Pace, 2010). This spatial pattern violates the cross-sectional OLS assumption that the data used for testing the argument are probabilistically independent and identically distributed. Once again, independence is assumed in the error term of the homicide-income inequality model. Positive spatial dependence leads to an inflation of type I errors due to artificially small estimates of standard errors. This occurs because the quantity of information carried by the data is less than what would be carried if observations were truly independent (Fletcher & Fortin, 2018; Haining, 2003).

Empirical complications multiply as more possibilities for error are introduced. As the effect of omitted variables is netted by the error term, the presence of spatially autocorrelated errors may also be indicative of omitted variable bias. Furthermore, if spatial dependence is present on both sides of the regression equation, omitted variable bias magnifies (Pace & Lesage, 2010). As a result, although a panel data fixed effects estimator in our homicide-income equation may account for any geographically aggregated unobserved variables, it is advisable to include spatial lags of homicide rates and income inequality measures as controls (Pace & Lesage, 2010; Vilalta, 2013a).

Spatial dependence may naturally imply the presence of key geographical processes driving the homicide-income inequality link. In fact, a recent study demonstrated the spatial link between violent crime and income inequality (Stucky et al., 2016). However, while theoretically relevant, spatial dependence may also be considered a nuisance in regression analysis. In sum, spatial dependence violates the three cross-sectional OLS assumptions of previous studies, namely, that income inequality data are free of measurement error, that no relevant independent variables are missing, and that the latter are exogenous (Plummer, 2010).

The seemingly implicit assumption in cross-sectional studies that income inequality is exogenous with respect to homicidal violence can also be questioned. There are reasons to suggest that the relationship between income inequality and homicidal violence is endogenous and simultaneous, such as reports of homicidal violence reducing economic mobility (Sharkey & Torrats-Espinosa, 2017), economic growth (Detotto & Pulina, 2013), and GDP per capita (Yepes et al., 2015). Concretely, Mexican organized crime has been found to deter foreign investment (Ashby & Ramos, 2013). One likely causal pathway is that as reductions in economic activity and investment cause poverty and income inequality, a recursive effect between the latter and homicide can be expected. Endogeneity leads to simultaneity bias, that is, that variables on both sides of an equation cause each other. This not only violates error independence but will result in biased estimators when testing the homicide-income inequality argument.

The above empirical issues imply that time-invariant or slow changing variables are needed in the examination of the argument. Some examples of time-invariant variables relevant for the study of the homicide-income inequality link in Mexico may be regional location (e.g., border with the U.S.), police and justice institutional effectiveness, local appropriations of legal norms, corruption or coercion of local police and judges by criminal organizations, the Narco music subculture, and *machismo* (Vilalta, 2020; Vilalta, Lopez-Ramirez, et al., 2021; Vilalta & Fondevila, 2020).

There are empirical solutions to some of these issues which will be used in this study. For instance, attenuation bias resulting from measurement error may be lessened with the 2SLS instrumental variable method, as may simultaneity bias resulting from endogeneity. Omitted variable bias resulting from the exclusion of geographically aggregated or place-invariant and/or time-invariant relevant variables when testing the argument, may also be eased with the use of the 2SLS instrumental variable method and/or the fixed-effects (FE and first-difference, FD) panel regression methods. FE and FD methods eliminate the impact of unobserved within-place variables if they are time invariant. The difference between the two methods is that FE

estimates are more efficient if error terms are serially uncorrelated, whereas FD are more efficient if they are serially correlated. The impact of time variant omitted variables can be controlled for with the use of yearly dummies. In this sense, panel analysis maintains the omitted variables fixed (constant) over time (within individual effects), constant over time (within time effects), or both (two-way fixed effects).

Lastly, as measurement error also affects panel data estimators (Nilsson & Agell, 2003), in this study we compare the results of the FD and FE estimates, as attenuation bias varies depending on the method, as explained by Griliches and Hausman (1986). Attenuation bias is less for short term periods but greater for longer periods of time (Cameron & Poot, 2019).

Below, we explain in detail the data and empirical strategy used in this study. Our empirical strategy is robust to the occurrence of previous statistical issues.

Data and Empirical Strategy

Data sources. The data utilized in this study are the 1990, 2000, 2010, 2015 Mexican homicide counts and their respective Gini indexes and Marginality indexes at the municipal level ($N=2,403, 2,443, 2,455, 2,458$, respectively). We use official and publicly available data from the Mexican National Institute of Statistics and Geography (INEGI), the National Social Development Policy Assessment Council (CONEVAL), and the National Population Council of Mexico (CONAPO). These are autonomous and technically-oriented institutions of the Mexican federal government.

Dependent and independent variables. Homicide data were collected from the INEGI mortality statistics. Homicide rates were calculated per 100,000 using the corresponding census data. The obtained rates were log transformed ($\ln$) for analysis given their skewed distribution. Zero values in the homicide rates were log-transformed using ($\ln[x + 1]$).

The Gini index is a measure of concentration of income across households and as such it can be calculated for a variety of geospatial information units. It is based on the cumulative frequency Lorenz curve, which compares the distribution of a variable, in this case household income, with the distribution under uniformity conditions. If income is equally distributed among households, the Lorenz's curve is a diagonal line. The magnitude of the Gini index varies from 0 (where the curve matches the diagonal) indicating zero inequality, to 1 (where all income is concentrated in a single household) indicating maximum inequality. The Gini index is the most frequently used measure of income inequality in criminology, perhaps due to its simplicity of calculation

and easy-to-understand graphical representation (Roberts & Willits, 2015). For our study, the Gini indexes at the municipal level were obtained from CONEVAL. Gini indexes were log-transformed for our analysis.

Control variables. As control variables we used the Marginality index, population density, and the spatial lag of homicide rates. The Marginality index is a Mexican statistical measure of socioeconomic development for geospatial information units such as states, municipalities, localities, and census tracts. It serves as a proxy measure for absolute deprivation. This index was created in 1993 by CONAPO. It is composed of the following nine census variables: Illiterate population of 15 years or more (%), population of 15 years or more without completed primary schooling (%), occupants in private dwellings without piped water (%), occupants in private homes without drainage or sanitary services (%), occupants in private homes without electricity (%), occupants in private homes with dirt floors (%), occupants in private homes with some level of overcrowding (%), population in localities with less than 5,000 inhabitants (%), and occupied population with a maximum income of two minimum wages (%). For the calculation of the Marginality index, a Principal Component Analysis was used to consolidate the previous variables into a single index. Index values are thus Z-standardized and vary from one census to another, where zero represents the national average at that point in time. The Marginality indexes at the municipal level were also obtained from CONEVAL.

Following Land et al. (1990) and McCall et al. (2010) population density was introduced as an alternative explanation to the homicide-income inequality link. Though they do not explain why population density may affect homicide, one somewhat obvious, negative externality of high population density is that it may propel more crime by creating more opportunities for crime. Population density was measured by the population per square kilometer log-transformed. However, unlike Land et al. (1990) and McCall et al. (2010), we did not consider population size as a control variable as the Marginality index already includes a measure of the percentage of people living in small urban and rural localities.

The spatial lag was calculated as the arithmetic mean of the homicide rate in neighboring municipalities, using a first-order spatial contiguity matrix under the queen criterion, that is, any geospatial unit with a common border or corner. The spatial lag represents a measure of cross-boundary dependence of homicide rates. It serves two roles: first, it controls for spatial autocorrelation in homicide rates, which if not controlled for, produces upward bias in OLS estimators and inflated significance statistics (Florax & Folmer, 1992); second, it can serve as evidence of spatial diffusion processes, where a significant and positive spatial lag may suggest that a greater number of violent neighbors

increases homicide rates locally and vice versa (Baller et al., 2001). In practice, it is difficult to determine which interpretation should be adopted. In any case, regression specifications should take into account the spatial patterning of homicide rates (Vilalta, 2013a).

Instrumental variable. Given our instrumental variable method approach, we used the spatial lag of income inequality as the instrumental variable. It was calculated in the same way as the spatial lag of homicide rates. The rationale behind this instrument is that if local income inequality were inducing someone to commit a murder, it would probably be irrelevant to them if the average of the Gini index among neighboring municipalities was zero or one. Income inequality in neighboring municipalities should not have a direct effect on homicide rates unless homicide offenders travelled from one municipality to another for income inequality related reasons. This would indicate the presence of spillover effects of income inequality on homicide rates. However, the spatial lag instrument is not a measure of spillover effects. Statistically, the instrument is a strong predictor of the endogenous relationship in the first stage (see Table A4 in the Appendix). Spatial autocorrelation was measured with Moran's Global coefficient using the same first-order spatial contiguity matrix under the queen criterion.

Empirical strategy. We began by estimating the following equation which assumes the relationship between homicide rates (HR) and income inequality (IN) to be linear:

$$HR_{it} = \beta_0 + \beta_1 IN_{it} + \beta_2 X_{it} + \beta_3 Z_i + u_{it} \quad (1)$$

Where the β_0 is the expected value of HR when all X s are equal to 0, X is a matrix of control variables, Beta coefficients are the ordinary least squares estimators, Z_i represents the time-invariant unobserved heterogeneities across municipalities, and u_{it} is the error term. By holding Z_i constant, we obtain the following model:

$$HR_{it} = \alpha_i + \beta_1 IN_{it} + \beta_2 X_{it} + u_{it} \quad (2)$$

Where intercept (α_i) is estimated for every municipality, capturing the fixed effects of observations. This fixed effects (FE) approach allowed us to control for omitted variables that are constant across time but not across municipalities.²

We then controlled for the effect of omitted variables that are constant across observations and over time. This was done by combining municipal fixed effects with time fixed effects:

$$HR_{it} = \beta_0 + \beta_1 IN_{it} + \beta_2 X_{it} + \gamma_2 D2_i + \dots + \gamma_n DT_i + \delta_2 B2_t + \dots + \delta_T BT_t + u_{it} \quad (3)$$

Where the D and B are $i-1$ and $t-1$ dummy variables for municipality (i) and time (t) respectively, and Gamma are the coefficients. This combined modelling approach allowed us to eliminate the effect of omitted variables that may change over time but are constant over municipalities, while also controlling for omitted variables that may differ across municipalities but are constant over time.

Specifications remove omitted variable bias by controlling for unobserved heterogeneity. However, this did not mean that the issues of concern were resolved. In order to do away with the correlation between income inequality and omitted predictors of homicide rates carried by the residuals, as well as the hypothetical simultaneity between homicide rates and income inequality and the effect of measurement errors on either homicide rates or income inequality, we estimated the model in equations (2) and (3) using the spatial lag of income inequality as an instrument for income inequality.

We completed our empirical strategy by comparing the FE estimates with first difference (FD) estimates. The first difference (FD) model is specified by:

$$HR_{it} - HR_{i(t-1)} = \beta_1 (IN_{it} - IN_{i(t-1)}) + \beta_2 (X_{it} - X_{i(t-1)}) + \gamma_2 D2_i + \dots + \gamma_n DT_i + \delta_2 B2_t + \dots + \delta_T BT_t + u_{it} \quad (4)$$

The comparison between FD and FE specifications allowed us to substantiate our results, as conflicting results between the two may be evidence of model misspecification. Differences between FE and FD specifications may be indicative of either misspecified dynamics or measurement error (Imai & Kim, 2021; Ziliak et al., 1999).

Empirical Results

Descriptive statistics. Homicide rates in Mexican municipalities increased and decreased over the years, reaching a high of 22.5 homicides per 100,000 in 2010. Differences also grew over time among municipalities as indicated by the coefficient of variation (see Table 1). It appears that the war on organized crime (WOC), declared in late 2006, was accompanied by a geographical unevenness unknown two decades before.

From the 1990s to the mid-2000s, homicide rates experienced a steady downward trend and were spatially clustered in a few rural areas and/or in the most populated cities on the border with the United States (Escalante, 2009). Following the declaration of the WOC, homicide rates increased and spread,

Table 1. Descriptive Statistics.

	Mean	Median	Std. dev.	Coef. var.	Min.	Max
Homicide rates*:						
1990	19.2	7.4	37.6	1.958	0.0	683.8
2000	12.6	5.0	25.1	1.992	0.0	391.7
2010	22.5	5.0	79.2	3.520	0.0	2,270.9
2015	17.4	7.2	36.8	2.115	0.0	702.0
Gini index						
1990	0.401	0.400	0.040	0.101	0.0	0.620
2000	0.461	0.460	0.072	0.157	0.0	0.710
2010	0.374	0.380	0.050	0.134	0.0	0.570
2015	0.361	0.360	0.060	0.168	0.0	0.880

*Rates $\times$ 100,000 inhabitants.

Table 2. Moran's I Spatial Univariate Autocorrelation Coefficients.

	Homicide rates (ln)	Gini index (ln)
1990	0.248***	0.254***
2000	0.152***	0.351***
2010	0.336***	0.374***
2015	0.241***	0.195***

* $p < .05$; ** $p < .01$; *** $p < .001$.

largely due to the military campaign against drug trafficking organizations (Beittel, 2015; Vilalta, 2014). Municipalities at the forefront of the WOC such as General Treviño (Nuevo León) and Tubutama (Sonora), experienced homicide rates well above 1,000 per 100,000 inhabitants (see Table A1 in the Appendix).

Income inequality, as measured by the Gini index, decreased somewhat (11%) during the period of study, although differences between municipalities became far more pronounced, as indicated by the coefficient of variation. The Marginality index is not presented in the table as it is Z-normalized with mean zero and one standard deviation.

The spatial clustering of homicide was especially high in 2010, when the WOC was at its peak (see Table 2). In 2015 homicide and income inequality remained spatially clustered, although the magnitude of their concentration varied significantly over time.

Spearman correlations show strong fluctuations in the statistical relationship between homicide rates and income inequality over time (see Table 3).

Table 3. Spearman Correlations of Homicide Rates (ln) and Variables of Interest by Year.

	1990	2000	2010	2015
Gini index (ln)	0.178***	0.044*	0.342***	0.043*
Marginalization index	0.020	0.023	-0.195***	-0.134***
Density (ln)	0.069***	0.061***	-0.023	0.056***

* $p < .05$; ** $p < .01$; *** $p < .001$.

The correlation between homicide and marginality was statistically insignificant before 2010. Furthermore, it was negatively associated, indicating that since then, homicide rates have been higher in more affluent municipalities. On the other hand, the correlation between homicide and population density was not statistically significant in 2010, when homicide rates were at their highest and were most spatially clustered. All this seems to indicate that the WOC was a defining point in the relationship between homicide and these variables of interest.

The inclusion of municipal fixed effects and time effects, which are designed to control for unobserved sources of variation not accounted for by these variables, offers radically different results from cross-sectional variation analyses. The reason for the differences between panel data analysis and cross-sectional analysis is that the latter cancels the local effects of municipalities when in fact they are relevant for explaining the link between homicide and income inequality.

Regression results. Table 4 below presents six different specifications for homicide rates. Specifications 1 to 3 show the first difference (FD) and fixed-effect (FE) two-way estimates of the homicide-income inequality link, controlling for marginality, population structure, and the spatial lag of homicide, while omitting the instrumental variable. Specifications 4 to 6 reflect the estimates using the instrumental variable (spatial lag of income inequality) method of estimation. While all specifications include municipal effects, only specifications 3 and 6 include the time effects. Simple linear and OLS regression estimates are presented in the Appendix (see Tables A2 and A3).

Starting with FD specifications (models 1 and 4), income inequality estimates are statistically significant and negative at the 1% level. The interaction term is statistically significant, suggesting that the effect of income inequality on homicide is conditional on levels of marginality (concentrated disadvantage). Similar to that noted by Burraston et al. (2018, 2019) in the case of U.S. counties, homicidal violence in Mexico is higher in areas with

Table 4. First-Difference and Fixed-Effect Estimates of Homicide Rates.

	Model (1) FD	Model (2) FE	Model (3) FE	Model (4) FD-IV	Model (5) FE-IV	Model (6) FE-IV
Gini index	−0.382*** (0.104)	−0.294*** (0.103)	−0.179 (0.139)	−0.621*** (0.091)	−0.566*** (0.132)	−0.910*** (0.280)
Marginality index	0.404*** (0.123)	0.513*** (0.119)	0.501*** (0.123)	0.467*** (0.082)	0.533*** (0.118)	0.581*** (0.122)
Gini × Marginality	0.449*** (0.100)	0.525*** (0.099)	0.501*** (0.103)	0.503*** (0.086)	0.552*** (0.100)	0.602*** (0.108)
Pop. density	−0.162 (0.104)	−0.099 (0.083)	−0.037 (0.097)	−0.189*** (0.011)	−0.169** (0.084)	−0.077 (0.090)
Spatial lag of DV	0.496*** (0.034)	0.570*** (0.034)	0.562*** (0.034)	0.485*** (0.018)	0.558*** (0.030)	0.547*** (0.031)
Intercept	−0.013 (0.015)			−0.025 (0.097)		
Municip. effects	Yes	Yes	Yes	Yes	Yes	Yes
Time effects		No	Yes		No	Yes
Hausman test	385.6***	175.6***	50.4***	92.2***	26.8***	60.9***
Pesaran CD test		3.167***	0.680		3.963***	−1.088
Durbin–Watson test	2.595	2.408	2.407	2.601	2.404	2.403
Two–ways <i>F</i> test			2.071***			2.029***
<i>R</i> ²	0.043	0.058	0.052	0.043	0.058	0.050
Obs.	7,300	9,757	9,757	7,098	9,487	9,487

****p* < .01, ***p* < 0.05, and **p* < .1.

DV = homicide rates (ln).

Heteroskedastic robust standard errors in parenthesis.

low levels of income inequality and high socioeconomic marginality. We return to this below.

Previous specifications exclude time effects, that is, they do not control for unobserved time sources of variation of homicide. However, when adding time effects (models 3 and 6), the FE specification (model 3) estimate of income inequality is not statistically significant, whereas the FE-instrumental specification (model 6) yields the largest estimate of the income inequality effect. According to these estimates, a one percentage point increase in the Gini index causes an approximately 0.9% decrease in homicide rates where the marginality index is equal to 0, that is, where marginality or concentrated disadvantage is the same as the national average. Thus, either estimates from specifications 1 to 5, though not OLS, are biased downwards towards 0, or estimates in specification 6 are biased towards greater negative values due to some unobserved time-effects bias. Most likely, estimates of models 1 to 5 are affected by the correlation between income inequality and other unobserved time factors, making the effect of income inequality on homicide rates appear

less than it really is. We are more inclined to the latter explanation given that the results of the two-ways fixed effects tests (models 3 and 6) suggest controlling for both municipal and time sources of unobserved heterogeneity.

The intercept in the FD specifications is the estimate of the time trend of homicide rates, or how much homicide rates changed from one period to another—in this case, negative but not statistically significant. Still, if time-effects affect such significant changes in the income inequality estimates, either time-effects have not mitigated the impact of omitted variables, or the impact of the shock created by the declaration of the WOC was not homogeneous across municipalities—which seems to be the case as indicated by the spatial autocorrelation coefficients. What is clear thus far is that our estimates, all negative, are consistent with what is reported by other studies using panel data (Brush, 2007; Messner et al., 2002; Neumayer, 2003).

Concerning the Marginality index, estimates are statistically significant and positive at the 1% level for all specifications. These effects are stable and subsist after the inclusion of time effects and time trends. According to estimates in model 6, a one percentage point increase in the Marginality index causes an approximately 0.6% increase in homicide rates where the Gini index is equal to zero, that is, where income inequality is nonexistent.

Population density estimates are either not statistically significant or negative, which contradicts findings in previous studies (Land et al., 1990; McCall et al., 2010). Concerning the spatial lag of homicide rates, neither the sign nor magnitudes of the estimates vary significantly across specifications. Significant spatial lags of the DV are endogenous by design, and so induce simultaneity bias (Franzese & Hays, 2009). Controlling for spatial effects typically shrinks coefficient estimates. However, it may also mean that homicides in one municipality affect homicides in neighboring municipalities, which has been found in a previous study in Mexico (Vilalta, Lopez-Ramirez, et al., 2021).

With regard to the performance of the instrumental approach, the linear hypothesis test shows the spatial lag of income inequality as a strong and relevant instrumental variable ($F=1,853$ and $p<0.001$). First stage regression results are provided in the Appendix. Thus, estimators seem consistent even under the heteroscedasticity of errors. We used robust heteroscedastic errors as the Breusch Pagan tests indicated that errors in the model were heteroscedastic—although the instrumental variable method was less heteroscedastic.³ Based on that, while Durbin Watson tests indicated no serial correlation in the residuals for any specification, only the results of Pesaran's test for model 6 showed no evidence of cross-sectional dependence in residuals—suggesting significance tests of the estimators are not biased. Hausman tests indicated that fixed effect estimators are more efficient and consistent

than random effects—in other words, unobserved municipal effects cannot be considered uncorrelated with independent variables.

The results from the OLS cross-sectional estimates are presented in Table A3 in the Appendix. They indicate that the relationship between homicide and income inequality is positive, that is, the exact opposite of panel FD and FE specifications. As such, while the results of panel specifications contradict the common finding of previous studies—that income inequality increases homicide—, OLS cross-sectional evidence supports it. However, based on previous research (Brush, 2007; Jencks & Leigh, 2006), we should not expect signs and magnitudes of the estimates to be similar. Differences are more likely due to a lack of robustness in the OLS estimates.

Discussion and Conclusion

We have provided panel data evidence that income inequality, as measured by the Gini Index, negatively affected homicide rates in Mexican municipalities in 1990, 2000, 2010, 2015. Likewise, we have provided pooled cross-sectional data evidence that the same measure of income inequality data positively affected homicide rates for the same period of study. As such, we have demonstrated that evidence regarding the homicide-income inequality link is dependent on method.

In this sense, we have also provided logical arguments and empirical evidence to support the claim that when discussing the homicide-income inequality argument using geographically aggregated data collected over time, panel data evidence is superior to cross-sectional evidence as the former allows the inclusion of time effects as well as the control of unobserved heterogeneity of municipalities. Inclusion of panel effects and statistical controls help alleviate methodological and model misspecification problems in this literature (Land et al., 1990; Pridemore & Trent, 2010). As such, in the discussion of whether income inequality increases or decreases homicidal violence, our main conclusion is that income inequality seems to have lessened homicidal violence between 1990 and 2015 across Mexican municipalities, based on the fact that our panel data evidence is more robust than the cross-sectional evidence.

The use of different panel data regression specifications all flipped the relationship between homicide and income inequality from positive to negative as compared to the pooled OLS cross-sectional approach. Panel specifications also weakened the previous relationship, with the exception of the FE instrumental variable with the municipal and time effects approach. Thus, it seems that not controlling for the sum of both municipal and time unobserved heterogeneity biases the estimates sufficiently enough to flip the sign of the relationship. In this sense, for researchers who may insist on testing the

homicide-income inequality argument with the use of cross-sectional data, our findings will be of concern. The obvious implication is that advances cannot be made without controlling for unobserved sources of variation (Brush, 2007; Messner et al., 2002; Neumayer, 2003; Pare & Felson, 2014).

The inclusion of a measure of absolute deprivation, that is the Marginality index, has been important in moderating the negative relationship between homicide and income inequality. Interestingly, while income inequality may have lessened homicidal violence across Mexican municipalities, greater marginality did not. These effects remained after controlling for municipal and time fixed effects as well as for alternative explanations to income inequality. Furthermore, the negative effect of income inequality on homicide was stronger with the use of a two-way linear fixed effects (FE) method, making the case more compelling for adjusting for municipality-specific and time-specific confounders when testing the homicide-income inequality argument. In addition, we have provided evidence of spatial dependence in the models, as measured by the spatial lag of the homicide rates. This spatial dependence may serve either as evidence of diffusion processes or as an artifact of unmeasured variables. The actual cause of this spatial dependence is left for future studies, however we speculate that among the possible reasons in the case of Mexico, are the contagious diffusion of behaviors and the movement of offenders and criminal groups across jurisdictions (Vilalta, Lopez-Ramirez, et al., 2021). For our objectives, it is enough to have proven that the homicide and income inequality relationship is spatially dependent, and hence spatiality must be controlled for given that, should spatial dependence be present on both sides of the equation, omitted variable bias magnifies and inflation of type I errors due to artificially small estimates of standard errors will occur.

It appears that it is necessary to revise the argument as to what actually causes homicide rates to increase as income inequality decreases. Previous studies have argued that income inequality triggers homicidal violence as the former creates social strain, anomic pressures, and/or social disorganization. Briefly put, income inequality boosts feelings of dispossession and grievance conducive to extreme violence (Rufrancos et al., 2013). However, it is possible that income inequality, at least in the case of Mexico, is not sufficient a grievance to create either strain, anomie, or collective inefficacy conducive to homicidal violence. We should keep in mind that recent spikes in homicide rates in Mexico were primarily due to the war on organized crime and that aggregates of organized crime activity do not correlate with economic deprivation, at least in the case of Mexico City (Vilalta, López-Ramírez, et al., 2021). At the national level, most of these homicides have been associated with organized crime by way of executions, gang confrontations, and aggressions against the authorities (Vilalta, 2013b, 2014). We have no way of measuring the actual effect of

organized crime on homicide as official data does not allow for these typologies to be explored (Heinle et al., 2017).

Among the limitations of this study is that only four time points of data were available to test the argument at the municipal level. As a case study, there is also the impossibility of generalizing our findings to other countries and times. While we confirm Burraston et al.'s (2018, 2019) finding that the effect of relative deprivation on homicide is contingent on absolute deprivation and vice-versa, this does not suggest that in the absence of one or the other, there will be no homicidal violence, nor does it imply that other places outside Mexico with more income equality in combination with high poverty levels will suffer from higher homicide rates. Today, Mexico is located in the upper tail of the world homicide distribution and is facing a war on organized crime. These contextual conditions impede the generalizability of our findings. However, Mexico as a case study demonstrates how and why the homicide-income inequality literature may be affected by a variety of empirical issues.

In sum, the continued lack of conclusive evidence with regards to the homicide-income inequality argument, is most likely due to the insufficient robustness of our statistical methods to withstand the range of empirical issues associated with the argument. In this study, we have provided reasons and evidence that suggest that the prospects for a solution to the argument will most likely depend on how future studies with more robust methods address the empirical issues that lie at its center.

Appendix

Table A1. Top 10 Municipalities by Homicide Rate.

Municipality	State	Homicide rate*	Year
General Treviño	Nuevo León	2,270.9	2010
Tubutama	Sonora	1,613.8	2010
Doctor Coss	Nuevo León	932.4	2010
Praxedis G. Guerrero	Chihuahua	854.3	2010
Paras	Nuevo León	773.7	2010
Guadalupe	Chihuahua	758.7	2010
Mier	Tamaulipas	735.0	2010
Saric	Sonora	702.0	2015
San Andrés Tepetlapa	Oaxaca	683.8	1990
Melchor Ocampo	Nuevo León	580.0	2010

*Per 100,000.

Table A2. OLS, FD, and FE Estimates of Homicide Rates in the Simplest Specifications.

	Model (1) Pooled OLS	Model (2) FD	Model (3) FE
Gini index	0.927*** (0.125)	-0.489*** (0.141)	-0.481*** (0.097)
Intercept	2.591*** (0.117)	-0.022	
R ²	0.009	0.003	0.003
Obs.	9,757	7,300	9,757

*** $p < .01$, ** $p < .05$, and * $p < .1$.

DV = homicide rates (ln).

Heteroskedastic robust standard errors in parenthesis.

Table A3. OLS Estimates of Homicide Rates.

	Model (1) Pooled	Model (2) 1990	Model (3) 2000	Model (4) 2010	Model (5) 2015
Gini index	0.461*** (0.094)	2.637*** (0.494)	0.455** (0.210)	2.787*** (0.281)	0.281 (0.218)
Marginality index	0.080 (0.086)	-0.341 (0.300)	-0.206 (0.151)	-0.430 (0.264)	0.216 (0.189)
Gini $\times$ Marginality	0.185** (0.091)	-0.470 (0.319)	-0.270 (0.182)	-0.362 (0.265)	0.352* (0.186)
Pop. density	0.069*** (0.010)	0.090*** (0.021)	0.101*** (0.018)	-0.029 (0.022)	0.064*** (0.019)
Spatial lag of DV	0.727*** (0.015)	0.676*** (0.030)	0.650*** (0.031)	0.694*** (0.031)	0.683*** (0.029)
Intercept	1.018*** (0.100)	3.012*** (0.483)	0.814*** (0.173)	3.736*** (0.330)	0.982*** (0.251)
AIC	34,719.6	8,683.7	8,512.4	8,659.7	8,687.3
R ²	0.198	0.213	0.148	0.283	0.186
Obs.	9,757	2,403	2,443	2,454	2,457

*** $p < .01$, ** $p < .05$, and * $p < .1$.

DV = homicide rates (ln).

Heteroskedastic robust standard errors in parenthesis.

Table A4. First-Stage Estimates of Homicide Rates.

	Model (1)
Marginality index	-0.014*** (0.003)
Gini $\times$ Marginality	-0.014*** (0.003)
Pop. density	-0.0004 (0.0003)

(continued)

Table A4. (continued)

	Model (1)
Spatial lag of DV	0.001** (0.0004)
Spatial lag of IV	0.193*** (0.005)
Intercept	0.512*** (0.004)
Linear hypothesis test	1,803***
R^2	0.294
Obs.	9,487

*** $p < .01$, ** $p < .05$, and * $p < .1$.

DV = Gini index (ln).

Heteroskedastic robust standard errors in parenthesis.

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Notes

1. Due to space limitations, whether the correlation between homicide and income inequality is constant across the homicide distribution will need to be addressed in another study.
2. Fixed estimators are also called within-estimators as they use time variation within each observation.
3. Where BP=533.3, $p < .001$, BP=568.9, $p < .001$, are the studentized results of models 1 to 3 and 4 to 6 respectively.

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